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FURTHER APPLICATIONS OF MULTIPLE-BEAM INTERFEROMETRY

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Introduction

Since Fizeau in 1862 introduced his celebrated optical interference method, the use of light waves for the study of the perfection of finish of surfaces has had considerable application. For long the technique was principally applied to glass surfaces, but within recent years it has been widely extended to include metals, and in fact to-day Fizeau's method is in daily use in numerous precision tool and optical workshops, both as a means for accurately gauging lengths and also for examining the perfection of surface finish.

The obvious reason for the popularity of the use of interference is, of course, the smallness of the measuring unit, the light-wave, which in the green region is some 5000 A.U. (*i.e.* some 1/50,000th inch). Now convenient micrometer methods of measurement can be pushed to about 1/10,000th of an inch; as this is five light waves, it is evident that interferometric methods take over just where micrometer methods begin to fail, and thus assure continuity in measurement.

With classical interference methods, it is just possible to measure down to 1/20th of a wave, *i.e.* one-millionth of an inch. It is the object of this report to describe some recent investigations using the modified multiple-beam interference methods which permit the detection of level changes of only 1/1000th part of a wave. In order to clarify the points involved, a brief survey of the basic principles of the Fizeau method of surface interferometry will now be given.

Two-beam Interference

When a parallel beam of white light falls upon a very thin transparent film, such as an air film between glass surfaces, a soap bubble

Now, owing to the sinusoidal nature of light vibrations, the fringe edges are not sharply delineated, but they merge into one another according to the intensity distribution shown in Fig. 2A. This intensity distribution sets a limit to the accuracy, and as a rule there is an uncertainty in setting on a fringe of the order of 1/10th of a band width, as indicated by the hatched region. This quantity ($\lambda/20 \approx 250$ angstrom units $\approx 1/1{,}000{,}000$th inch) is the usual limit of accuracy attainable, and within this limitation the technique is of very great value, for local errors on the surface can be recognised as kinks or fringe displacements to within this order of magnitude.

Multiple-beam Interference

In the classical interference method, *two light beams* only are used, both having the same intensity, but in the various techniques developed by the writer use is made of *multiple-beam interference*, in which as many as perhaps 100 effective beams contribute to *each* fringe. By vacuum evaporation, it is possible to deposit on a surface a thin layer of silver, perhaps 100-atom layers thick, which will reflect over 90 per cent. of incident visible light and transmit 5 per cent., the remainder being absorbed. When two such surfaces are brought very closely together (no further apart than 1/100th mm., preferably nearer) and correctly illuminated with monochromatic light, a profound change in the shape of the fringes takes place, primarily because of the high reflectivity.

The beams of light are reflected to and fro between the silvered surfaces and on recombination form fringes which have an intensity distribution of a highly sharpened character, as illustrated by Fig. 2B. As an example taken from practice, Plate I(a) shows the fringes given by a curved mica surface. The fringe sharpness is clearly rendered, since it will be recognised that the width is but a small fraction of the distance between a pair of fringes. Since this fringe separation corresponds to $\lambda/2 = (2500$ A.U.), and as the fringe sharpness is such that a local variation of 1/250th of the fringe separation is measurable, it is clear that on such a photograph surface detail corresponding only to a 10 A.U. alteration in height is detectable. This quantity is of molecular dimensions, and one sees that the multiple beam technique has considerably extended the range of observation.

So sensitive is the procedure that indeed, in favourable cases, one is able to measure surface height changes corresponding to only a single molecular layer, and by this means it has actually been found possible to measure the crystal lattice spacing of some crystals

with ordinary visible light. An obvious development is to apply the fringes to the study of surface quality, *i.e.* to surface finish and polish, and in this connection studies of metal surfaces are of paramount importance. But the technique as described above requires *transmission* of light through the film and is clearly not directly applicable to opaque bodies. Fortunately, by slight modifications, it is possible to obtain multiple-beam fringes in *reflection*, wherein one obtains a system of fine, narrow, *dark* fringes on a bright background (as distinct from the transmission fringes, which have fine, narrow, *bright* fringes on a dark background). The transmitted and reflected systems are almost, but not quite, complementary. As an example of the precision reflection fringes obtainable, Plate I(*b*) shows those given by the face of a quartz crystal (100 face).

In this report, a brief account will be given of two current researches now in progress at Royal Holloway College, using respectively transmission fringes and reflection fringes. These form but one section of some comprehensive investigations now being pursued. The transmission fringe studies to be described are concerned with oscillating quartz crystals, whilst the reflection studies refer to metal surfaces with varying degrees of surface polish and surface treatment.

Interferometric Study of Quartz Oscillators

An investigation of considerable promise now being pursued involves the successful application of multiple-beam interference methods to the study of both the surface and internal bodily vibrations of rapidly oscillating quartz crystals. When an alternating potential is applied to opposite faces of a suitably cut quartz crystal, the latter is set in mechanical oscillation, a matter of considerable practical importance, for such crystals can be and are used for strictly controlling the frequency of radio and radar transmitters, and for use as accurate, very high precision astronomical clocks. Typical crystals may oscillate within a frequency range extending from 100,000 to 5,000,000 times per second, and it would clearly be of some considerable interest if a method for examining the mode of such oscillations could be derived. Such a method has in fact been developed, using interferometry, and will now be described.

The first experiments were originally undertaken with some hesitation and diffidence. It is simply axiomatic in multiple-beam interferometry that shocks and vibrations must be avoided, for the technique is highly sensitive, and when operating at its best movements even of only 10 A.U. (which is less than many crystal lattice spacings) can impair definition. Under the circumstances,

it might have appeared foolhardy to attempt to make precision interference observations on a crystal vibrating at, say, 1,000,000 times a second, and with perhaps considerable amplitude, too. Yet, as the event showed, it is possible to obtain valuable interference pictures by the most simple of means. It was recognised at the outset that it was not only necessary to obey the usual optical conditions imposed by multiple-beam interference, but in addition to restrict the amplitude of oscillation, for reasons which will soon appear obvious; in fact, as will be seen, this latter restriction has much to do with the success of the experiment.

Two types of investigation are now in progress. One concerns the observation made on *one surface* of an oscillating crystal, the other refers to vibrations *within* the body of the crystal.

SURFACE VIBRATIONS

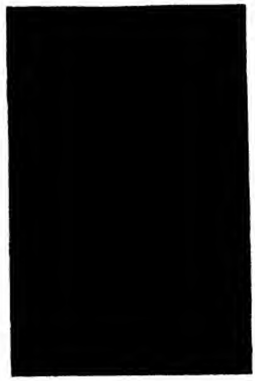
The surface studies are at the moment the more spectacular and the simpler to carry out and to interpret. The crystal is polished to a higher degree than that usually adopted in commercial oscillators, in order to give fringes with good definition. The face to be examined is coated by evaporation with the usual type of high reflecting silvering now adopted for multiple-beam interferometry. The opposite and parallel face of the crystal is *lightly* silvered. This latter silvered surface does not take part in the interference observation, the silvering performing merely as an electrode. The highly reflecting face is placed resting lightly on a similarly silvered flat. Electrical connection is made by gentle spring contact with the silvered flat and by gentle spring contact with the lightly silvered *rear* face of the crystal. A variety of contacts has been tried, *i.e.* single point, broad strip, wide mesh, wire grid, etc. They are all successful. The connecting leads are taken to a controlled oscillator circuit. Thus, under such conditions, the crystal suffers no serious constraints. Other experiments using clamped crystals have also been successful in giving excellent vibration photographs.

When the crystal is at rest, illumination with parallel light at normal incidence (green mercury line) forms *sharp line* multiple-beam fringes in the air film between the surface of the crystal and the optical flat. The particular fringe *shape* seen is determined by the optically worked figure of the crystal surface and the fringe *dispersion* by the angle between the flat and the crystal, this being under control.

When the crystal is set in rapid oscillation, then the originally sharp fringes take on a striking appearance, a typical example being

PLATE

(a)

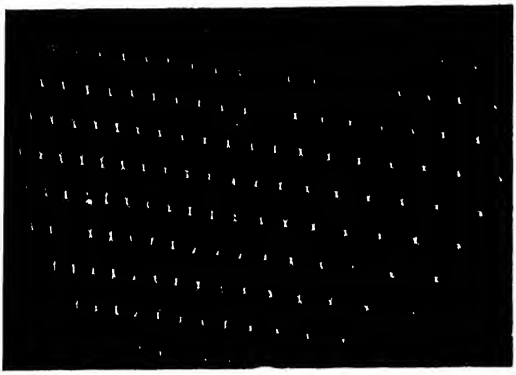


(b)

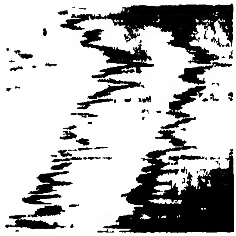
(c)

(d)

(e)



(f)

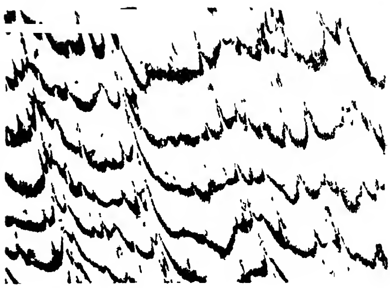


(j)

(g)

(h)

(i)



(k)

that shown in Plate I(*c*). At once the whole distribution of oscillation of the crystal surface can be recognised, and accurate measurements of amplitudes and displacements can be made. One sees clearly marked nodal regions when the crystal surface is effectively at rest, whilst elsewhere the fringes broaden so that the fringe width is an exact measure of the local amplitude of vibration. It is possible to excite a crystal into many distinct modes of vibration, and each mode leads to a quite distinctive interference pattern. Plate I(*d*), (*e*) and Plate II(*f*) illustrate some of the interesting complex patterns which appear when distinctive specific overtones are excited.

Although the investigations are but young, already interesting information has been obtained. Thus the stability of the interference picture is a matter for comment. In some nodal regions the fringes are so sharp that the local amplitude must be less than $\lambda/100$, *i.e.* less than 50 A.U., and indeed it is possible that there is no effective vibration at all at these positions, to within the amplitude of a few crystal lattice spacings at any rate. Of course, when there are only a few fringes in the field of view, the exact nodal regions may be missed in a particular exposure, but then picked up if the fringes are moved to another region. Plate II(*f*) shows in a most surprising way how the crystal has subdivided itself into vibrations giving higher harmonics. Many of the interferograms exhibit local irregularities, doubtless due to crystal flaws or perhaps partial local twinning. Most important of all, it has been established (as was to be expected) that there is a close correlation between the interference picture and the electric radiation harmonics given out by the valve circuit the oscillations of which the crystal is controlling.

All these aspects are under exploration, and it is clear that a big field lies ahead. The effect of twinning, of air damping, of clamping, the dependence on crystal shape, etc., are all obvious subjects for further detailed study, and many more come immediately to mind.

It is to be emphasised that no stroboscopic approach has yet been used, although its employ is contemplated. For it should be possible to excite a mercury discharge with the same frequency, indeed the same circuit, and, by using this discharge as a light source for the fringes, a perfect stroboscopic investigation should become practicable.

It will be recognised that the immediate success of the experiments just described depends upon restricting the maximum amplitude of vibration to, at most, a little more than half a light wave, *i.e.* the separation between fringes. For, if the amplitude exceeded this by a great deal, the fringes would then run into each other and

only the nodal regions would remain visible. With stroboscopic studies one could, however, increase the vibration amplitude to much higher values. The matter is under investigation.

Body Vibrations

In the experiments described above the observations have been concerned with the examination of the *surface* movements only, but, since the crystal vibrates as a whole, it is of equal if not of more interest to examine vibrations *within the body* of the crystal. Some preliminary observations on this have already been made using a modified technique, and they have shown the anticipated effects. Inherently, because of the restrictions imposed by the conditions of observation, the definition of the fringes, although good, is inferior to that in the former experiments.

A thin plate crystal, 1 mm. thick, was optically worked on both sides to approximate plano parallelism. One face was then deliberately worked into a slight spherical curvature. Both faces were silvered with high reflecting layers and the crystal was then illuminated with a parallel beam of monochromatic light, using quite a small pinhole source at the focus of a good lens to make the incident beam parallel.

The particular crystal was mounted clamped between brass plates, as is often common in practice for the particular mode of excitation involved. The fringes which form *within* the crystal (see Plate II(*g*)) are fairly sharp roughly circular double rings. (The doubling arises from the double refraction property of quartz, and causes little inconvenience.) Because the fringes are formed with a relatively large optical gap (the faces are 1 mm. apart), this leads to experimental complications which markedly impair fringe definition and also lead to the formation of weak satellite rings, whose origin is known and which can moreover be quite disregarded in these studies. It is necessary to employ special light sources in these experiments. On setting the crystal into oscillation the fringes broaden out locally, showing thus where the crystal thickness changes during the oscillation. The effect is shown clearly by Plate II(*g*) and (*h*). In Plate II(*g*) the reasonably sharp, approximately circular fringes given by the crystal at rest are shown, whilst Plate II(*h*) shows the change in appearance when the crystal is set in oscillation.

Although these observations have only just been begun, it is clear that the interferometric method for studying oscillating crystals has much to offer, and an ambitious programme of investigation is being actively pursued with the assistance of Mr. Bardsley.

Interferometric Studies on Metal Surfaces

The study of opaque surfaces (*e.g.* metals) by multiple-beam interferometry leads to a number of experimental difficulties which do not exist in the case of transmission fringes. For example, it is necessary to use monochromatic light. Again, for reflection studies at higher magnifications, one must perforce employ a metallurgical type of microscope with some form of internal reflector system between the microscope objective and eyepiece lenses. Now, in such arrangement, it is necessary to make the microscope objective attempt two functions simultaneously, *i.e.* to throw a *parallel* beam of light on the object under survey, and at the same time to image this object as in normal microscopy. It has been found that existing microscope objectives cannot satisfy this double function, hence a compromise must be adopted. With a careful technique it has, however, been found possible to obtain successful multiple-beam interference pictures, with useful magnifications up to × 750. (It is possible to go up to some × 2000 with two-beam interference, but, of course, such fringes are far inferior to the multiple-beam fringes from the point of view of precision and application.)

The fact that × 750 can be used indicates that the interference technique should have considerable metallurgical, and indeed micrometallographical applications. Obvious fields of study include, for example, surface finish, polish, indentations, hardness tests, surface flow, crystalline structure, slip bands, etc. It has been found necessary to re-examine in some detail the theory of the interference process in reflection, and this has shown that the critical factor affecting performance is the absorption of the silver film on the optical flat placed close to the metal surface under study.

Two types of fringes have been adapted to the examination of metals. The one uses monochromatic light, the other white light, and to illustrate the kind of result being obtained we shall consider a few examples of as yet unpublished observations, most of which have been made in this laboratory by Mr. J. Holden.

The monochromatic fringes given by a highly polished steel surface (slip gauge) are shown at × 80 in Plate II(*i*). The fringes are relatively very sharp, but do show indication of structure. It can, however, be shown both theoretically and experimentally that the white light fringes which the writer has developed (fringes of equal chromatic order) are better adapted to revealing the detail of surface structure, and such white light fringes at magnification × 480 are shown for a similar surface in Plate II(*j*). It is a

characteristic of these fringes that each resembles its neighbour and any one is the contour profile of a selected linear region on the surface under examination. The type of secondary structure which is only barely noticeable in Plate II(*i*) is rendered very clearly in Plate II(*j*) in the form of the ragged fringe shape.

It is at once evident that both fringe types reveal much information regarding the state of surface finish. For example, the very different picture ($\times$ 66) shown in Plate II(*k*) was given by a sheet of rolled steel, and the surface character is very clearly delineated.

Another investigation, now to be described, shows in a characteristic manner how the interferometric method can be adapted to a wide variety of problems, such as, for example, the study of etching, or of film porosity, or of corrosion, etc. It is well known that repeated electric discharges on to a metal electrode wear away the surface, and the manner in which this takes place is now under interferometric examination. Typical results are shown in Plates II(*l*) and (*m*). A small disc of tungsten was highly polished and from this a small number of separate electric discharge sparks were drawn. It was found that each spark produces a crater or pit, often of spherical cap-shape, such a pit being illustrated in Plate II(*l*) (magnification $\times$ 100).

The depression in Plate II(*l*) is about 2 light waves deep, and a calculation shows that the amount of metal removed is about 4×10^{-9} c.c. Much smaller craters could have been measured with volumes even below 10^{-12} c.c. The second crater is of similar dimensions but one immediately notices a marked difference in the *quality* of the surface in the two cases. In Plate II(*m*) the fringes for another crater are narrow and reasonably smooth, whilst in Plate II(*l*) they are broken and ragged. Plate II(*m*) shows a pit at $\times$ 500.

The descriptions of results already given show that metallographic applications abound, and indeed some such fields are already under exploration. Thus, the indentations produced by hardness testing are under examination, the slip bands which show up when crystals are stressed are being studied, and metallic polish phenomena are being investigated.

A special replica technique has also been developed for studying metal surfaces. In this process a replica of the surface is taken on a stout piece of transparent plastic material. This is then silvered and the replica surface examined in *transmission*. By this means the inherent difficulties associated with reflection fringes are avoided and in addition other specific advantages accrue.

Space does not permit a description of the experiments now

being conducted on crystal surfaces. In these investigations, it is necessary to employ the techniques of both reflection and transmission, since crystals may often have one excellent surface but be internally crazed, or have a bad opposing surface, or indeed no suitable face of entry, either condition preventing effective employ of transmission fringes.

For a comprehensive account of the theory and practice of multiple-beam interferometry the reader is referred to the following recent book: *Multiple-Beam Interferometry of Surfaces and Films*, by S. Tolansky, Clarendon Press, 1948, which includes a bibliography.

THE CONTROL OF DISEASES OF LIVESTOCK

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LIVESTOCK of all species suffer from disease, the incidence of which may have an important bearing upon farming economy. During the past 25 years much progress has been made in an understanding of the causes of some of the important diseases and methods of control have been worked out and are now in practice. Veterinary research work has increased and the results, as well as the findings of workers in other branches of science, have resulted in the devising of methods of control which are applicable on a large scale under field conditions.

The methods used vary considerably and depend largely upon the nature of the infecting agent.

There exist in livestock diseases of a highly infectious character, some caused by bacteria or viruses, others by ecto-parasites, which lend themselves to legislative control. They are dealt with by the Ministry of Agriculture under the Diseases of Animals Acts and Orders, which require owners to report the presence or suspicion of their existence to the responsible authorities and which provide for the slaughter of infected and in-contact animals, prohibition of movement of animals in prescribed areas except under licence and, in some diseases, defined treatment of infected animals. Examples of such diseases are foot and mouth disease, anthrax, swine-fever, sheep-scab, parasitic mange in horses and certain forms of bovine tuberculosis. By the application of the various Acts and Orders the country has been freed from and remains free of infections such as cattle plague (rinderpest) and bovine pleuro-pneumonia which still cause severe economic loss in various parts of the world.

Infectious diseases which do not lend themselves to legislative control, as well as some few in that category, are largely controlled by the induction of resistance or immunity, which is now being practised on an ever-increasing scale. Methods of immunisation are

worked out in the laboratory, following the ascertaining of the causal agent : field application is the ultimate object.

In this article some of the diseases for which satisfactory immunisation methods have been elaborated will be considered.

Typical examples are some of the diseases in sheep. Sheep are highly susceptible to infection with the anaerobic group of bacteria, and in some districts infections with members of the clostridia group of organisms cause many deaths. Several important clostridial diseases have been the subject of much research work, the result of which has been the devising of vaccines and serums for their control.

Clostridium welchii was found to be the cause of the disease "lamb dysentery." At first it was considered that the organism was the type largely responsible for gas gangrene in human beings. It was soon shown however that, although the *Cl. welchii* of lamb dysentery possessed many of the qualities and characteristics of the group, it differed in that the toxin produced by it in artificial media did not correspond entirely to that produced by other members of the group. From the study of the toxins it was shown that there were at least four types of *Cl. welchii* and that each might produce several toxins. Whereas the type associated with human gas gangrene (the A type) produced only one toxin, other types produced a number of toxins; some in abundance, others in small quantity only. Recent research work has shown that there may still be further types of toxin, some hitherto unrecognised, produced by the members of the group. The *Cl. welchii* group is divided into four types, A, B, C and D, each of which is characterised by its toxin production. The type associated with the disease, lamb dysentery, is B and at least four, and probably more, toxins are produced by it. It is essential in considering immunity to this disease to ensure that antigens used in the production of active immunisation cover each toxin and that, for passive immunisation, the antisera contain the respective antitoxins. It was from a study of the *Cl. welchii* isolated from lambs affected with lamb dysentery that much of our present-day knowledge of *Cl. welchii* was obtained and further research work stimulated.

Two methods are in operation for the control of lamb dysentery, *viz.* the passive immunisation of lambs by the injection of antitoxins and the active immunisation of the mothers (ewes) with vaccine before and during pregnancy. Because the infection takes place very early in life it is necessary to inject the antitoxin within 12 hours of the lamb's birth, and the amount must be sufficient to neutralise the highly potent toxin elaborated in the intestine of the infected

animal. On farms where lambing is carried out in enclosures and shepherds are in constant attendance during the lambing period, passive immunisation is the method of control largely adopted. Active immunisation of ewes is practised on hill farms where it is impossible to handle all the lambs at such an early age. The ewes are treated with vaccine which is prepared by the action of formalin on the rapidly growing culture of *Cl. welchii*, type B, in an artificial culture medium. Various tests are carried out in the laboratory to ensure the potency and safety of the vaccine: they vary from tests for sterility to tests for antigenic value. It is usual to inject the vaccine into ewes soon before pregnancy and again soon before lambing: it has been shown, however, that, following the establishing of immunity in young ewes by two inoculations of vaccine, one dose injected in subsequent years near the time of lambing is a sufficient stimulus to raise the immunity to a desirable level.

The lambs born from ewes so immunised are themselves still susceptible to the infection at birth. They derive their immunity from the colostrum (first milk) from their immune mothers. During pregnancy when colostrum is forming in the mammary gland a transference of antibodies from the tissues of the mother takes place and at parturition there is a heavy concentration of antibodies in the colostrum. As colostrum gradually gives place to milk the concentration of antibodies becomes less, until eventually they are difficult to demonstrate. Much study has recently been made of the qualities of colostrum in ruminants, and experimental work is being carried out on an increasing scale. Its value to the newly-born animal is well recognised: its antibody content is of much importance. Experiments have shown that the antibodies in the colostrum are absorbed through the intestine of the young lamb for about 4 days after birth: subsequently no further absorption takes place. From these observations the immunisation of ewes in the control of lamb dysentery was evolved and is largely practised on hill farms in all parts of the country. The lamb receives a passive immunity from the colostrum, but its duration is long enough to ensure protection against the infection during the susceptible period of its life.

Another disease of lambs associated with infection of *Cl. welchii* is pulpy kidney disease, in which *Cl. welchii*, type D is the causal agent. Here, the infection apparently takes place later in life than in lamb dysentery, for it is unusual to find affected lambs under a few weeks of age. The toxins of type D are somewhat similar to those of type B, but those of most importance in the causation of the two diseases vary in their amounts. Passive immunisation with

antitoxins is recognised as a method of control. Some success has attended the active immunisation of the ewes, but the results depend on the age at which the lambs become infected and on whether a sufficient supply of antibodies has been available during the period when colostrum was present, as well as on the antibody content of the milk secreted after the colostrum.

A third anaerobic infection of importance in sheep is the disease "braxy," caused by *Cl. septique*. This organism also produces a potent toxin. The disease affects sheep usually towards the end of their first year of life and is associated with frosty weather conditions. It is highly probable that many sheep acquire a natural immunity to the infection early in life : this is shown by the observation that many sheep kept on infected pastures do not develop the disease, whilst sheep introduced to such pastures later in life acquire the disease. Braxy was the first of the anaerobic infections of sheep to be studied. It was only after concentrated research work that *Cl. septique* was found in pure culture in the wall of the stomach and that the true cause was recognised. The first agent to be used in the immunisation against braxy was a diluted *Cl. septique* toxin. More recently, formalinised cultures have been introduced. This method of active immunisation produces good results in the field.

The most recently discovered anaerobic infection in sheep in this country concerns *Cl. œdematiens*, the cause of "black disease" which has been recognised in Australia for a number of years. While in the three diseases already described the infectious agents are found in the alimentary tract, where they elaborate their toxins which are then absorbed into the circulation and so reach all parts of the body, *Cl. œdematiens* is found largely in the liver. It would appear that *Cl. œdematiens* may do little damage, even if present in the resistant spore stage, in the healthy liver or other tissues : it is only when damage is done to the healthy tissue that the organism invades, multiplies and produces toxin. Sheep become infected with liver fluke (*Distomum hepatica*), which invade the liver. It appears that, following this invasion, any *Cl. œdematiens* present in their turn invade the damaged tissue and produce "black disease" through the action of their toxins. Experimental work has not yet progressed far enough to show definitely whether it is possible to control this infection by immunological methods. Control of the liver fluke by destruction of its intermediate snail host (*Limnæa truncatula*) should assist in reducing the incidence of black disease.

Sheep also become infected, largely through wounds in the skin, with the anaerobe, *Cl. chauvœi* : the disease set up is "blackleg," so called because of the dark appearance of the infected muscle. It

is found in some parts of the country more than others. Active immunisation is practised in these areas, the vaccine being again a formalinised culture of the causal organism. In some districts, in which the infection is apparently highly virulent, passive immunisation with antiserum is adopted. It is difficult to demonstrate that *Cl. chauvœi* produces any toxin. Artificial infection can be set up only if the organisms themselves are used, and only if they are activated with a substance such as calcium chloride. Cattle also suffer from blackleg caused by *Cl. chauvœi.*

Tetanus, caused by *Cl. tetani*, is a disease of all species of livestock. The organism usually gains entrance to the body through the medium of wounds : it remains *in situ*, multiplies in the damaged tissue and produces an extremely potent toxin which finds its way to the central nervous system. Passive immunisation with antitoxin is practised in many animals following wounding or operation, with satisfactory results. The treatment of the infection subsequent to the appearance of symptoms of the disease is of very doubtful value. Active immunisation with toxoid (formalinised toxin), to which alum is added, is a successful method of prophylaxis and is often carried out in studs of horses, army horses, horses used in coal mines, etc.

In cattle, a disease of much economic importance is " brucellosis " —infection with *Brucella abortus*. The condition is popularly termed " contagious abortion " from the fact that one symptom is the abortion of the developing fœtus, usually at about the seventh month. The term " contagious abortion " is misleading, for *Br. abortus* infection may be present in many animals, pregnant and non-pregnant, and abortion is by no means the general finding in infected pregnant cows : in addition, the infection is found in bulls. In this country we are fortunate in that only one type of *Brucella* occurs, *viz. Br. abortus*. In other countries *Br. melitensis* and *Br. suis* are common, infecting mainly goats, sheep and pigs, but also on occasion cattle.

Control of brucellosis can be accomplished in two ways : (1) The diagnosis of infected cattle by the demonstration of agglutinins in their blood serum, their removal from contact with non-infected animals, the taking of precautions to prevent further introduction of infection to the healthy animals, and the periodic examination of the healthy cattle to ensure their freedom from further infection ; (2) by active immunisation. In the former method use is made of the agglutination test, carried out in the laboratory. Recent research work has shown the best types of antigen to use, the techniques yielding satisfactory results and the correct interpretation of the results. Agglutination testing is practised extensively in this

country on animals for export, for some countries prohibit the import of animals reacting positively to the test.

While many herds have been freed from brucellosis by the first of the above methods, much disappointment has been experienced in this method of control. Until recently, the incidence of the infection throughout the country was so high that it was only a matter of time before a herd cleared of infection again became diseased. Transmission takes place readily and the chances of introducing infection into a herd are many.

Active immunisation is now being widely undertaken: vaccine is used for this purpose. Immunisation against brucellosis has been in operation for many years, but within very recent times a new type of vaccine has been used. To ensure a satisfactory degree of immunity it is necessary for the vaccine to be composed of living cultures. The earlier work was carried out with virulent cultures and vaccination was confined to young non-pregnant animals in already-infected herds. There is no doubt that this treatment was followed by some success, but its application was much limited. A few years ago it was shown that good results followed the use of strains of *Br. abortus*, so modified that they were no longer virulent for cattle and therefore did not set up infection following their injection. A strain of this type is 19, produced by American workers, (popularly referred to as S.19): this strain has been the subject of much study and there is now ample evidence that it is quite non-virulent for cattle and is highly antigenic. Experiments have been carried out in cattle in this country: the results show clearly the high immunising value and the safety of the vaccine. Other strains have also been the subject of examination in cattle, but have proved to be unstable in that they again became virulent when injected. This reversion to the virulent form was shown by their becoming located in the udder, from which fully virulent *Brucella* were recovered.

Female cattle of any age respond well to S.19 vaccine, and there is evidence from other countries that a single injection of the vaccine, in a satisfactory dose and prepared by the method laid down, gives rise to an immunity of long duration, probably for several years.

Vaccination is followed by the production of agglutinins. They tend to disappear in time, however, without any apparent decrease in the state of immunity. It appears that the younger the age at which the vaccine is injected, the sooner do the agglutinins disappear: when it is desired to have immunised animals free from agglutinins, *i.e.* non-reactors to the agglutination test, vaccination is done at 4–8 months of age. Animals injected before 4 months of

age often fail to respond. Vaccination at this young age is referred to as "calfhood vaccination."

Experiments have been made to examine whether *Br. abortus*, strain 19, undergoes change in the animal body. They consisted in passaging the strain through a series of seven pregnant cattle. Large doses of the culture, injected intravenously, cause abortion of the fœtus: the strain was recovered from each of the injected animals and passed through the next in series. Examination of the strain at the end of the experiment showed that no change had taken place: it had maintained all its characteristics, including its low virulence.

The production of S.19 vaccine is attended with some difficulties, for there is a tendency for the colonies of the organism to become "rough," and it is the "smooth" colonies only which are found antigenic. Further, as the vaccine is a living culture, every operation in its preparation has to be made in such a way that the introduction of any extraneous contaminant, *e.g.* from the air, is avoided, no antiseptic or preservative being permissible.

Although bulls become infected with brucellosis, it is not found advisable to attempt to immunise them with this vaccine. There are two reasons for this decision: one is that there is some evidence that the strain may become located in the genital tract of the bull and may there assume a virulent character; the other, that the strain stimulates the formation of agglutinins, as does natural infection. In the bull it is often difficult to diagnose natural infection by clinical methods in its early stages and the finding of agglutinins may give a misleading interpretation to the clinician in making a diagnosis.

Research work has been carried out with killed cultures of *Br. abortus*. It has been shown that an immunity of high order follows the intramuscular injection of the suspension of a large number of the virulent organisms, killed by formalin and suspended in lanolin and liquid paraffin. Tests carried out in goats and in cattle have given satisfactory results, although the resulting immunity was not so high as that following the use of S.19 vaccine. It is doubtful whether a killed culture so applied can be made an economic method of immunisation, because of the very large number of organisms in each dose necessary for the production of satisfactory results.

Further experiments have been made with phenol and heat extracts of virulent cultures of *Br. abortus*. Good immunity can be produced but, here again, economic considerations have to be taken into account. A large number of organisms is required for each dose of the extract. It may be that with further study a method of

extraction can be found whereby the organisms can be reduced to a satisfactory number.

Attempts have been made to set up a resistance to bovine tuberculosis. It has long been recognised that living cultures are necessary for this purpose. In this country two cultures have been used—a modified strain of *M. tuberculosis* of bovine origin and a strain of acid-fast organism with many characters of *M. tuberculosis*, isolated from the vole. The former strain was produced by the French workers, Calmette and Guérin, and is popularly referred to as B.C.G. By repeated sub-cultivation on a medium containing ox bile it became non-virulent for animals, including cattle. Research work carried out at Cambridge showed that, following its injection by the intravenous route, a serviceable immunity was set up in bovine animals. The tests applied consisted in the exposure of vaccinated animals to artificial infection given by mouth. The immunity, however, is of short duration and it is necessary to repeat the injection every six months in order to maintain it at a high level. Some experiments or trials have been carried out in the field. Unfortunately, it has not been possible to control them adequately, but the strong indications are that, beginning with a herd of cattle in which a high incidence of tuberculosis is present, it is possible, by practising B.C.G. vaccination on the calves born in the herd, to maintain them free from tuberculosis and gradually to replace the infected animals by immunised cattle, and so produce from the original animals a herd free from the disease. This is made possible because practically all calves, even from infected mothers, are born free from tuberculosis and can be subjected to vaccination.

The strain of acid-fast organism from the vole was isolated by Wells in this country. Experiments have shown that, following its injection into cattle, a high degree of resistance to artificial infection with *M. tuberculosis* is set up. This immunity differs from that engendered by B.C.G. in that it is of much longer duration, a single intravenous injection being followed by resistance for probably two years or more. There is some evidence that two doses of the vole organism, given not more than 28 days apart, give rise to a higher degree of immunity than does a single dose. Most of the observations have been confined to cattle kept under laboratory conditions. A few field trials have been made, and the indications are that cattle vaccinated with this strain will to a marked extent withstand exposure to natural infection.

It is not clear how far the immunisation of cattle will be used in this country in the control of tuberculosis. The method of control being adopted is to diagnose infected cattle by the application of the

tuberculin test, to dispose of reacting cattle and prevent the introduction of further infection to the now healthy herd. Cattle vaccinated with either B.C.G. or the vole strain of organism become reactors to the tuberculin test. After some time, twelve months or more, following a final injection, they again become non-reactors. The tendency is to encourage farmers to eliminate reactors to the tuberculin test from their herds rather than to practise immunisation.

Another disease of cattle of considerable economic importance is "mastitis"—inflammation of the udder: this disease cannot be controlled by immunisation, at least in so far as our present state of knowledge is concerned. It may be that the basic cause of mastitis has not yet been definitely ascertained, but all the recent work indicates that micro-organisms in the udder are closely associated with the disease and that their elimination from the udder is followed by a cessation of the symptoms of the disease. The organisms most commonly found in this condition are streptococci, and much of the mastitis in dairy herds is associated with *Str. agalactiæ*. While the incidence of the acute disease varies from herd to herd, there is much evidence that *Str. agalactiæ* infection leads to a chronic mastitis, with induration of the udder and consequent depression of milk yields. A chronic or sub-acute infection may become acute, following damage to the udder from extraneous causes. There is little doubt that infection is largely confined to the udder and adjacent structures, *e.g.* the teats, and that infection is transmitted from infected to healthy animals during the milking process, either by the hands of milkers or through the medium of milking machines. Precautions against such transference of infection has proved valuable in controlling the spread of the disease within a herd.

The most recent method of control is to destroy the streptococci *in situ* and to guard against further introduction of the organisms into the herd. Use is made of the action of drugs on streptococci; drugs of the sulphonamide series and penicillin are valuable agents in the control of this form of mastitis. Sulphonilamide suspended in water or in wax and oil was the drug of choice until penicillin became available. Penicillin has now largely replaced sulphonilamide. The application of the drug is through the teat canal by injection, and amounts as small as 1 c.c. will penetrate to the necessary parts of the udder tissue. Repeated injections are necessary to ensure a sufficiently high concentration of the drug for a period long enough to destroy all the streptococci. Treatment of the udder is accompanied by treatment of the teats, especially when cracks or sores exist. The results of the treatment vary according

to the amount of penicillin injected and the number of applications. Up to 70 per cent. bacteriological cures follow two treatments given at 24 hours interval with 20,000 units. Better results follow higher doses given once daily for 4 or 5 days. Field experiments show that the best results, *i.e.* 100 per cent. cures, can be obtained with 100,000 units injected every 24 hours for 5 days, accompanied by adequate treatment of the teats and a thorough disinfection of all apparatus and equipment, as well as a cleansing of the skin of the cow. In order to prevent reinfection of the udder, for the cured udder is still susceptible to infection, it is highly important to carry out adequate disinfection of apparatus, etc. There is much evidence that streptococci can remain alive and virulent for long periods outside the body: milkers' hands, for example, can retain infection for many days. There is also considerable proof that multiplication of *Str. agalactiæ* takes place only in the udder and probably to a much less extent in teat sores.

Mastitis associated with *Str. agalactiæ* differs from the disease in which some other types of organism are involved. In some herds corynebacterium, especially *C. pyogenes*, is the organism associated with mastitis. Whereas *Str. agalactiæ* is confined, within the body, to the udder and, when removed, re-infection can only occur from outside the body, *C. pyogenes* is found in sites within the body, other than the udder. It has been isolated from the uterus, feet, tonsils, lungs, etc.: it follows, therefore, that its removal from the udder without further introduction from outside the body may not result in the control of mastitis associated with its presence in the udder. Given the necessary conditions in the udder, *C. pyogenes* can invade from other sites within the body. This causes much difficulty in controlling mastitis in which *C. pyogenes* plays a part. Immunological methods are being applied for the control of the organism, but the results have not yet been entirely satisfactory.

In pigs, the most important disease which can now be controlled by immunological methods is swine fever. In some countries the method adopted is the simultaneous injection of blood containing the causal virus and immune serum. In this country the use of the living virus is prohibited. Immune serum is sometimes used on pigs in contact with the infection, in the hope that, while there is sufficient passive immunity set up by the serum, they will pick up virus naturally from infected pigs or premises and so be rendered actively immune. The results have been disappointing.

Recent work has shown that a satisfactory immunity can be produced by the use of a vaccine prepared from blood containing the virulent virus, to which the dye "crystal violet" is added in a

solution of ethylene glycol, the mixture being incubated at 37° C. for 14 days. Experiments have shown that the virus is either killed by this treatment or is so altered that it is quite incapable of setting up infection and that vaccinated pigs do not excrete active virus. Immunity, following a single injection, lasts for at least 12 months. Much interesting work has been done with this vaccine and it is now in general use throughout the country.

In poultry, immunological methods of disease control are also in operation. The best example in this country is the control of fowl pox, a disease caused by a filterable virus. Use is made of the closely-allied pigeon-pox virus in the immunisation of fowls. The pigeon-pox virus is cultivated either on the skin of the living pigeon or on the allantoic membrane of developing chick embryos. It is applied in an emulsion to a few feather follicles, following light scarification. The vaccine reaction is slight, consisting only of a swelling of the treated follicles : it is followed by an immunity which persists for several months. Fowls treated at any age resist artificial and natural infection, and the method is regularly used in flocks in which fowl pox is known to exist.

In controlling the form of fowl pest known as "Newcastle disease" legislative measures are adopted in this country. In some countries, however, active immunisation is given by the use of a specially attenuated strain of the causal virus, cultivated in developing chick embryos. Highly satisfactory results are reported from countries in which the disease has acquired a high virulence.

There are many other diseases affecting livestock for which satisfactory methods of control are in use. These few discussed above will serve to show how modern methods are being practised. As time goes on, improvements will doubtless be made and the results of research in the various branches of science will be applied to our livestock problems whenever they are found to have any bearing. It cannot be emphasised too strongly that research work on any subject may be found to have a place in our own work and that full advantage must be taken of any finding, no matter how remote it may appear to be at a first examination.

A CRITICISM OF MATHEMATICAL METHOD IN THE ANALYSIS OF OBSERVED PHENOMENA

By JOHN NANCE

ALPHA-THOUSAND people arrive at St. James's Park Station, Westminster, between the hours of 9 and 9.30 every morning from Monday to Friday, and leave again between 5.30 and 6. Only Beta-thousand arrive on Saturday mornings, and they leave between 12 and 12.30. A negligible number arrive on Sundays and Bank Holidays. The total number arriving and leaving on weekdays is reduced during the latter half of July and the month of August.

Individual person Nance is implicated in this majority movement in the average of five mornings out of six, four evenings out of five, roughly every other Saturday. On the mornings when he is not implicated, he has got up late, travelled on to Westminster Station with a friend, caught a Number 11 bus instead of the train, is in bed with a cold, has had an argument over breakfast, or found his coffee too hot to drink. On the evenings when he is not implicated he has stayed in town to go to the theatre, or has had to work late, and so on. Similar considerations affect his Saturday movements.

To an electronic intelligence, examining activity in the macroscopic universe, the periodicity of events of the order of St. James's Park Station would be observable and recordable. It could be expressed in statistical laws, and those laws when formulated would probably prove accurate to within plus or minus point something per cent. They would indicate an unfailing regularity of macroscopic method, which in its turn would suggest the probability of some predetermined order. A more detailed examination, however, would introduce a disturbing factor. Though the periodical activity of Alpha-thousand units of arrival or departure during the lapse of Gamma-pulses of para-time in a cyclical series has been established by experiments the authenticity of which cannot be impeached, it appears that the activity of the individual arrival and departure unit Nance is not bound by those laws. Underlying the statistical

regularity of the mass there appears to be a genuine indeterminism amongst the individual units.

Moreover, since the electronic intelligence will be intrinsically incapable of appreciating space-time, sensory perception, pretty girls, periodical literature, too hot coffee, conversation, or the workings of the human "mind," it will be able to obtain no picture of the causes which underlie the erratic behaviour of the individual units, and there will appear to be a complete discontinuity of relationship between the movement of individuals as a mass and the movement of individuals as individuals. Nor will it be able to hazard even an opinion regarding the reality of the units themselves or the nature (other than the mathematical nature) of the relationships which exist between them. "If we cannot discover"—one can imagine an electronic intelligence as saying—"some molecular action underlying the mathematical foundations on which we base our conjectures, the alternative would appear to be something of an extra-atomic nature. So far as the reality of these units is concerned, we can only refer to them as waves of probability." *

There is a well-known passage in *Physics and Philosophy* in which Sir James Jeans likens the physicist to an inland peasant who knows nothing of the sea or of ships but owns a wireless set upon which he picks up messages sent from ships all over the world reporting their positions in terms of latitude and longitude. By intelligent analysis of the data thus obtained he could acquire a mass of accurate information concerning the movements of shipping, would be able to construct the world as a sphere, and would even be able to mark upon its surface in some detail the land masses at any rate to the extent of their being parts never visited by ships; but at the end of it all he would still know no more than he did when he started of the *nature* of sea or of ships.

He would have been working, of course, by abstraction of the factors "movement" and "location" from the natural whole. Abstractions are, at the moment, disregarded by philosophy as being (apparently) too abstruse, and by mathematical physics as being (apparently) outside the frame of reference. This leaves us in the peculiar position of having no field in which they may be studied. But much interesting work has been done in the consideration of the problems presented by abstraction, notably, within the frame of physical reference, by Mr. Dunne in his books on *Serialism*. Commencing from the hypothesis, with which I do not think there will be much disagreement, that Action in a system of becoming

* Quoted, with slight modification, from E. Zimmer, *The Revolution in Physics*.

is the equivalent of Substance in a pattern of being, whilst Energy, the intensity of Action, is the equivalent of Mass, the concentration of substance, he envisages Energy as abstracted from Action by the movement through Action of a three-dimensional field.

Action (equals substance equals sensory phenomena) is a "reality." On the same plane of reality is the instrument employed to perform the abstraction. The observer notes how reality reacts on his recording instruments, and in this way discovers a world of abstractions. These abstractions tell him all about the thing he is investigating within the scope of each investigation performed, but even the total of all investigations may not reveal the nature of the thing itself (a whole has attributes which neither are the sum of the attributes of its parts nor derive from the attributes of its parts). Thus, when I measure a piece of wood with a foot-rule, the travelling field employed by me abstracts from the realities A wood, B foot-rule, C psychological process: A length, B measurement, C understanding. If "wood" were excluded from sensory observation, all my statements about "wood" would be on the plane of abstraction—how long, or hard, or dark, or heavy it was. What was the real nature of the reality "wood" would remain a matter of speculative interest. Similarly, since sensory perception is an abstraction from sensory phenomena, my observation of objects will always be on the plane of an abstraction from true reality.

Yet there is another dictum of Sir James Jeans, in which he speaks of the mathematical *nature* of the universe! This nature is the more remarkable (he observes) since we have not deduced the principles of mathematics from the study of nature, but out of our own minds. Having formulated the laws of mathematics to our own satisfaction, we turn our attention to the world outside ourselves and find, to our astonishment, that it obeys those laws. It is as if, having drawn up the rules of the game for ourselves, we were to find everything in the universe playing the game according to our rules.

Having developed powers of tactual discrimination, in short, we are astonished and delighted to find that the universe is full of hard things and soft things! Surely the fallacy is elementary. What has been overlooked is the absolute limitation of mathematical principle to a certain specific class of phenomena; to a class of phenomena, in fact, to which mathematical principles can be applied! No mathematician would deny that an object which is capable of mathematical dissection may also be beautiful; whilst when calculating the velocity of a moving object the mathematician may have an entirely different sort of interest in whether or not that

object wins the race. But the suggestion, of course, is that in some way the mathematics of the universe is more fundamental, or more real, or more important than its sensory attributes.

Professor Whitehead has fought, unfortunately, his last fight for the inconceivability of the assumption that the warm, colourful, noisy world we know is a mere figment of our sensory apparatus. But this inconceivability is not a matter of reason, but of feeling. Upon what basis do we assert that what reason tells us is right, yet what feeling tells us is open to any criticism which we may care, reasonably, to make?

The view is widely held that the principles of mathematics are the principles of what we call logic. There is nothing so impossible as a mathematical impossibility, nothing so sure as a mathematical certainty. Logical and mathematical principles have frequently been put to the test of exhaustive practical experiment, and have been proved unassailable. If I disagree with X, whose logic is impeccable, then I must attack his inferences or his premises; I may not attack his argument. I am not entitled to throw doubt upon any conclusion arrived at by logical or mathematical means. That is, I am not entitled to throw any logical or mathematical doubt.

It is interesting to record that it was by no means always so. The oldest written record of History and Philosophy we possess is that of the Old Testament. In the whole of the Old Testament, outside the Apocrypha, you will find no single example either of logical reasoning or of mathematical argument, that is, of reasoning from mental conclusion to mental conclusion and not from sensory experience to sensory experience. What a man saw or tasted or heard or felt or smelled was an accepted fact. If Baalam's ass seemed to talk, it talked; if Elijah seemed to be swept up to heaven in a whirlwind, he was so swept up to heaven. No one was one whit incommoded if such an event did not accord with the general run of sensory experience; it was recognised that the range of sensory experience is unlimited. No one thought of querying the circumstance that God should speak out of a whirlwind; no one thought of querying the possibility of an impalpable and invisible Deity existing on equal terms with palpable and visible man.

It was never seriously suggested that the world was not as it seemed to be until it was suggested by the Greeks. It is no accident that logic and mathematics are so closely akin. They are merely respectively verbal and symbolic representations of the same attitude. Greek philosophers set themselves verbal dilemmas to solve just

as Greek mathematicians set themselves symbolic dilemmas. That a verbal system of philosophy has never received the universal acceptance accorded to the symbolic system is attributable simply to the circumstance that the proof of a verbal hypothesis is in experience, and experiences differ, whilst the proof of a mathematical hypothesis is inherent in itself.

Let me adduce a simple simile to sum up the operations involved in each case. The process of the acquisition of knowledge may be likened to a shuffling of counters, all of which to begin with are equally valueless. Values are given to the counters by actual or derivative experience (I *know* that fire is hot; I am prepared to accept the "fact" that the North Pole is cold). As experience increases, the counters fall into a pattern, and we find that additions to the pattern are made according to a certain scheme. By applying the scheme to the pattern we can intelligently suppose, on the basis of existing experience, what future developments are likely to take place in it. Thus we assume that, if fire is hot, other fire-like substances will be hot as well. This leads to some fascinating errors of attribution which have absorbed the attention of philosophers for centuries. But in general, the wider the pattern of experience grows, the clearer the picture we can get of the scheme as a whole; and so the more accurate our prognostications will become. "False" knowledge added to the pattern will, of course, derange the scheme; so that when the experience is corrected we may have to make a substantial rearrangement of our "conceptions." If the false experience is bedded deep enough in the pattern, we may find that impossible.

The basic pattern of experience is purely sensory. We build upon that basic pattern a pattern of "mental" experience which we call knowledge, by shuffling the counters ourselves instead of having them shuffled for us by the sensory apparatus in our bodies. But the two processes are akin. When we are trying to work out a problem, whether voluntarily on the mental level or involuntarily on the sensory level, we are, in effect, trying to fit as yet unvalued and unapportioned counters into the pattern and give them values which will (*a*) accord with the existing pattern as a whole (what we already know to be true) and (*b*) conform to the scheme along which that pattern has been developing.

In pre-Greek times, thought was a voluntary shuffling of the counters of experience at the sensory level; and language developed along similar lines, so that we cannot make ourselves understood at all except in terms of sensory simile. What the Greeks discovered was that it was possible to indulge in pure pattern making;

that it was possible to shuffle the counters into a coherent design without any regard for the values in terms of sensory experience.* When the shuffling was done on a purely symbolic level (mathematics), the only "proof" necessary was that of schematic conformity and inherent consistency; but, when the shuffling was done with language, there remained the essential difficulty of the sensory undertones, and the "proof" of the pudding remained in the eating.

The practical convenience of the symbolic method is obvious; the danger is that its inherent limitations may be overlooked. The first of these limitations is that it consists in *absolute* abstraction; the second that its abstractive possibilities are limited by the space-time character of the counters employed. Whilst it is impossible, on the one hand, to explain the artificial pattern in terms of the practical one, it is equally impossible, on the other, for the artificial pattern to express that which is beyond the inherent capacity of the practical one to express. The problem which has for thousands of years confounded the seer and the mystic now confounds the mathematician; but whereas the mystic knows what he feels, even if he cannot express it, the mathematician does not know what he knows. He is accordingly particularly prone to confusing what is a conclusion of his mathematics with a conclusion as to what his mathematics is about.

Because it is impossible to *argue* except from A to B, and because that type of argument has an important practical significance, we are at the moment going through a phase in which the A-to-Bness of things outweighs in reality and importance all their other attributes. There is a genuine significance in the circumstance of mathematical "discovery" always preceding physical discovery. Sir Arthur Eddington pointed out how impossible Einstein's work on relativity would have been had the tensor calculus not already been available at his elbow. The process is nothing new. How impossible craft was before we learnt to use our hands! How impossible art was before we learnt to feel! There is more than a little truth in Spengler's paradoxical assertion that all manifestations of art and science have no validity save as chronological phenomena produced in response to the culture spirit of the age. We laugh at the Russians and their political genetics; but if there might be any reason for supposing that the Russians have not inherited the Greek mind to the extent that we have—and there is—one could at once appreciate their point of view. Heresy hunting was taken quite seriously until comparatively recently—until, in fact, the mathematical abstraction had attained complete dominance over

* See, for example, NANCE, *Hibbert Journal*, 1948, *Vol. XLVII*, p. 65.

all other methods of approach. The new heresy is to dispute the validity of conclusions arrived at by mathematical means.

Meanwhile we are, at the moment, all materialists and determinists because mathematics understands no values except material values and is absolutely rooted in determinism. For, in the construction of the artificial pattern, each step must necessarily entail the next, and it is of fundamental importance in the interests of the inherent proof that the steps as a sequence should be unassailable whatever the sequence is about. No one can doubt the usefulness or validity of such conceptions within a special frame of reference; it is when we attempt to extend the deductions and make them applicable to all frames that we go wrong.

Insurance companies have long known that the statistical mathematics of a mass can be formulated into laws of practical significance; but, though there is as a result an appearance of determinism, no practical determination is in fact involved. Thus, though it is a statistical fact that X persons will die during the coming year, and that of those persons Y will be killed as the result of accidents, no one can say as a result of what human errors attributable to what human failings or preoccupations those accidents will occur; and no one can say which of any number of persons, however small, or large, will be the ones to be involved. Individual co-operation which is, in one sense, absolutely implicit is, in another sense, absolutely unascertainable.

The plain truth is that no statement whatever can be made on the basis of a mathematical or statistical calculation except a statement about mathematical or statistical values. That is true of all such statements, just as it is true of statements concerning the results of other abstractive enterprise. The beauty of a thing is no criterion of its goodness; its colour is no criterion of its shape; its enjoyableness does not authenticate its mathematical accuracy. Nor do the rules which govern one abstraction necessarily govern another. We cannot talk of the shape of beauty nor the mathematics of goodness. We are, in fact, in difficulty with beauty and goodness all the time because they are not amenable to human reason. But to proceed from that point to the assertion that because they are not amenable to human reason they are therefore less true, or less real or less valid than the things that are is simply to misunderstand the whole nature of the problem involved.

Mathematics is the law of one special relation. That relation has assumed at the present time a transcendental importance because mathematical determinism is also the law of argument and the law by which at present, but not always in the past nor necessarily

always in the future, we interpret our sensory experience. But we have no warranty for carrying over the laws into any other field of abstraction ; and in particular we have no warranty for asserting that, because we cannot in this age think of a thing or relate a thing in other than mathematical terms, mathematical terms have an absolute significance in the universe as a whole.

SOME NEWER TECHNIQUES IN ANALYTICAL CHEMISTRY

By J. R NICHOLLS, D.Sc, F R.I.C.

The classical methods of quantitative chemical analysis are based primarily upon gravimetric and volumetric determinations, separation of interfering substances being effected by preliminary operations. Most of these operations in the inorganic field depend upon selective precipitation, but other procedures may be used for special purposes, *e.g.* electrolytic deposition. With organic substances separations are effected by solution or extraction with immiscible solvents, distillation, or reactions which may involve chemical breakdown. With increasing knowledge these methods developed to a high degree of accuracy and precision, covering a very wide field. They still form the backbone of the science.

Of relatively recent times newer techniques have been developed, some of them depending on physical or biological principles, and these have widely extended the scope of the science. By their use much more information can be obtained and certain errors inseparable from the older methods can be avoided. The modern chemist must be familiar with all these newer techniques, since on occasion only one may give the desired information, while a combination of several may be necessary to extract the maximum of information. Originally applied as research tools, most are now used in some connection for routine analysis.

The principles of some of these newer methods will be described.

Micro-analysis

Quantitative analytical determinations depend upon some final measurement, usually of a weight or a volume. The accuracy of a determination cannot exceed the accuracy of the measurement and chemists have always been concerned about their weights and measures. The importance of good analytical balances was early recognised and makers produced remarkably good instruments

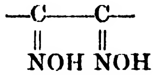
weighing to something under a milligram. It was realised that greater sensitivity combined with high accuracy involved exceptional expense and there was little demand for instruments having a precision greater than that of the methods for which they were used. For many years, therefore, the best balances in ordinary use weighed to about $\frac{1}{10}$ of a milligram.

With the development of organic chemistry and the need for ultimate analyses on quite small quantities of material, the possibility was investigated of scaling down considerably the quantities of sample to be analysed. This involved the elaboration of apparatus and equipment capable of dealing with the small amounts of material handled, investigation of the errors likely to arise in the operations involved and the precautions to be taken from their avoidance. The pioneer in this work was Pregl and he was able to devise methods capable of determining carbon, hydrogen, halogens, nitrogen, sulphur, etc., on a few milligrams of organic substance. He developed specialised micro apparatus and equipment, and used specially designed micro-balances capable of weighing accurately to less than $\frac{1}{1000}$ of a milligram. Other workers, amongst the foremost being Emich, applied similar principles to inorganic substances, and nowadays micro-methods can be applied to a large number of determinations of varying types. The technique is specialised and experience is required to avoid unsuspected pitfalls; but the saving in time and material may be considerable. It cannot be presumed that all ordinary determinations can be reduced to the micro-scale since certain factors, such as solubility, which have negligible effect when working with 1 gram of material may produce considerable errors when a few milligrams are employed, since the volumes of solutions used in the operations cannot be reduced proportionately.

Organic Reagents for Inorganic Analysis

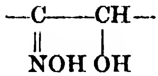
It has long been known that some organic substances react with certain metallic ions giving either a coloured solution or an insoluble complex, and such reactions have been utilised for special determinations. The complexes are either addition compounds or chelate compounds formed by co-ordination. It was later found that definite groupings in the organic substance were responsible for the formation of the complex and that there were numerous compounds containing a particular arrangement of atoms which reacted similarly. Frequently one grouping is specific for one ion under definite conditions.

Examples are

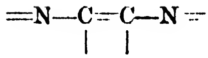


for nickel

for cobalt



for copper



for iron (ferrous)

Some of the known reactions were unsuitable for quantitative purposes because of inaccuracy due to solubility or other factor. By preparing a series of compounds each containing the specific grouping it became possible to find a reagent giving a complex sufficiently insoluble or otherwise suitable for quantitative use. Many of these organic complexes are soluble in immiscible solvents so that by extraction from an aqueous phase they can be separated from other ions.

Certain of these organic reagents are capable of forming complexes with many inorganic ions, but each has a definite *p*H range within which it is formed in aqueous solution and a slightly smaller range within which it can be completely extracted. By careful control of *p*H it is possible successively to extract a series of complexes formed by different cations with the same reagent. After suitable manipulation, the separated complex can be utilised for gravimetric or volumetric determination of the cation or, as most of the complexes are coloured, the final determination can be carried out colorimetrically or photometrically.

Polarography

Many substances, both inorganic and organic, are capable of being oxidised or reduced. This reaction takes place at a specific electrical potential characteristic of the ion or radical reacting. At potentials differing from the oxidation or reduction potential a solution containing the substance passes a small current depending upon the conductivity of the solution; but at the oxidation or reduction potential there is an increase in current proportional to the number of molecules of the reacting substance. For quantitative purposes the solution must contain relatively few molecules or else the differences in current are not accurately measurable. This is an advantage since quite small proportions of substances can be

determined. It is necessary to employ a special electrode consisting of a reservoir of mercury delivering drops at regular intervals from a capillary tube to fall through the solution being tested. This electrode is maintained at a steadily increasing potential so that each successive drop of mercury differs in potential from adjacent drops. By plotting a current/voltage curve (which may be done automatically with a recording pen) the graph will show the current which flows at each of the potentials represented by the drops of mercury. By arranging a suitable drop rate and choosing the galvanometer, the graph shows a practically continuous line with sharp steps or waves at the potential where the reaction current is flowing. The voltage is a qualitative recognition of the ion or radical and the rise in current at the voltage is a quantitative measure of its amount. When several substances are present in the solution each reacts separately and the one test will identify and determine each provided the amounts present are appropriate. The pioneer in this work was Heyrovský.

Chromatography

When a solid substance is in contact with a liquid containing substances in solution certain forces come into play whereby the solutes are attracted to the surface of the solid. In many instances the forces are extremely weak, but in others they may be sufficiently strong to cause adsorption. This phenomenon has had practical uses for many years and is the basis of the use of decolorising carbons. If a solution is passed through a column of powder capable of exerting the adsorptive power, some of the substances in solution may be retained in the column and will not appear in the solvent which passes out at the bottom of the column. If now some pure solvent be passed through the column the adsorbed substances gradually pass lower and lower down the column, the speed depending upon the adsorptive forces. The column thus has a series of rings of adsorbed substances which may be quite separate from one another. If the substances are coloured they are clearly shown as annular zones and it was experiments on coloured natural substances which gave the name of chromatography to this form of separation. The originator of this technique was Tswett, but the discovery was not used for several years when its usefulness for separating plant pigments was realised. Slight differences in adsorptive forces, such as obtain in isomers, are accentuated by the elution process, and substances have been shown by such means to be mixtures when they were previously thought to be single substances. By continuing the elution with the pure solvent the

separated substances eventually pass from the column as a solution, but it is usual to carry the elution only so far as to give effective separation in the column. Obviously the method is particularly valuable with coloured substances where the progress of the separation can be directly observed. The method can, however, be applied to colourless substances when by special means the course of the separation can be detected. If the substances are fluorescent the column can be observed in ultra-violet light; or the column may be drained, pushed out of the tube and a reagent streaked down the column, the zones being then mechanically separated by cutting. Many substances can be used as adsorbents and the solvent may be water or organic liquids; but most work has been done with a column of alumina and a non-polar solvent such as light petroleum or benzene.

Partition Chromatography.—Substances show considerable variation in their partition coefficients between an aqueous phase and an immiscible solvent and the technique of chromatography has been applied to separation by such means. The aqueous phase may be held stationary by silica gel and the immiscible solvent allowed to flow through. With acidic substances it is sometimes possible to incorporate an indicator in the water so that the adsorbed acid zones are clearly shown. A similar principle is applied in paper chromatography whereby moistened filter paper acts as the column and the solvent is used in the vapour form. The movement of the substances being separated is recognised by spot tests carried out on the removed and dried paper. A greater degree of separation is obtained by carrying out the test first in one direction and then in a direction at right angles, so that zones in two dimensions are obtained. Different solvents can be used for the two directions. By applying appropriate colorimetric tests to the paper the position of the zones can be indicated, and where separation is reasonably complete the zones may be cut out and approximate quantitative results obtained by suitable determinations.

Spectrographic Methods

Radiations are known ranging in wave-length from about 30,000 metres down to about 1×10^{-12} cm.; the longest (down to about 10 cm.) are used in radio communications and the shortest are called cosmic rays. Within these extremes is a relatively narrow region where the radiations show optical phenomena capable of being adapted for analytical purposes. The optical range of the human eye is from about 7×10^{-5} cm. to 4×10^{-5} cm. Photoelectric cells and photographic plates are sensitive appreciably further on

either side and can be used to detect infra-red rays having longer wave-lengths than visible light and ultra-violet rays, X-rays and gamma rays having shorter wave-lengths. By use of a spectrograph for resolving the radiations, investigations can be carried out throughout the whole range from infra-red rays to gamma rays.

When such radiations are incident on a molecule they suffer one of several fates, depending upon the wave-length of the radiation and upon the nature of the molecule; generally the rays may be transmitted, absorbed or scattered. By observing the effect of the radiations of different wave-lengths information can be obtained about the molecules concerned.

Absorption Spectrography.—When radiations pass through a column of gas or liquid some are transmitted and some absorbed, the absorption depending upon the nature of the molecules which are in the path. By means of the spectrograph the absorption at the various wave-lengths can be recorded and these are characteristic of the molecules concerned. The intensity of the absorption at any particular wave-length is proportional to the number of molecules affecting the radiation, and, by comparison with the intensity of absorption of a known concentration of a substance at the same wave-length, quantitative results can be obtained. Absorption spectra of pure substances are complicated systems of absorption and transmission, and the spectra of mixtures are additive. For quantitative purposes mixtures can only be resolved if absorption maxima can be selected which are unique for the individual substances being determined.

In general, radiations absorbed in the far infra-red part of the spectrum correspond with changes in the rotational energy of the absorbing molecules, those absorbed in the near infra-red with changes in vibration and rotation, and those absorbed in the visible and ultra-violet correspond with electronic transitions accompanied by vibrational and rotational changes.

Infra-red absorption spectroscopy is the ideal tool for the determination of the composition of mixtures of substances which are of closely similar constitution and properties, such as are almost impossible to separate or determine by other means.

Emission Spectroscopy.—Any mixture or combination of elements can be made to emit radiations which can be resolved into line spectra characteristic of the elements concerned, and the intensities of these lines are proportional to the amounts of the elements responsible for the lines. The radiations may be produced by flame, arc or spark. A reference spectrum of another element, *e.g.* copper, is taken side by side with the test spectrum to facilitate identification

of wave-lengths. The spectra obtained are compared with those obtained from similar samples containing known amounts of the particular element being determined, or lines of the element in question are compared with those of another element present in known amount as a form of internal standard.

RAMAN EFFECT.—Radiations which are incident upon a molecule are to some extent scattered and a small proportion of the scattered light may be of different wave-length from that of the incident light. This phenomenon was discovered by Raman. Each different linkage and structure has characteristic Raman frequencies, which may be modified slightly by the rest of the molecule, but are generally unaffected by the presence of foreign molecules. In principle the Raman spectrum may be used to identify a compound in terms of its structure and to determine the proportions present in a mixture, since the intensity of each line is proportional to the concentration of the entity concerned.

X-RAYS.—The frequencies of X-rays emitted by a target on bombardment with cathode rays are characteristic of the element composing the target. Primary X-rays on striking another target may give rise to characteristic secondary rays which can be used for determination of the second target.

Atoms in a crystal are arranged in a regular pattern and are capable of acting as a diffraction grating to X-rays. From the position of the diffracted beams the interplanar spacings and hence the structure of the crystal may be calculated. Thus it is possible to distinguish the different polymorphic forms in which a substance may exist.

ELECTRON DIFFRACTION.—An electron beam on striking a crystalline surface is subject to diffraction governed by known laws just as X-rays are. The low penetrability of electrons is turned to advantage in the examination of surface films. X-rays pass through the surface atoms and penetrate to great depths, whereas electrons are diffracted strongly by the outermost layers.

MASS SPECTROGRAPHY.—At very low pressures a suitable ribbon-shaped stream of charged gaseous ions can be deflected in an electric or magnetic field so that ions having different ratios of mass to charge can be brought to different foci. The series of foci can be recorded on a photographic plate and the intensities of images measured. This method of attack is of great value for determining abundance ratios of isotopes. It is also of unique value for mixtures of gases or vapours of closely allied compositions, each of which when suitably ionised produces particles of specific abundance in mass to charge ratios.

Tracer Elements

As a result of atomic research certain isotopes of elements are available in small quantities for scientific work in other fields and some have been applied to analytical problems. Compounds containing these isotopes are earmarked and their subsequent history can be traced. In principle, any element which is present in a substance in an isotope abundance ratio different from normal can be followed through a sequence of processes or reactions by means of appropriate isotope abundance measurements. If the isotope is radioactive the activity serves as a detector, and if it is not radioactive the compound must be converted into one capable of being analysed by the mass spectrometer. For example, methionine was synthesised to have isotope abundances of carbon and of sulphur above normal and when fed to rats the cystine recovered from their hair had abundance ratios above normal for sulphur, but normal for carbon. The sulphur was examined as H_2S and carbon as CO_2. Clearly the sulphur but not the carbon of methionine had taken part in the formation of the cystine. The isotope dilution method of analysis is based upon the fact that a compound which has an abnormal isotope content of an element is inseparable from the compound of normal composition by ordinary laboratory processes. If therefore a known weight of a substance X, having a predetermined concentration of a particular isotope, is added to a mixture containing normal X, and after the mixture has been made homogenous a proportion of X (no matter how small) is isolated and purified, the concentration of the isotope in the separated material enables the proportion of X in the original mixture to be determined.

Micro-biological Methods

The discovery of vitamins resulted in a need for methods of determination. Since these substances are essential for the maintenance of bodily health, their absence results in deficiency diseases. Symptoms or effects of such deficiencies are often characteristic and detectable in animals or birds, and biological methods were developed capable of giving an approximate assay of the vitamins. Such tests require many animals and the time taken for an effect to be observable may be weeks or even months. The tests are the province of the biologist and not the analytical chemist. Since relatively small amounts of substance are in question and they are present in very much larger amounts of food material, chemical methods are difficult and often impracticable. It was shown, however, that vitamins are essential factors for the growth of bacteria and that

these could be used as test subjects, since in the absence of the vitamin the bacteria do not grow and up to a definite limit growth is proportional to the amount of vitamin present. Growth involves multiplication but the number or amount of bacteria is not readily measured accurately. Certain bacteria produce acid when they grow and the amount of acid is proportional to the extent of growth. The test therefore consists in washing such bacteria free from acid and placing measured amounts of them in a series of tubes containing a medium having none of the particular vitamin to be determined but all other necessary growth factors. The series is then divided into two halves and to the tubes of one half are added known amounts of the pure vitamin and to the tubes of the other are added known amounts of an extract of the test material. All the tubes are incubated at the optimum growth temperature for a definite time and then the acid production in each tube is determined by titration. From the results obtained from the series of tubes to which the pure vitamin has been added a graph can be drawn relating acid production to vitamin added. If the quantities taken have been of the right order the graph will be a straight line and will serve as a reference. The results from the other series when similarly plotted should also give a straight line if the amounts taken are appropriate and can be directly compared with the reference graph to give the amount of vitamin in the test sample.

This method can also be applied for the determination of certain amino-acids. The amino-acids can be divided into two classes called essential and non-essential. The essential amino-acids for a particular animal are those which the animal cannot build up for itself and must therefore be introduced in its food. The non-essential amino-acids may be no less necessary to the animal, but they can be manufactured from simpler material in its metabolic processes. Without an adequate supply of the essential amino-acids proper growth and development do not take place. This applies to lower organisms such as the bacteria as well as to animals and by proper selection they can be used as test agents. As with the vitamins it is the acid-producing bacteria which are the most convenient to use. Much research has had to be undertaken with every selected organism to find out exactly all the materials which are essential to its growth and to provide a medium which contains all these essentials except the one particular substance it is desired to determine. In addition, pure strains of the organism have to be propagated and frequent tests have to be made to see that the organism behaves normally and does not become acclimatised to a deficiency of any one ingredient.

Precipitin Reactions

Proteins are complex nitrogenous bodies which are readily distinguishable as a class, but in our present state of knowledge are not easily differentiated by chemical means. On occasion it may be necessary to identify a protein to ascertain the origin of some material common to many organisms. For example, it may be shown that a stain is blood and it may be of great importance to say from what animal the blood came. If the stain is old it may not be possible to identify microscopically the corpuscles, but a specific test for some other constituent might enable identification to be made.

The blood of living animals is known to possess specific characteristics which may be profoundly affected by foreign substances. If a foreign protein is introduced into the blood-stream of an animal, natural protective measures come into force and substances called anti-bodies are produced, which have the power of precipitating the foreign protein, thus reducing its effect on the animal's metabolism. If the amount of added protein is not too great, anti-bodies are produced in excess of that required to deal with the intruding material, and if they are separated from the blood they can be used to precipitate more of the protein. The anti-bodies produced to deal with any one protein are specific for that protein and do not react with others unless the relationship is extremely close. To make use of this test, a number of animals are taken and into a vein of each is injected a small amount of a known soluble protein, *e.g.* the clear serum from the blood of a horse, or sheep, or bird, or man, etc. After a few days a small amount of blood is removed from each animal and treated to remove the blood corpuscles and to produce a clear serum. The sera are then used as test reagents by adding each to a clear serum of the blood or protein being examined. Precipitates are given only with those proteins corresponding to the ones which were used in making the antiserum. This test can be carried out on extremely small quantities and, although it is primarily qualitative, it can be made quantitative within somewhat wide limits.

Statistical Analysis

All analytical determinations are subject to errors and it is essential to have a proper appreciation of their magnitude. Errors arise in sampling for analysis, in the actual method used and in the final measurement. It is characteristic of chemical and physical methods of analysis as a class that the inherent precision is relatively high and the accuracy capable of being checked. Sampling errors

may, however, be considerable and statistical analysis may be necessary to decide the extent of sampling and the interpretation of the results of the different parts of a consignment. Data obtained on natural products may require statistical analysis to obtain the variation from the average.

In devising new analytical processes it may be necessary to investigate the influence of several variables. The proper design of experiments to obtain the desired information with a minimum expenditure of time and material is best achieved by the method known to statisticians as "factorial," in which the effect of changing several variables simultaneously can be examined.

Other Techniques

In addition, there are many techniques of less general application but invaluable for specialised problems.

The *electron microscope* enables magnifications to be obtained far greater than are possible by optical means, and, although there are limitations of definition, no other method is available for the direct viewing of sub-microscopic particles.

The *ultra-centrifuge* separates colloidal bodies into fractions for separate examination.

Infra-red and ultra-violet photography are capable of revealing marks which are otherwise invisible, so that erasures and alterations can be detected.

A variety of *physical instruments*, incorporating such equipment as radio valves, electron tubes and glass electrodes, facilitate measurements and give accuracies not attainable by other means.

Analytical chemistry employs the discoveries of most other sciences to supplement and complement the information obtained by purely chemical means.

A SURVEY OF SOME RECENT ADVANCES IN COMPUTING DEVICES

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This paper is intended to provide those interested in the design and use of computing devices with a concise survey of present-day trends in this field. The survey is not claimed to be comprehensive : limitations of space make it inevitable that many interesting and important devices have been omitted. Thus, in dealing with computers for X-ray crystal structure work, no mention has been made of the Fly's Eye technique, with which the names of Bragg, Huggins and Stokes are associated. For the most part, emphasis has been laid on those developments involving electronic and electro-mechanical principles. The author offers his apologies to those whose work has escaped mention in the survey. Almost all the information presented has been obtained from published papers. So many papers on computing devices appear every month that it should be mentioned that no papers appearing after the end of July 1948 have been included in the bibliography.

Good general accounts of the subject, with bibliographies, are given by Bush [1], Murray [2] and Hartree [3, 4]. It is well known that there are two main classes of computing devices, namely "digital," where mathematical quantities are handled directly in digital form, and "analogue," where the quantities are represented by physical quantities such as lengths, voltages, etc. Dealing first with digital machines, the most familiar examples of this class are the keyboard calculators (such as the Brunsviga), the applications of which in scientific computing have been developed over a long period. The general principles of these calculators have been described by Crew [5] and Willers [6] : Comrie [7] has surveyed their application in computing. Next must be considered the accounting machines embodying the punched card principle (Hollerith, Powers) : here we encounter the concept of the storage of information, or "memory," which plays a very important role

in the new large digital computers. Some idea of the versatility of the punched card machines can be gained from the following list of problems to which they have been applied in the last few years :— solution of ordinary linear second-order differential equations [8]; automatic integration of linear sixth-order differential equations [9]; eigen value problems [10]; molecular structure determinations [11]; boundary value problem for the potential equation [12]; numerical solution of initial-value problems [13]; tabulation of polynomials [14]; punched card tables of the exponential function [15] and of logarithms [16].

The first of the great new digital machines was the Automatic Sequence Controlled Calculator at Harvard University. This has been described by Aiken and Hopper [17]. Here the counting in the various units is performed mechanically and is controlled by electro-magnetic clutches which themselves receive operating signals according to the sequence of calculations to be performed. Next came the ENIAC, working on the same broad principles but with the counting done electronically, the various units receiving trains of pulses of 2 μsec. duration, the pulse routes through the machine being controlled by electronic gate units which are actuated by programme pulses. Several descriptions of the ENIAC have appeared [18–21]. Digital operations can also be effected by means of relays, and it is not surprising to find that the pioneer American work in relay computers was done by the Bell Telephone Laboratories. In the B.T.L. Relay Interpolator [22] use is made of the biquinary scale in which a number (from 0 to 9) is represented by either 0 or 5 in association with one of the numbers 0, 1, 2, 3, 4. With every digit of a number is associated a group of seven relays, two of which will be energised, and the expression of the digit is the appearance of earths on two out of seven cores. Here we may remark that the need for the Relay Interpolator arose from the computational work incidental to the testing of another computing device—an analogue machine for the control of anti-aircraft gunfire, which will be mentioned later. Descriptions are available of two other B.T.L. relay computers—a ballistic computer [23] and a computer for general application [24]. In England war-time work was done on relay computers for the calculation of auto-correlation coefficients, and Booth has built a relay computer for the British Rubber Producers Research Association. The biquinary scale has already been mentioned as an instance of a departure from the conventional scale of ten in order to obtain a simpler working system for a digital machine. The simplest scale of all is the scale of two in which each digit of a number can only be either 0 or 1 and is

represented either by the absence (0) or the presence (1) of a signal. Baker [25] has given a good account of the principles of computing in this scale, using relays and uniselectors.

At the present time interest in the field of digital machines is concentrated on high-speed electronic computing circuits and the associated problem of providing adequate memory facilities. One memory method involves the propagation of trains of ultrasonic pulses along mercury columns. This technique is being applied in the EDSAC which is under construction at the University of Cambridge and has been described by Wilkes and Renwick [26]. Another method, due to F. C. Williams, is based on secondary emission phenomena occurring on a cathode ray tube screen: ingenious circuit arrangements result in the number to be stored being written on the screen as a pattern of luminous spots. This has been described at a Royal Society discussion [27]. The largest memory capacity in any existing calculator is said to be that possessed by the Selective Sequence Electronic Calculator built by the International Business Machines Corporation [28]. Here four different kinds of memory are utilised—electronic storage, relay storage, the punching of continuous tapes and the punching of cards. The combined storage capacity is 400,000 digits.

Turning now to analogue computers, we find that most of the development here has been in the direction of electronic and electromechanical methods, which is to be expected, since electrical techniques are so versatile. In particular, the application of negative feedback in all its diverse forms is encountered in many recent analogue computers. Mynall [29] and Korn [30] have dealt with the general principles of analogue computing. An excellent summary of electrical computation methods is given by Williams and Ritson [31]. Considering now purely electronic computers, there has been much development of electronic integrators, the basic circuit being the well-known Miller integrator. General principles have been expounded by Korn [32] and Ragazzini *et al.* [33]: several complete electronic differential analysers of varying degree of complexity have been described [34–38]. Electronic differentiation is dealt with by Schmitt and Tolles [39]. It should be remembered that in electronic integration, the independent variable is current time and this imposes limitations not present in mechanical differential analysers using integrators of the wheel-and-disk variety. From integration we pass to function generators in electronic computers. A very elegant arrangement for producing a voltage V_1 as a given function of another voltage V_2 (*i.e.* $V_1 = f(V_2)$) was developed independently by MacKay [40] and Mynall [41]. Here

a mask is placed on a cathode ray tube screen having a profile representing the required function f. A photo-electric cell receives light from the spot on the screen when the spot is not obscured by the mask. The cell is connected to an amplifier whose output provides the vertical deflecting voltage in the CRT. The whole constitutes an electronic servo which constrains the spot to follow the mask profile. Thus if the horizontal deflecting voltage is used to represent V_2, the vertical voltage becomes $f(V_2)$, which is the required function V_1.

Electro-mechanical analogue computing methods have undergone considerable development in recent years. The electro-mechanical attack has much to commend it since the fundamental operations are quite easily performed. Thus multiplication $A \times B$ can be effected by applying A as the rotation of a potentiometer shaft, a voltage representing B being applied across the potentiometer. The voltage at the potentiometer slider is then representative of $A \times B$. Several voltages $e_1, e_2, e_3, \ldots$ can be added together by connecting them to a star point, each through a high resistance. The star point is taken to the input of a high gain amplifier and a high degree of negative feedback from output to input of the amplifier is applied. The output of the amplifier is then very nearly $-(e_1 + e_2 + e_3 + \ldots)$, the accuracy depending on the amplifier gain and the degree of feedback employed. Only in the crudest computers are voltages added by putting the sources in series, since stray reactances to ground introduce undesirable effects. In some cases, of course, the sources could not in any event be put in series, since they have a common terminal (generally grounded). Then again automatic operation can be secured by the use of servo-mechanisms. Thus if it is required to translate a voltage into a mechanical motion, a servo motor can be used to drive a resetting potentiometer, whose output is opposed to this voltage, the difference between them being taken to an amplifier which is connected to the motor. A comprehensive account of electro-mechanical analogue computers is contained in a volume of the M.I.T. Radiation Laboratory series [42]. The subject of servo-mechanism design is intimately connected with that of automatic computing: amongst many papers on servos, we mention specially the Proceedings of the I.E.E. Convention on servomechanisms [43], where much useful information on this and allied subjects, such as data-transmission systems, is included. Mention has already been made of the analogue computer developed by the Bell Telephone Laboratories for anti-aircraft gunfire control. This computer saw considerable service during the recent war both with the American and British armies. To the Americans it was known

as the M9, to the British as the No. 10 Predictor, and accounts of its general principles have been given [44, 45]. In this computer, d.c. is used in the computing circuits, the voltage scale being 200 yards per volt. Electronic differentiation in R.-C. circuits is used to derive the rates of change of target position along three cartesian co-ordinate axes, a procedure essential for the computation of the future position of the target. A parallel development in England was that of the No. 9 Predictor [45], which however uses a.c. in the computing circuits. Unlike the B.T.L. computer, which employs resistance potentiometers of large physical size to achieve high accuracy, the No. 9 uses precise auto-transformers (Ipots) which have the advantage of being low impedance sources. The production of sine and cosine terms in the B.T.L. computer is effected by means of graded resistance potentiometers: the No. 9 Predictor uses a goniometer arrangement of rotor and stator windings (Magslip Resolver). Details of the Ipot and Resolver have been published [46]. The construction of resistance potentiometers and methods of reducing errors caused by loading them are dealt with by Nettleton and Dole [47, 48]. Hofstadter [49] has described a simple potentiometer circuit for the tangent function: precision resistance networks for computer circuits are considered by Hagemann [50]. A compact analogue computer has been described by Frost [51].

Multiplication is, as we have seen, quite easily done electromechanically. When several quantities are to be multiplied together, the use of bridged-T attenuators has been advocated by Shannon [52], the quantities being set in on the shafts of the potentiometers in the attenuator circuit. Purely electronic multiplication is not so readily achieved, but there is no lack of ingenious circuits. Thus the mark/space ratio of a square wave may be made to depend on one voltage X and its peak-to-peak amplitude on another voltage Y. If the wave is fed to a suitable detector circuit, an output is obtained proportional to XY [35]. To form the product XYZ, Hardy and Dench [53] employ three pulse generators the repetition frequencies of which are irrationally related. The pulse durations are made proportional to X, Y, Z respectively and the three pulse signals are applied to the grid of a valve suitably biassed off. The mean anode current of the valve is then proportional to XYZ. This technique is somewhat reminiscent of coincidence counting in nuclear physics.

For many years the mechanical differential analyser (first developed by Bush in 1931) has been probably the best-known example of analogue computing. A good account of the differential analyser and its applications has been given by Crank [54]. At the

present time, improvements in the differential analyser are being made by the incorporation of various technical advances. Thus the inter-connection of the integrator units is greatly facilitated by the use of servo-mechanisms and data-transmission systems. The (American) G.E.C. differential analyser contains 14 integrators in which a polarised-light servo system has been applied for reproducing the integrator output at a suitable power level [55]. Bush has described his latest analyser, in which the working accuracy has been increased considerably [56]. By the use of servos and data-transmission systems, the laborious task of selecting gear ratios and dealing with many shafts can be replaced by that of plugging in connections on a control panel [57]. For accounts of differential analyser applications, see Hartree [58] and Maginniss [59]. Even in this field we cannot avoid feedback, and Amble [60] has described regenerative integrator connections which greatly assist in function generation.

Another field which appears to be very attractive to computer designers is that of the solution of simultaneous linear equations. In 1933 Mallock [61] constructed an equation solver in which the variables were represented by alternating magnetic fluxes in transformers, coefficients being set in as the number of turns in the windings on these transformers. By connecting various windings in series, the fluxes adjusted themselves automatically so as to satisfy the set of equations. Nowadays the emphasis is on circuits where the coefficients are set in on potentiometers, null conditions being achieved either manually or automatically. Thus Berry *et al.* [62] and Frost and Tamres [63] adjust each of the variables in turn manually, *i.e.* they follow the Gaus-Siedel iteration method. Adcock [64] uses a feedback method: to solve

$$\sum_{j=1}^{n} a_{ij}x_j - b_i = 0 \qquad (i = 1, 2, \ldots n)$$

he starts with

$$\sum_{j=1}^{n} a_{ij}x_j - b_i = \varepsilon_i \qquad (i = 1, 2, \ldots n)$$

where the ε's are error voltages. X_j is then expressed as

$$-\sum_{i=1}^{n} E_i a_{ij}$$

where E_i is defined by $\dfrac{dE_i}{dt} = K\varepsilon_i$. The error voltage is taken to an amplifier feeding a motor, the speed of which is approximately

proportional to the input signal. The motor is geared to a potentiometer, the slider voltage of which is then representative of E_i. In all this it should be noted that the x_j's and E_i's are d.c. voltages and the a_{ij}'s and b_i's are potentiometer settings, unit voltages being applied to the b_i potentiometers. Goldberg and Brown [65] have built an equation solver which uses as many amplifiers as there are variables. To solve

$$\sum_{j=1}^{n} a_{ij}x_j - b_i = 0 \qquad (i = 1, 2, \ldots n)$$

each amplifier output x_j feeds potentiometers on the sliders of which the coefficients a_{ij} are set. The constant terms b_i are set in on potentiometers fed with a constant voltage. The slider of a potentiometer b_m is taken to the input terminal of the corresponding amplifier (through a high resistance), the sliders of the appropriate coefficient potentiometers $a_{m1}, a_{m2}, a_{m3} \ldots a_{mm} \ldots a_{mn}$ being similarly connected to this terminal. When equilibrium is attained, the output voltages of the amplifiers represent the desired solutions of the set of equations. As in Mallock's instrument, the solution is obtained automatically without moving parts (except for the initial setting of the coefficient potentiometers), but the stability of the whole system must be considered carefully. The method of multiplication devised by Hardy and Dench [53] is incidental in the design of an equation solver.

An equation of the form $\frac{a}{b} = \frac{c}{d}$ can be solved by making three arms of a Wheatstone bridge represent a, b and c, say. If the fourth arm is adjusted so as to balance the bridge, then it represents d. Ergen [66] has described computers of this type, and Matthews [67] has designed a bridge computor for the solution of spherical triangles in celestial navigation, the accuracy attainable in his instrument being within $\pm \frac{1}{2}'$ of arc in the calculation of zenith distance. From bridge computers we turn to the network computers in which an electrical network is set up, the equation of which will correspond formally to the equation which is to be solved. Thus Hughes and Wilson [68] solve secular equations with a network supplied with a voltage of fixed frequency. A ganged set of variable condensers varies admittances in different parts of the network simultaneously and solutions are obtained by adjusting the condensers until the voltage at a network junction point is a maximum. On the other hand, Many and Meiboom [69] determine eigenvalues and eigenvectors of a real symmetric matrix by means of a network in which the frequency of the energising voltage is varied. Depackh [70]

has described a network for the approximate solution of Laplace's equation.

Finally we consider those computers in which the summation of a number of harmonic terms is called for. The calculation of crystal structure from the results of X-ray analysis requires the performance of this kind of computation (Fourier Synthesis), and for many years X-ray crystallographers have invoked the aid of the Beevers-Lipson strips. Several computers for use in X-ray crystal structure analysis have been described.* Fourier synthesisers include those of MacEwan and Beevers [71] (using uniselectors and relays), Hägg and Laurent [72] (potentiometers, transformer with sine-graded taps, special coupling switches) and Tolstov [73]. Maxwell [74] has given a bibliography of earlier work on harmonic analysers and synthesisers. Structure factor calculations are effected in a machine due to Evans and Peiser [75]. Booth [76] describes a tetragonal space-group calculator for effecting

$$F(hkl) = \sum_r f_r \left\{ \cos 2\pi \frac{h}{a} x_r \cos 2\pi \frac{k}{b} y_r - \cos 2\pi \frac{k}{a} x_r \cos 2\pi \frac{h}{b} y_r \right\} \cos 2\pi \frac{l}{c} z_r$$

and an adding machine for evaluating

$$\sum_r f_r \left\{ \begin{matrix} \sin \\ \cos \end{matrix} \right\} 2\pi x_r.$$

Another important requirement in X-ray crystal work is the making of Fourier projections of crystal structure. A machine which produces the Fourier projection as a sand pattern has been described by McLachlan and Champaygne [77]. Pepinsky [78] forms the interference fringes, corresponding to the projection of the crystal structure on the (hko) plane, on a cathode ray tube screen. A slow vertical sweep frequency (f_V, = 1 c.p.s.) and a fast horizontal sweep frequency (f_H, = 1000 c.p.s.) are applied to the deflecting plates of the tube and the grid of the tube receives a signal of amplitude $\propto F_{hko}$ and of frequency $f_{hko} = hf_V + kf_H$. The resulting pattern on the screen is the desired projection. Pepinsky's derivation of f_{hko} is ingenious: a signal of frequency kf_H is produced from an oscillator locked to a master oscillator on frequency f_H. This signal is fed to an integrating feedback amplifier which therefore produces an output signal with a 90° phase shift. This 90° shifted signal and the original signal then go to the stator windings of a two-phase Selsyn machine and so produce in it a magnetic field rotating at kf_H c.p.s. If now the Selsyn rotor is turned at hf_V c.p.s. (a small speed) then its output is a signal of the desired frequency. Thus

* The whole subject of Fourier technique is discussed in a recent book by Booth [90].

is solved the problem of the accurate production of a frequency differing from a given frequency by a small amount. A photo-electric Fourier transformer has been described by Born *et al.* [79]: Brown and Lyttleton [80] apply the same principle to the harmonic analysis of sound recorded on film. Photo-electric technique features again in the rotary periodograph of Moncrieff-Yeates [81].

Since the equation

$$a_n x^n + a_{n-1} x^{n-1} + \ldots + a_1 x + a_0 = 0 \qquad (a_k \text{ is real})$$

can be rewritten in the form

$$a_n \mathrm{M}^n \sin(\omega t + n\theta) + a_{n-1}\mathrm{M}^{n-1} \sin(\omega t + \overline{n-1}\theta) + \ldots + a_1 \sin(\omega t + \theta) + a_0 \sin \omega t = 0$$

it is seen that computers for the solution of algebraic equations also require the combination of harmonic terms. Herr and Graham [82] describe a solver using a.c. generators, the terms $n\,\theta, \overline{n-1}\theta, \ldots \theta$ being set in by moving the stators which are geared together suitably. Brown and Wheeler [83] solve polynomials by graphing them in the complex plane, using a mechanical synthesiser with 15 sine and 15 cosine components: they describe applications of their machine (solution of trigonometric and certain types of transcendental equations, double summations in Patterson contours) in a second paper [84]. In the computation of the polar diagrams of aerial arrays combination of vectors of differing phase and amplitude are required. Many computers for this purpose have been described recently [85–89].

REFERENCES

1. V. Bush, *Bull. Amer. Math. Soc.*, **42**, 649, 1936.
2. F. J. Murray, *The Theory of Mathematical Machines*, revised edn., King's Crown Press, New York, 1948.
3. D. R. Hartree, *J. Sci. Instrum.*, **24**, 172, 1947.
4. — — *Calculating Machines: Recent and Prospective Developments*, Cambridge University Press, 1947.
5. E. W. Crew, *Students' Quarterly J. Instn. Elect. Engrs*, **12**, 16, 1941, and *The Engineer*, London, **172**, 438, 1941.
6. F. A. Willers, *Arch. techn. Messen*, No. 103, J081-1, Jan. 1940, and No. 105, J081-2, March 1940.
7. L. J. Comrie, *J. Sci. Instrum.*, **21**, 129, 1944.
8. L. Feinstein and M. Schwarzschild, *Rev. Sci. Instrum.*, **12**, 405, 1941.
9. L. F. Hausman and M. Schwarzschild, *ibid.*, **18**, 877, 1947.
10. G. Kind-Schaad, *Schweiz. Arch. angew. Wiss. Tech.*, **13**, 161, 1947.
11. P. A. Schaffer, V. Schomaker and L. Pauling, *J. Chem. Phys.*, **14**, 648, 1946.
12. M. Kormes, *Rev. Sci. Instrum.*, **14**, 118 and 248, 1943.
13. J. P. Kormes and M. Kormes, *ibid.*, **16**, 7, 1945.

14. J. C. McPherson, *Ann. Math. Statist.*, **12**, 317, 1941.
15. G. W. King, *Rev. Sci. Instrum.*, **15**, 349, 1944.
16. G. B. Thomas and G. W. King, *ibid.*, **15**, 350, 1944.
17. H. H. Aiken and G. M. Hopper, *Elect. Engng*, New York, **65**, 384, 449, and 522, 1946.
18. A. W. Burks, *Proc. Inst. Radio Engrs*, **35**, 756, 1947.
19. D. R. Hartree, *Nature*, **158**, 500, 1946.
20. H. H. Goldstine and A. Goldstine, *Math. Tables and Aids to Computation*, **2**, 97, 1946.
21. M. V. Wilkes, *Electronic Engng*, **19**, 105, 1947.
22. O. Cesareo, *Bell. Lab. Rec.*, **24**, 457, 1946.
23. J. Juley, *ibid.*, **25**, 5, 1947.
24. S. B. Williams, *ibid.*, **25**, 49, 1947.
25. G. T. Baker, *P.O. Elect. Engrs' J.*, **39**, 58, 1946.
26. M. V. Wilkes and W. Renwick, *Electronic Engng*, **20**, 208, 1948.
27. Reported in *Nature*, **161**, 712, 1948.
28. *Electronics*, **21**, 138, April 1948.
29. D. J. Mynall, *Electronic Engng*, **19**, 178, 214, 259 and 283, 1947.
30. G. A. Korn, *Electronics*, **21**, 122, April 1948.
31. F. C. Williams and F. J. U. Ritson, *J. Instn. Elect. Engrs*, **94**, Pt. IIA, 112, 1947.
32. G. A. Korn, *Electronics*, **21**, 124, May 1948.
33. J. R. Ragazzini, R. H. Randall and F. A. Russell, *Proc. Inst. Radio Engrs*, **35**, 444, 1947.
34. J. S. Koehler, *J. Appl. Phys.*, **19**, 148, 1948.
35. Royal Aircraft Establishment exhibit, *Physical Society 32nd Exhibition Catalogue*, p. 279, 1948.
36. Y. G. Tolstov, *Bull. Acad. Sci. U.R.S.S., Dep. Sci. Tech.*, No. 3, 319, 1947.
37. R. N. Varney, *Rev. Sci. Instrum.*, **13**, 10, 1942.
38. L. I. Gutenmacher, *Bull. Acad. Sci. U.R.S.S., Dep. Sci. Tech.*, No. 5, 511, 1947.
39. O. H. Schmitt and W. E. Tolles, *Rev. Sci. Instrum.*, **13**, 115, 1942.
40. D. M. MacKay, *Nature*, **159**, 406, 1947.
41. D. J. Mynall, *ibid.*, **159**, 743, 1947.
42. M. I. T. Radiation Laboratory series, Vol. 21 : *Electronic Instruments*, McGraw-Hill Book Co., 1948.
43. *J. Instn. Elect. Engrs*, **94**, Pt. IIA, Nos. 1 and 2, 1947.
44. C. A. Lovell, *Bell Lab. Rec.*, **25**, 114, 1947.
45. E. J. H. Douch, *J. Instn. Elect. Engrs*, **94**, Pt. IIA, 177, 1947. See also British Patent Spec. 579,325.
46. *Physical Society 32nd Exhibition Catalogue*, p. 168, 1948.
47. L. A. Nettleton and F. E. Dole, *Rev. Sci. Instrum.*, **17**, 356, 1946.
48. —— *ibid.*, **18**, 332, 1947.
49. R. Hofstadter, *ibid.*, **17**, 298, 1946.
50. E. C. Hagemann, *Bell Lab. Rec.*, **24**, 445, 1946.
51. S. Frost, *Electronics*, **21**, 116, July 1948.
52. W. Shannon, *ibid.*, **19**, 110, August 1946.
53. A. C. Hardy and E. C. Dench, *J. Opt. Soc. Amer.*, **38**, 308, 1948.
54. J. Crank, *The Differential Analyser*, Longmans, Green & Co., 1947.
55. T. M. Berry, *Trans. Amer. Inst. Elect. Engrs*, **63**, 195, 1944.
56. V. Bush and S. H. Caldwell, *J. Franklin Inst.*, **240**, 255, 1945.

57. *Physical Society 32nd Exhibition Catalogue*, p. 270, 1948.
58. D. R. Hartree, *J. Instn. Elect. Engrs*, **90**, Pt. I, 435, 1943.
59. F. J. Maginniss, *Gen. Elect. Rev.*, **48**, 54, 1945.
60. O. Amblæ, *J. Sci. Instrum.*, **23**, 284, 1946.
61. R. R. M. Mallock, *Proc. Roy. Soc. A.*, **140**, 457, 1933.
62. C. E. Berry *et al.*, *J. Appl. Phys.*, **17**, 262, 1946.
63. A. A. Frost and M. Tamres, *J. Chem. Phys.*, **15**, 383, 1947.
64. W. A. Adcock, *Rev. Sci. Instrum.*, **19**, 181, 1948.
65. E. A. Goldberg and G. W. Brown, *J. Appl. Phys.*, **19**, 339, 1948.
66. W. K. Ergen, *Rev. Sci. Instrum.*, **18**, 564, 1947.
67. B. H. C. Matthews, *Proc. Roy. Soc. A.*, **185**, 241, 1946.
68. R. H. Hughes and E. B. Wilson, *Rev. Sci. Instrum.*, **18**, 103, 1947.
69. A. Many and S. Meiboom, *ibid.*, **18**, 831, 1947.
70. D. C. Depackh, *ibid.*, **18**, 798, 1947.
71. D. MacEwan and C. A. Beevers, *J. Sci. Instrum.*, **19**, 150, 1942.
72. G. Hagg and T. Laurent, *ibid.*, **23**, 155, 1946.
73. Y. G. Tolstov, *Bull. Acad. Sci. U.R.S.S., Dep. Sci. Tech.*, No. 3, 389, 1946.
74. L. R. Maxwell, *Rev. Sci. Instrum.*, **11**, 47, 1940.
75. R. C. Evans and H. S. Peiser, *Proc. Phys. Soc.*, **54**, 457, 1942.
76. A. D. Booth, *J. Appl. Phys.*, **18**, 664, 1947.
77. D. McLachlan, Jr., and E. F. Champaygne, *ibid.*, **17**, 1006, 1946.
78. R. Pepinsky, *ibid.*, **18**, 601, 1947.
79. M. Born, R. Fürth and R. W. Pringle, *Nature*, **156**, 756, 1945. Fürth and Pringle, *Phil. Mag.*, **37**, 1, 1946.
80. D. Brown and J. W. Lyttleton, *Nature*, **160**, 709, 1947.
81. G. B. Moncrieff-Yeates, *J. Sci. Instrum.*, **24**, 35, 1947.
82. D. L. Herr and R. S. Graham, *Rev. Sci. Instrum.*, **9**, 310, 1938.
83. S. L. Brown and L. L. Wheeler, *J. Franklin. Inst.*, **231**, 223, 1941.
84. —— *J. Appl. Phys.*, **14**, 30, 1943.
85. C. E. Smith and E. L. Gove, *Trans. Amer. Inst. Elect. Engrs*, **62**, 78, 1943.
86. C. Snowdon, *Electronic Engng*, **17**, 146, 1944.
87. H. A. Ray, Jr., *Proc. Inst. Radio Engrs*, **34**, 898, 1946.
88. G. H. Brown and W. C. Morrison, *ibid.*, **34**, 992, 1946.
89. Reference 35, p. 280.
90. A. D. Booth, *Fourier Technique in X-ray Organic Structure Analysis*, Cambridge University Press, 1948.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By A. HUNTER, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

IF there had been no World War II, astronomers from all countries would have attended the customary triennial conference of the International Astronomical Union at Zürich in 1941. As it was, no such meeting could be held, and the postponed General Assembly of the Union there last August was the first occasion on which most of them had met their foreign colleagues for ten years. Not that international collaboration had ceased meanwhile : throughout the war, news of astronomy in Britain and America was filtering into Germany, Italy and the occupied countries through monthly news-letters emanating from Harvard, whilst German astronomical periodicals were reaching the Western allies in an intermittent trickle through neutral countries. The Central Bureau for Astronomical Telegrams at Copenhagen, which gives speedy world-wide circulation to news of comets, novæ, etc., was enabled to function even after 1940 by the good offices of Swiss and Swedish astronomers. Other essential work of the Union, normally carried out by its General Secretary, Prof. Oort, who was immured in the occupied Netherlands, was performed by prominent astronomers in countries more fortunately situated. As a result of this maintenance of contacts, it proved possible to hold a restricted meeting of the Union as soon after the cessation of hostilities as March 1946. At this Copenhagen meeting solutions were found for the most urgent problems facing astronomers in restarting those activities that the war had interrupted and in extending others that had been restricted ; and the foundations were laid for a plenary Assembly in 1948.

Much of the work done at Zürich in August was of an administrative character and is unsuited for reporting in these pages. But a delegate attending the commission meetings and the many colloquia and informal lectures could return with a clearer picture of the present state of the science than he could gather from a mere perusal, however exhaustive, of the current literature. The interplay of discussion, the presentation of differing points of view and the

linking of various approaches to the outstanding problems all contributed to a general illumination of the activity current on the frontiers of astronomy which could hardly be gained in any other way.

The constructional units of the Union are the Commissions, of which there are some 40. As the emphasis varies on different lines of research, so new commissions are added and old ones are dropped. This Assembly, for instance, has created a new commission on Radio Observations and dropped a sub-commission on stellar magnitude standards. Not all astronomical work will of course fall neatly into one or other of these pigeon-holes, and mixed meetings of allied commissions were a feature of the Zürich Assembly.

The four solar commissions, in particular, worked in close collaboration—so close that it was not felt necessary to adopt a U.S. proposal to rationalise and synthesise their activities. It is clear from experience during the present solar cycle that the course of solar activity is not yet accurately predictable, though it is possible to set wide limits to what is likely to develop in both the amplitude and the periodicity shown by the 11-year cycle. But there is no agreement on the cause of sunspots or of their associated magnetic fields. In the hope that it is the early stage of sunspot growth that determines the character of the final disturbance, the Swiss Federal Observatory is to start a thorough investigation of the typical developing spot-group.

During the past decade, interest has shifted outwards from the solar surface as it has become more and more clear that solar radiation (electromagnetic and corpuscular) is largely responsible for the control of the earth's ionosphere and, with it, of telecommunication. Loose connections traced between magnetic storms, auroral displays and sunspots directly visible in white light have been replaced by closer correlations of short-wave radio fade-outs and ionospheric storms with chromospheric flares observable only in the light of certain emission lines, notably the red line of hydrogen. Still more recently it has become recognised that the newly-discovered solar radiation in the radio region probably emanates from the corona. Before the war, surveillance by spectrohelioscopes strategically placed in longitude enabled the sun's chromosphere to be watched (given good weather) throughout the 24 hours. This state of affairs, inevitably interrupted by the war, has only just been attained once more. Now it is becoming clear, too, that cinematography of solar prominences will have much to contribute to our knowledge both of chromospheric structure and of the nature of the forces causing the extraordinary motions observed in eruptive

prominences. It is particularly unfortunate, as was pointed out at Zürich, that the four observatories equipped for making such films are within 120° of longitude of one another. The immediate need is for at least one other station—certainly in the East and preferably in the southern hemisphere. The position is no better as regards coronal observations. The coronagraph, which enables the corona to be photographed in full sunlight, can be used only at high-altitude stations where the scattering of sunlight by atmospheric particles is at a minimum ; and as yet only Europe and the U.S. have established such stations.

One development in technique may, however, improve the completeness of the watch on the chromosphere. This is the ingenious and very selective interference filter devised by M. Lyot and on public view for the first time at Zürich. By allowing interference between the ordinary and extraordinary rays in a series of quartz crystals each double the thickness of the one before, absorption is produced in a continuous spectrum in such a way that the final transmission is confined to a few narrow, widely-spaced bands whose wave-lengths can be predetermined by properly selecting the crystal thicknesses. In the example shown at Zürich the transmission was centred on Hα at 6563 Å in a band less than one angstrom unit wide. Such a device can be attached to an ordinary telescope, and since it absorbs only some 65–70 per cent. of the wanted light (though of course it rejects all the remainder) the resulting image of the sun in hydrogen light is amply bright.

Two vexed questions were thrashed out in meetings of the Commission on Solar Radiation. The first concerns the sun's general magnetic field. The unhappy history of the determination by the optical Zeeman effect of the magnitude of this field was doubtless present in every delegate's mind : the early positive results (themselves not confirmed by all those who measured the plates) could not be repeated at subsequent solar minima. Then came the work at Hamburg during the war, when Thiessen developed an elegant variation of the method in which wave-length shifts due to the field are nullified by altering the pressure in the interferometer used to detect the wave-length changes. This work appeared to confirm the earlier results at Mt. Wilson. But the Zürich meeting was disturbed to hear that in more recent work with the new apparatus the field cannot be detected. It was also announced that further observations at Mt. Wilson using a Lummer plate crossed by a grating had first confirmed the existence and approximate magnitude of the field and then failed to detect it. Delegates were left in doubt whether there had been a real diminution in the

strength of the field so that it was now undetectable, whether it was perhaps periodically masked, or whether with all the observational methods it was never more than on the edge of detectability, and that normal accidental errors produced sometimes a positive result, sometimes a negative. Too much recent theoretical work has been based on the classic value of 50 gauss as the polar intensity for the matter to be left in this unsatisfactory state, and the time seems ripe for the emergence of some new and more sensitive method for measuring the field. Much interest was therefore taken in a suggestion by Dr. Ohman that the sun's radio emission should be tested for circular polarisation at points on the earth's surface where either the northern or the southern hemisphere of the sun are covered during a partial eclipse.

The other controversy concerns the supposed variability of the so-called "solar constant." Measurement of this quantity has hitherto been practically a monopoly of the Smithsonian Astrophysical Observatory, which maintains several high-altitude stations whose function it is to make the necessary measurements with pyrheliometers and spectrobolometers. The results show variations in the total solar radiation falling on the earth which have been analysed by the Smithsonian observers into periodicities whose reality has not commanded assent from many astronomers. Two lines of investigation were proposed at Zürich: first, to standardise the observing instruments, and in particular to study the geometry of the actinometers used; and second, to try all conceivable indirect checks on the supposed variability. On this latter point it was announced that some Czech measures of the brightness of the moon showed a surprisingly high correlation with the solar constant. On the other hand, no sign has been detected of the 3–4 per cent. drop in the brightness of Uranus which would be expected if the solar constant measurements in 1943 show a real fluctuation in the sun's energy output. What is not in any doubt is that at the ends of the sun's energy distribution curve its radiation is extremely variable with time: both in the extreme ultra-violet, where ionospheric work shows that the ionising radiation increases by 130–160 per cent. from minimum to maximum of solar activity, and in the radio region, where fluctuations amounting to factors of thousands are not uncommon from day to day.

Commissions dealing with positional astronomy had perhaps less lively meetings, though some of the discussions showed that new techniques are permeating even meridian astronomy, which as the oldest branch of the science might be thought to have settled down by now. But at Pulkovo, photoelectric means have been success-

fully used for recording the transits of naked-eye stars, whilst Washington has for some time been taking micrometer and circle readings photographically so as to eliminate errors of recording by the fallible observers. Punched-card methods of computation are becoming standard where heavy repetitive reductions are involved, and the development of electronic calculators, first ENIAC and now EDVAC (Electronic Discrete Variable Computer), promises not only to make drastic reductions in the labour associated with such work but also to bring within practicable range many problems hitherto dismissed as intractable. In photographic astrometry, astronomers are now looking beyond the standard technique of measuring star positions on plates taken with wide-angle lenses covering up to 12° square in one exposure. Such plates must be reduced by reference to a network of stars of known position, and up to now it has been necessary either to make special meridian observations of fainter stars than is desirable, or else to accept a lower standard of accuracy in the places of the reference stars. Yale University Observatory is now, however, using a coarse objective grating so that bright stars whose positions are well known produce spectral images faint enough for direct comparison with the field stars. Perhaps the most revolutionary step in meridian astronomy, now being undertaken both in the U.S. and in the U.S.S.R., is to link directly the system of fundamental star positions with the extra-galactic nebulæ. When this is complete, astronomers will be for the first time in a position to relate all their observations of position to a reference network based on objects with no appreciable peculiar motions of their own.

The existing position in stellar photometry was discussed at some length. The deplorable lack of balance between the numbers of observatories in the northern and southern hemispheres produces no more striking anomalies than in this field. The present international standards of stellar brightness all lie within a few degrees of the north celestial pole and are therefore not visible at all from southern observatories. Photometric standards in the southern sky do exist, but they are so rough that a projected programme of magnitude determinations at the Cape Observatory has been delayed whilst a re-calibration of the standards is undertaken. The literature contains a tremendous mass of heterogenous material with such large differences of scale and zero point that intercomparison is a major task which cannot be undertaken except by the expert. In such circumstances the unfortunate observer who wants, perhaps, a few good magnitudes in a limited area of the sky is at a complete loss and is likely to plump for a set of unreliable values or else to

measure his own and add to the confusion. It is now proposed, however, to set up reliable magnitude sequences, near the celestial equator and in the southern sky, tied rigidly to the North Polar Sequence. The Cambridge Observatories are to relate the Harvard regions at + 15° declination to the international standards with equipment that will later be used in South Africa to link + 15° to − 45°. The use of photoelectric photometry may be expected to improve the accuracy of the linkages, though of course allowance must be made for the different sensitivity ranges of the photocell and the photographic emulsion.

That raises the whole question of the fundamentals of stellar photometry. At present the vast bulk of the work has yielded photographic and photovisual magnitudes in which the stellar radiation measured is integrated over wave-length ranges that are large (several hundreds of angstroms) and are centred at points (about 4260 Å. and 5290 Å.) that are relatively close together. With receivers other than the photographic plate it is possible to narrow the integrated ranges and to extend the base-line. Indeed, the theoretical conception of monochromatic magnitudes is not unattainable by spectrophotometric methods. But the decision at Zürich was that practical considerations must continue to prevail at present; and overwhelming among these is the fact that if the wave-length range is strictly limited the faintest stars are put out of reach. This must be regarded as intolerable whilst all the pressure is towards obtaining magnitudes for the faintest possible stars that are observable even if they do not represent precisely definable physical concepts. The best compromise that can yet be reached is to insist on the specification, with each standard in a magnitude system, of parameters expressing its colour and the extent to which line absorption affects its radiation in the relevant wave-length range.

The meetings of the Spectrophotometry Commission were notable for reports on intensive current work on the infra-red spectroscopy of celestial bodies. With a lead sulphide photo-conductive cell attached to the McDonald 82-in. telescope, Dr. Kuiper has been surveying the spectra of stars and planets in the region $0{\cdot}8$–$2{\cdot}5\mu$, and has attained resolving powers of 80 and 250 respectively. The spectrum of Venus near 2μ shows new CO_2 bands corresponding to 50 metre-atmospheres of the gas above the surface clouds. Steep absorptions in the infra-red spectra of Mars, of Saturn's rings and of the inner satellites of Saturn are attributed to ice: the spectrograms of the polar caps of Mars, for example, can be closely matched by reflection spectra from solid CO_2 covered with a thin layer of

boar-frost. The green areas on Mars show spectra resembling that of lichen more closely than the chlorophyll absorption spectrum typical of vegetation. Another discovery announced during the discussion was that of methane absorption in the earth's atmosphere. Dr. Migeotte, working with a prism and echelette grating, has found telluric bands in the solar spectrum due to CH_4 at about $3{\cdot}4\mu$; and Dr. Goldberg, with a lead sulphide cell, confirms this by his observation of a further CH_4 band at $1{\cdot}6\mu$ whose rotational structure corresponds to the temperature at 8 km. above the surface. Evidently the earth must now take its place in the ranks of the other major planets whose atmospheres show evidence of methane.

A truly international symposium on the spectral sequence and its anomalies drew contributions from half a dozen countries. Early results with the new 24/36-in. Schmidt camera at Cleveland were presented by Dr. Nassau. With a 2° objective prism and infra-red plates, M-type stars as faint as the fourteenth magnitude can be recognised, and S-type stars are picked out by a band at 7950 Å. attributed to lanthanum oxide. Improvements have been made by the French school of astrophysicists in their spectrophotometric studies of ultra-violet stellar spectra. The exact position and magnitude of the intensity discontinuity in the continuum at the head of the Balmer series can be used, as Dr. Barbier showed, as a sensitive criterion both of spectral type and of luminosity in early-type stars. Speaking on Wolf-Rayet stars, Dr. Beals pointed out that it is still quite uncertain where (if anywhere) is their proper place in the stellar sequence. If the ionisation mechanism in the extended atmospheres of these objects is similar to that in planetary nebulæ, *viz.* absorption of the ultra-violet radiation from the intensely hot nuclear star, their temperatures must range from 70,000° to 110,000° K., and the stars should be placed at the upper end of the main sequence, before even the O-type stars. But as Prof. Swings pointed out, ionisation must be caused by emission lines as well as by the continuum in the ultra-violet, and the ionisation temperatures may be quite illusory. There seems little hope of clearing up this problem until we have a better idea of the physical mechanism of Wolf-Rayet emission.

Nor are Wolf-Rayet stars alone amongst the early-type stars showing puzzling spectra suggesting extended atmospheric shells. Even amongst the apparently normal absorption-line stars there are many showing anomalous line intensities, sometimes involving all lines of a given element, sometimes only one multiplet or a few. Some of these peculiarities are due to dilution effects in the exciting radiation; others to fluorescence depending on chance wave-length

coincidences; others again to abnormal population of some excited atomic levels by an unusual spectral distribution in the exciting radiation. With so many factors operating to produce anomalous line intensities it would be premature to conclude without the strongest evidence that the peculiarities are caused by differing abundances in the elements concerned.

An important contribution by Ambarzumian, rather off the subject of the colloquium but none the less interesting, concerned his attempts to explain the observed patchiness of the Milky Way in terms of a random distribution of discrete interstellar clouds some 10 parsecs across. Many such clouds will be illuminated by neighbouring stars; the number of bright nebulæ actually observed is consistent with the view that nothing but chance proximity (no ejection mechanism, for instance) associates the interstellar clouds with early-type giant stars. If the total absorption is 0·25 magnitude in each cloud, a reasonable estimate of the spatial frequency of the clouds leads to satisfactory agreement both with star and galaxy counts at different galactic latitudes and with the observed fine structure of the interstellar absorption lines.

Questions of the greatest import were raised during meetings of the Commission on Extragalactic Nebulæ. Prof. Shapley drew attention to the possibility that the classification sequence of nebular types might be an evolutionary sequence going in the opposite sense to that usually assumed: that a galaxy might in fact pass from the spiral to the elliptical form as it ages. A disturbing flavour of Ptolemaic astronomy entered when he also pointed out that no external galaxy seems to contain such bright globular clusters as our own, by at least half a magnitude. Prof. Stebbins also caused an uneasy stir by showing that the colour indices of nebulæ seem to increase with red-shift, so that the more distant ones appear redder than their red-shifts would indicate. This is what might be expected if in the past (and we see the distant ones in the past) the nebulæ contained a larger proportion than they now do of those red supergiant stars that current theory believes to be the first to burn themselves out. But whatever may be the explanation, the point of immediate concern is the effect on nebular counts: evidently distance, red-shift and now this intrinsic reddening must be allowed for. An echo of recent controversy was heard when the theoretical cosmologists present appealed to the observers to refrain from applying to their results the rather dubious cosmological corrections introduced in the past. Such corrections, based as they are on the properties of a particular world model, beg the question by automatically excluding others from consideration. In this way

the results hoped for from the 200-in. telescope would make possible a fair test of the different theories of world structure between which the present data cannot definitely decide.

Current progress in the adjustment and testing of the 200-in. Hale telescope was described in an informal lecture by Dr. Hubble. The main point of departure from tried principles in the construction has been in the cellular structure of the mirror, and it is perhaps not surprising that the only major alteration to the design has concerned the supporting mechanisms maintaining the figure of the mirror. The addition of a lever to each of the 36 supporting pads has delayed the work by two months, but many sections of the mirror are already correct to a fifth of a wave, and the opticians are aiming at (and believe they can attain and hold) a tenth of a wave. The vibrating-string standard controlling the $\frac{1}{12}$ h.p. driving motor has proved so satisfactory that the quartz-crystal standard is now merely held as a reserve. An automatic corrector in the drive modifies the constant rate to allow for predetermined quantities of refraction and flexure. The final correction of residual errors will be done by mapping them empirically for a year or two and correcting them thereafter by introducing into the drive a cam specially cut according to the irregularities found. The light-gathering power is proving to be up to expectations: Dr. Hubble believes that the new instrument's radius of visibility will be doubled as compared with the 100-in. telescope, though much of this gain will be attributable to improved photographic emulsions.

The culmination of the week's conference was undoubtedly a symposium on the abundance of the chemical elements in the universe. This fundamental problem of astrophysics is, of course, closely related to the equally fundamental one of the mechanism of energy generation in the stars. If, as is currently believed, this latter process is essentially one of conversion of hydrogen into helium by thermonuclear reactions, the proportions of these two elements ought to vary with the age of the celestial bodies observed. Now, the most spectacular apparent differences of composition amongst the stars can nearly all be explained either by the limitations of spectroscopy in indicating abundances correctly at first glance (as when "missing" elements turn out to have principal lines in inaccessible regions of the spectrum) or else by the operation of special physical conditions (as when planets lose their light constituents by thermal escape from their weak gravitational fields). But when these effects are discounted, a truly remarkable uniformity of composition appears. The first column of the table shows logarithmic abundances in the sun derived from the work

Element	Sun	τ Scorpii	Planetary Nebulæ	Interstellar Matter
H	10·0	9·8	10·0	10·0
H^2	5·0			
He	9·30 (?)	9·1	9·3	
C	6·89	6·0	6·1	
N	7·08	6·4	7·1	
O	7·18	6·8	7·3	
Na	4·28			4·4
Mg	5·51	5·6		
Ca	4·23			5·0
K	3·20			3·6
Ti	2·96			3·3
F			4·8 (?)	
S	4·92		5·6	
Al	4·33	4·3		
Si	5·29	5·6		
Fe	4·99			

of Prof. Minnaert on curves of growth for faint Fraunhofer lines, and of Dr. Hunaerts on the frequency of occurrence of such molecules as CH, CN and OH. Beyond oxygen the abundances shown are practically identical (within the estimated uncertainty factor of 2 or 3) with those found in the earth's crust and in meteorites. (For the lighter elements the terrestrial abundances would of course be unrepresentative owing to atmospheric loss.) The second column contains the abundances in the B0 star τ Scorpii as determined by Prof. Unsöld. For this star, and indeed for all main-sequence stars, he deduces abundances that are not significantly different from the solar ones. Clearly such stars have not yet made any appreciable inroads into their hydrogen stocks—at least not into those in their atmospheres: we cannot be sure how far convective mixing makes the composition of the outer layers of a star identical with that of the deep interior.

In the third column are given revised abundances deduced by Dr. Menzel for the planetary nebulæ. Earlier estimates for these objects had given a hydrogen abundance much in excess of the solar value, but recent photographs by Dr. Baade show them to have a filamentary structure, and if the effective luminous volume is calculated from these filaments rather than from the spherical shell previously assumed, the densities involved become much higher (up to 17 times) and the hydrogen abundance is correspondingly reduced to a figure which agrees well with the other columns. Prof. Strömgren's most recent results (col. 4) on interstellar matter also show a surprising measure of agreement with the other sources, though here the recognition of large regions of space where hydrogen exists in the ionised state complicates the problem, and an

uncertainty factor of 4 cannot be excluded. Whether the broad similarity shown means that the stars have condensed out of a primæval nebula of which we observe the remnants as interstellar matter, or whether the gas clouds we see are the result of prominence activity on a gigantic scale is a problem for the future.

The first rift in the lute of universal uniformity of chemical composition came in a contribution from Mr. Hoyle, who has calculated theoretical mass-luminosity relations for stars of different mean molecular weights, *i.e.* stars showing different hydrogen contents. The main-sequence stars fit closely the curve for $H \sim 100$ per cent., but the high-luminosity stars lie scattered towards higher molecular weights. Thus many of the giants must be presumed to have burned up already a considerable part of their hydrogen, as would indeed be expected from their high rate of radiation.

Another theoretical contribution came from Prof. Klein, who endeavoured to account for the present observed abundances by postulating the primæval existence not of an all-pervading hydrogen cloud (in which no thermonuclear reactions could start to produce the heavier elements) but of hypothetical stars with nuclei of specific gravity 10^{14} and temperature 10^{10} degrees. In the interiors of such stars the present heavy elements would be produced, and as they degenerated and the present stars evolved, the physical conditions needed to modify the existing abundances of the heavy elements disappeared. Thus the present atomic proportions are fixed except in so far as nuclear reactions involving the light elements can still proceed in the interiors of existing stars.

The final paper was by Dr. Struve, chairman of the symposium. He pointed out that many peculiarities in stellar spectra, till recently attributed to abnormal abundances, can now be better explained by invoking processes such as fluorescence, selective ionisation, charge transfer, etc. Other anomalies are, however, less easily accounted for. Dr. McKellar had, for instance, shown that in certain stars the heavier carbon isotope C^{13} was twenty times more abundant relative to the lighter C^{12} than on the earth. Systematic differences between the spectra of giants in the Hyades and the Pleiades are difficult to explain unless the hydrogen content in the Hyades is less than that in the Pleiades. This would be consistent with an assumption of greater age for the Hyades: for the dwarfs would have been able to conserve their hydrogen and would not yet show any detectable differences, whereas the more spendthrift habit of the hotter stars would have produced by now a noticeable hydrogen deficiency.

Astronomy is clearly within sight of burying for ever Comte's dictum of 1835, dead since Kirchhoff identified the Fraunhofer lines in 1861 : " There are questions which for the human mind will remain for ever unknown, for example the composition of the heavenly bodies."

PHYSICS. By F. A. Vick, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

Phenomena at Very Low Temperatures—Part I.—By very low temperatures are meant those below about 4° Abs. The study of matter at these temperatures is of very great interest, particularly in relation to general theories of the properties of solids and liquids.

1.—Ten years ago there appeared in these pages (Science Progress, **33**, 517, Jan. 1939) an account of the then recent discoveries of the intriguing properties of liquid helium, in its two forms I and II. Soon afterwards the onset of war shut down work on this subject, except in a very few places, though W. H. Keesom did manage to complete his monumental work on *Helium* (Elsevier, 1942). As soon as conditions permitted, experimental and theoretical work was resumed, and papers have been appearing at an increasing rate during the last two or three years. Related topics being studied actively include superconductivity, methods of obtaining and measuring very low temperatures, and the properties of solids at these temperatures, *e.g.* magnetism.

2.—Among the properties of liquid helium which any theory should seek to explain are : the transition between He I and He II at the λ-point and the accompanying discontinuous changes of (*e.g.*) specific heat ; superfluidity and the fountain effect in He II ; the negative thermal expansion coefficient of He II ; why helium should remain a liquid at the lowest temperatures so far reached (helium freezes only at pressures above 25 atmospheres) ; the very high thermal conductivity of He II (this is apparently related to superfluidity) ; and various other more recently discovered properties. The main characteristic of superfluidity is the almost complete absence of viscosity.

3.—An early attempt to formulate a satisfactory theory of the properties of both liquid He I and II was put forward by F. London (*Phys. Rev.*, **54**, 947, 1938 ; *J. Phys. Chem.*, **43**, 49, 1939 ; *Phys. Soc. Int. Cnfce. Report*, **2**, 1, 1947), who considered that the λ-point transition might correspond to the condensation of a Bose-Einstein " gas." It will be recalled that, according to quantum statistics, particles containing an even number of protons, neutrons and electrons (having symmetrical wave functions) have their energies

distributed according to the Bose-Einstein expression, and particles with odd numbers (having anti-symmetrical wave functions) obey the Fermi-Dirac statistics. The Bose-Einstein distribution is

$$\delta n = \frac{2\pi(2m)^{3/2}}{h^3} \cdot \frac{\sqrt{\varepsilon}}{e^{(\alpha+\varepsilon/kT)} - 1}\delta\varepsilon \quad . \quad . \quad . \quad (1)$$

where δn is the number of particles which have energies between ε and $\varepsilon + \delta\varepsilon$, α is a constant, and the other symbols have their usual meanings. At sufficiently high temperatures or low pressures, expression (1) must become the ordinary Maxwell-Boltzmann distribution

$$\delta n = \frac{2n}{\sqrt{\pi}(kT)^{3/2}} \cdot \frac{\sqrt{\varepsilon}}{e^{\varepsilon/kT}}\delta\varepsilon \quad . \quad . \quad . \quad (2)$$

These two expressions become equivalent when exp. $(\alpha + \varepsilon/kT) > 1$ and then

$$\frac{nh^3}{(2\pi mkT)^{3/2}} < 1 \quad . \quad . \quad . \quad . \quad (3)$$

For the gas to become degenerate and cease to obey classical statistics the left-hand side of (3) must become approximately equal to or greater than one. This gives us a rather crude method of estimating the critical temperature T_c at which degeneracy begins, and we find that for helium it is a few degrees absolute only. F. London has made a closer estimate of the temperature of discontinuity of specific heat which accompanies the Bose-Einstein degeneracy and finds it to be about 3·1° Abs. The λ-point is, experimentally, 2·19° Abs. Of course, liquid helium is not an ideal Bose-Einstein *gas*, and one would not expect to find perfect agreement. F. London has shown that some of the properties of liquid He II are consistent with the Bose-Einstein statistics and the existence of " condensed phase " in equilibrium with a " gas," the separation between the phases being in momentum space rather than ordinary space. The condensed phase is supposed to have superfluid properties and the other the normal properties of a Bose-Einstein gas.

4.—The ideas put forward by F. London have been extended by L. Tisza into a more detailed theory of the behaviour of liquid helium (*Nature*, **141**, 913, 1938 ; *J. Phys. Radium*, (8) **1**, 164, 1940 ; *Phys. Rev.*, **72**, 838, 1947). Liquid helium below the λ-point is supposed to consist of two fluids, a " super fluid " with density ρ_s and a " normal fluid " with density ρ_n, so that the normal density is $\rho = \rho_s + \rho_n$. The interaction between the two liquids is assumed to be very small, and the whole of the superfluid to be represented by one quantum state (or a narrow range of quantum states), *i.e.* to

have nearly zero entropy. The entropy of liquid He II is thus almost entirely due to the normal fluid, which consists of thermally excited molecules. The viscosity of the superfluid is zero, but the normal fluid obeys the ordinary hydrodynamical equations, except that it is more akin to a degenerate gas than to a liquid. The viscosity of a gas increases with temperature, while that of an ordinary liquid decreases as the temperature rises. It is, therefore, significant that the viscosity of liquid helium increases with temperature (with a discontinuity at the λ-point), and even He I is not an ordinary liquid in this respect. On the basis of this mixture of two fluids, many of the observed results may be accounted for qualitatively, such as (*a*) the apparent absence of viscosity in a narrow capillary tube, due to the motion of the superfluid while the normal fluid is held back, and, by contrast, the ordinary value of viscosity obtained by an oscillating disc method, (*b*) the very high apparent thermal conductivity of He II, due to the mass movement of the superfluid, (*c*) the fountain effect, (*d*) the rapid flow of the He II over surfaces, etc. For the normal fluid, the tangential and perpendicular components of fluid velocity at a surface are both zero, whereas for the superfluid only the perpendicular component is zero, the tangential one being finite. Consider the moment of inertia of a rotating vessel containing liquid He II. The superfluid component will not take part in the rotation, so that the apparent moment of inertia will be given by

$$I = I_0 \frac{\rho_n}{\rho} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This decrease of moment of inertia has been verified experimentally by E. Andronikashvilli (*J. Phys. U.S.S.R.*, **10**, 201, 1946). His results may be expressed approximately in the form

$$\frac{\rho_n}{\rho} = \left(\frac{T}{T_0}\right)^n \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where n is about 6.

5.—Tisza's theory predicted a new phenomenon, namely that temperature differences in liquid He II should be propagated as an undamped wave motion with a characteristic velocity—the so-called "second sound." The velocity of the propagation should be

$$C = \left[- \frac{\rho_n}{\rho_s} \cdot \frac{\partial}{\partial T}\left(\frac{1}{S}\right)\right]^{-1/2} \quad . \quad . \quad . \quad (6)$$

where S is the entropy of He II, determined from measurements of specific heats.

6.—The existence of second sound was apparently forecast independently by Landau (*J. Phys. U.S.S.R.*, **5**, 71, 1941, and **8**, 1,

1944) as an outcome of some work in quantum hydrodynamics, in which quantum rules are applied to liquids. This is again a two-fluid theory, but Bose-Einstein statistics is not specified.

7.—A different theory forms part of an interesting and important investigation by M. Born and H. S. Green into the general kinetic theory of liquids (*Nature*, **159**, 251 and 738, 1947; *Research*, **1**, 165, Jan. 1948; *Proc. Roy. Soc.*, **A.194**, 244, Aug. 1948, and the papers cited therein). Green believes that the quantised theory not only applies to liquid helium, but also to the electrons in a metal, including superconductivity. Tisza's theory is not directly applicable to electrons, because the latter obey Fermi-Dirac statistics. Quantum laws are applied to a system of closely interacting particles to obtain equations governing the behaviour of liquids. In most cases these equations are the classical ones, but they deviate in important respects in extreme conditions. For example, near absolute zero the de Broglie wavelengths of molecules are large, because of the very small velocities, and practically no scattering takes place. Hence all phenomena dependent on molecular collisions should become insignificant.

8.—The prediction of "second sound" naturally stimulated experimental work. The existence of such waves has been proved by V. Peshkov (*J. Phys. U.S.S.R.*, **8**, 381, 1944; *Phys. Soc. Int. Cnfce. Report*, **2**, 19, 1947). An alternating current of angular frequency $\omega/2$ was passed through a flat heater of low thermal capacity. Then the heat generated is given by

$$W = RI_0^2 \cos^2 \tfrac{1}{2}\omega t = \tfrac{1}{2} RI_0(1 + \cos \omega t) \quad . \quad . \quad (7)$$

According to the theories of Tisza and Landau, thermal waves of frequency ω will be propagated through liquid He II, and will not be damped out at a short distance from the heater. A plane resistance thermometer having rapid response was placed in the liquid parallel to the heater, and its distance from the latter varied. Various refinements were adopted to increase the sensitivity of the method, including the use of standing waves and resonance. The velocity of the second sound increases slightly as the temperature rises from about 1° Abs., and then abruptly falls near the λ-point. No dispersion was detected. Independent experiments have been reported by C. T. Lane, H. A. Fairbank and W. M. Fairbank (*Phys. Rev.*, **71**, 600, May 1947). A heater similar to Peshkov's was used and standing waves produced in a vertical column of liquid, the reflections occurring at the free liquid surface. If this free surface is an antinode, the temperature fluctuates at twice the frequency of the alternating current in the heater, giving rise to fluctuating evaporation from the surface, fluctuating vapour

pressure above it, and the generation of an ordinary sound wave detected by a microphone hanging in the vapour. The values of velocity obtained at various temperatures of He II agree quite well with Tisza's prediction and equation (6) above. More detailed theoretical calculations on second sound have been carried out by W. Band and L. Meyer (*Phys. Rev.*, **73**, 226, Feb. 1948), D. V. Gogate and P. D. Pathak (*Proc. Phys. Soc.*, **59**, 457, 1947) and R. B. Dingle (*Proc. Phys. Soc.*, **61**, 9, July 1948). Radar technique is now being adapted to the study of the transmission of second sound pulses through liquid helium (D. V. Osborne, *Nature*, **162**, 213, Aug. 1948). J. R. Pellam (*Phys. Rev.*, **73**, 608, March 1948) has considered the mechanism of transmission and reflection of ordinary and second sound in liquid He II.

9.—According to the theories of London and Tisza outlined above, the superfluid part of liquid He II exists because helium obeys Bose-Einstein statistics. But, in addition to He^4, there is a small proportion of He^3 in ordinary helium, and the latter isotope does not contain an even number of elementary particles, thus obeying Fermi-Dirac statistics and not Bose-Einstein. Hence, He^3 should not take part in superfluid flow if these ideas are correct. As has been indicated in a previous article in this series (SCIENCE PROGRESS, **36**, 85, Jan. 1948), this prediction has been confirmed, and it has been found possible to achieve partial separation of the isotopes (Daunt, Probst, Johnston, Aldrich and Nier, *Phys. Rev.*, **72**, 502, 1947). As these authors point out, however, the result does not prove conclusively that superflow is essentially connected with Bose-Einstein statistics, since other properties, such as B.Pt. and the λ-transition, may depend on the difference in mass. The experimental work has been extended, by Lane, Fairbank, Aldrich and Nier (*Phys. Rev.*, **73**, 256, Feb. 1948; *ibid.*, **73**, 729, April 1948) and others very recently.

Further work on liquid helium and related low-temperature phenomena will be discussed in a later article in this series.

METEOROLOGY.
By P. A. SHEPPARD, B.Sc., F.Inst.P., Imperial College of Science and Technology, London.

WIND STRUCTURE AND ATMOSPHERIC TURBULENCE

PART II—THEORETICAL STUDIES

(*Continued from* SCIENCE PROGRESS, No. 143, July 1948)

4. THE EQUATION OF DIFFUSION

THE equation of diffusion of a conservative entity s in a fluid of density ρ with a vector wind V and an isotropic coefficient of eddy

diffusion A (defined by: eddy flux of $s \equiv F = -A\nabla s$) is

$$V.\nabla s = \frac{\partial s}{\partial t} + \frac{1}{\rho}\nabla.(A\nabla s)$$

which, for two-dimensional horizontal flow, and horizontal diffusion negligible compared with advection, reduces to

$$u\frac{\partial s}{\partial x} = \frac{\partial s}{\partial t} + \frac{1}{\rho}\frac{\partial}{\partial z}\left(A\frac{\partial s}{\partial z}\right) \quad . \quad . \quad . \quad (4.1)$$

where u is the wind speed in direction x, with z the vertical distance. In the steady state $\partial s/\partial t = 0$ and

$$u\frac{\partial s}{\partial x} = \frac{1}{\rho}\frac{\partial}{\partial z}\left(A\frac{\partial s}{\partial z}\right) \quad . \quad . \quad . \quad (4.2)$$

The treatment of conditions varying both in the direction of the wind and with time is normally too intractable for mathematical manipulation, but W. G. L. Sutton (1943), in a very comprehensive discussion of the nature of the solutions of the steady-state equation when u and A vary as z^m and z^{1-m} respectively ($0 < m < 1$)—so-called conjugate power law theory, refers to solutions of the equation which may be used with some success to provide a first approximation to the vertical flux of s and the s-profile when the temporal change $\partial s/\partial t$ is slow. (See also Lettau's work discussed in this section below.) For the steady state proper Sutton constructs integrals of (4.2), which satisfy boundary conditions of the types arising in physical problems, and applies the results to the theory of evaporation into a turbulent atmosphere; his results are however applicable to the theory of heat as well as of mass transfer. Sutton's treatment is very detailed, and we shall not attempt to summarise it here, but one consequence of his analysis is very notable for meteorological problems of a synoptic scale and is worthy of mention. Various workers have given solutions of the steady-state equation using conjugate power law theory and assuming unrestricted validity of the power laws in respect of height. In reality, however, the range of validity may be restricted, *e.g.* to the depth of a boundary layer, or to the lower part of a complex boundary layer, and it is then important to know the order of magnitude of the error incurred in the simpler solution. Thus, assume the depth of atmosphere for which the power law, with $m = 1/7$, is valid to be 50 m., with u and A constant above and continuous with their respective values at 50 m. It may then be inferred from W. G. L. Sutton's treatment that the error in the calculated total flux per unit width from a horizontal surface of

dimension x_0 downwind, with uniform value of $s(= s_0)$ at $z = 0$, is, on this account, approximately 0·1 per cent. for $x_0 = 10$ km., and approximately 20 per cent. for $x_0 = 50$ km., the error being sensitive to changes in the height up to which the power laws are valid and relatively insensitive to wind speed. This implies the need for great care in establishing the range of validity of a given form of A when a solution of the diffusion equation is sought for meteorological problems of a synoptic scale. Such limitations of conjugate power law theory have been brought out in a rather different way by Booker (1948). He finds that the theory may not be used satisfactorily to interpret the observations of Craig (*cf.* Pt. I, § 3) in the lowest 1000 ft. or so of air passing from land to sea. The failure is to be ascribed to the A, z relation rather than to the u, z relation, for the solutions of 4.2 are quite insensitive to the latter which, in the immediate sense, controls only the dilution. In diffusion on such a scale the results can only be interpreted if m in A $\propto z^{1-m}$ may take values greater than as well as less than unity, *i.e.* A may decrease as well as increase with height. This is of course entirely reasonable, for, while the presence of the underlying boundary is likely to result in A increasing with height ($m < 1$) immediately above the boundary, the turbulence at greater heights is likely to decay with height to the residual, " free stream " value outside the boundary layer. Booker's analysis suggests that under statically stable conditions over the sea the decrease of A with height may set in at about 50 m.

The most physically satisfying treatment given recently of Eq. 4.2 appears to be that of Calder (1948) for turbulent flow in a dry-adiabatic atmosphere. Calder's starting-point is the *observed* profile of mean wind velocity in such an atmosphere, *viz.*

$$\frac{u}{v_*} = \frac{1}{k}\log\frac{v_* z}{\nu} + 5{\cdot}5 \qquad z > \frac{30\nu}{v_*} \quad . \quad . \quad (4.3)$$

for flow over an aerodynamically smooth surface, and

$$\frac{u}{v_*} = \frac{1}{k}\log\frac{z-d}{z_0} \qquad z > d + z_0 \quad . \quad . \quad (4.4)$$

for flow over an aerodynamically rough surface. Here v_* the " friction velocity " is written for $(\tau_0/\rho)^{\frac{1}{2}}$ of (3.1), ν is the kinematic viscosity of the medium, k is equal to 0·40, z_0 is the roughness parameter, and d, the zero-point displacement, varies with the surface roughness involved and is to be determined empirically—physical considerations restrict d to a value less than the maximum

height of the roughness elements. The experimental criteria for "smooth" and "rough" flow are

$$\frac{v_* z_0}{\nu} < 0{\cdot}13 \quad . \quad . \quad \text{smooth flow}$$

$$\frac{v_* z_0}{\nu} > 2{\cdot}5 \quad . \quad . \quad . \quad \text{rough flow}$$

there being an intermediate range of the parameter in which the flow is transitional and of no great practical interest. As already stated in § 3, the logarithmic profile for rough surfaces is very well established for the laboratory and in the field (though to what height above 10 m. is rather uncertain). The profile for smooth surfaces is also well established in the laboratory, but an aerodynamically smooth surface is a rare occurrence on land, and there are, consequently, few data by which to check it. It would, however, be very surprising if the law were not well satisfied in the atmosphere when the surface concerned meets the given requirement. In regard to the sea, Munk (1947) has given satisfying evidence, theoretical and observational, that a change in aerodynamic nature of the surface occurs in a wind, at a few metres height, of about 6·5 m. sec^{-1}. Eq. 4.3 is probably valid at lower windspeeds than this, and Eq. 4.4 at higher, though presumably with a subtractive term on the left-hand side for the mean speed of the water at the surface. The need for this correction appears to have been overlooked previously.*

Calder's treatment applies to both smooth and rough surfaces, but for simplicity we shall deal here only with the rough surface which is of greater meteorological interest.

For $s = u$, we have as the defining equation for A for momentum

$$\tau = A\frac{\partial u}{\partial z}$$

where τ is the turbulent shearing stress at height z, and A is now assumed to be independent of the nature of s. (Note, however, that u is not a conservative entity, on account of pressure force, so that the assumption of A for momentum being the same as for conservative entities is to be judged partly by the success of the theory in the interpretation of observations.) Now Calder (1939), following Ertel, has shown that the relative variation of τ is negligible in the first 30 m. or so in a dry-adiabatic atmosphere, so that

* Munk's paper had not been received when the remarks in Pt. I on the aerodynamic nature of the sea surface were written.

with τ = constant = τ_0, the surface drag, the last equation can be written

$$\frac{A}{\rho} = \frac{\tau_0/\rho}{\partial u/\partial z}$$

whence, using (4.4) to give $\partial u/\partial z$, we obtain

$$K = A/\rho = v_* k(z - d) \quad . \quad . \quad . \quad (4.5)$$

If (4.4) and (4.5) are introduced into (4.2) an intractable equation results. Calder therefore approximates the logarithmic profile by a power law,

$$\frac{u}{v_*} = q\left(\frac{z-d}{z_0}\right)^{\alpha} \qquad z > z_0 + d \quad . \quad . \quad (4.6)$$

where the values of q and α are to be taken so as to give the best fit with the more accurate Eq. (4.4) over the range of height over which diffusion is effectively confined. The corresponding expression for K is readily deduced,

$$K = Mu_1 z_1^{-\alpha} z^{1-\alpha} \quad . \quad . \quad . \quad (4.7)$$

where

$$M = \frac{z_0^{2\alpha}}{\alpha q^2},$$

$$u = u_1 \text{ at } z = z_1$$

and $(z - d)$ has been set equal to z, *i.e.* the datum plane of height has been raised to level d. Calder assumes (4.7) to be valid down to $z = 0$, not, as he explains, for any good physical reason—the conditions of flow in the immediate neighbourhood of a rough surface are very complex and can hardly be formulated as either turbulent or laminar—but because it leads to a simple mathematical treatment with results in very good agreement with observation. It may be remarked that since the drag of a rough surface arises from pressure forces associated with the breakaway of fluid at the individual roughness elements, and is not a direct consequence of viscosity, the flux at the surface, for matter and heat, as well as for momentum, must be related to the (viscosity independent) form drag. This implies that the boundary value of the flux cannot be expressed in terms only of the physical properties of the fluid and, in particular, can hardly be representable in terms of $(\partial s/\partial z)_{z=0}$ which has no physical meaning at a rough boundary.

Calder applies (4.2), using (4.6) and (4.7), to the following problems:

(i) the diffusion of matter from an infinite line source at $z = 0 = x$,

(ii) evaporation from a ground surface of finite extent downwind, with a constant value of $s(=s_0)$ at $z=0(0<x<x_0)$,

(iii) evaporation from a ground surface of such large extent that the vapour distribution can be regarded as uniform horizontally.

His solution to problem (i) for the concentration $\chi(=\rho s)$ at point (x, z) for a source of strength Q per unit length of line is

$$\chi = \frac{Q \exp\left\{-\dfrac{z^{2\alpha+1}}{M(2\alpha+1)^2 x}\right\}}{(2\alpha+1)^{\frac{1}{2\alpha+1}} M^{\frac{\alpha+1}{2\alpha+1}} \Gamma\left(\dfrac{\alpha+1}{2\alpha+1}\right) u_1 z_1^{-\alpha} x^{\frac{\alpha+1}{2\alpha+1}}} \quad . \quad . \quad (4\cdot8)$$

and the height * of the cloud is

$$H = \left\{2\cdot3M(2\alpha+1)^2 x\right\}^{\frac{1}{2\alpha+1}} \quad . \quad . \quad (4.9)$$

Note that the concentration profile $\chi/\chi_1(\chi=\chi_1$ at $z=z_1)$, and, consequently, the height of the cloud are independent of the wind speed, while χ itself is inversely proportional to wind speed at a reference level, in agreement with the observations reported in § 3. The variation of χ is inverse with x to a power less than unity, depending on the α of (4.6), while $\chi(x, z)$ decreases, that is, diffusion increases, with increasing z_0, as is also found in practice. The determination of the meteorological parameters α, q, d and z_0 or their equivalents v_*, d and z_0 is made from observations of mean wind speed at three levels over the surface above which the diffusion is to be evaluated. Calder quotes very striking agreement between calculated and observed values of concentration at distances from the source up to 1000 m., and there is no respect in which the theory is in conflict with observation. The agreement provides some evidence for the equality of K for momentum and matter in a dry-adiabatic atmosphere.

The results of the treatment for the evaporation problem (ii) above are :

(*a*) Rate of evaporation E per unit width normal to wind from $x=0$ to x_0,

$$E \propto (s_0 - s_1) M^{\frac{\alpha+1}{2\alpha+1}} u_1 z_1^{-\alpha} x_0^{\frac{\alpha+1}{2\alpha+1}} \quad . \quad . \quad (4.10)$$

(*b*) The profile of s is given by

$$\frac{s_0 - s}{s_0} \propto \Gamma\left\{\frac{z^{2\alpha+1}}{M(2\alpha+1)^2 x}, \frac{\alpha}{2\alpha+1}\right\} \qquad 0 < x \leqslant x_0 \quad (4.11)$$

* For the definition of height of a cloud see SCIENCE PROGRESS, **36**, 480, § 3.

The corresponding results for a smooth surface are in excellent agreement with laboratory data (Pasquill, 1943), and they may later prove applicable with slight modification to the sea surface. An adequate experimental test of (4.10) and (4.11) for the rough surface has not, however, been made on the meteorologically interesting scale, though the results are in agreement with small scale work involving the artificial contamination of the ground. Note that s_0, which may be very difficult if not impossible to measure directly, may be determined from observations on the profile of s using (4.11) with the relevant constant, and it may then be inserted into (4.10) in order to provide the vertical flux at the surface.

Problem (iii) above is one which may be dealt with on more exact lines, and has been so dealt with by Sverdrup and others, the relevant equation being

$$\frac{\partial}{\partial z}\left(A\frac{\partial s}{\partial z}\right) = 0$$

to which (4.4) and (4.5) may be applied. Using Calder's treatment, the local rate of evaporation per unit area, or more generally the surface flux of s, is

$$F = \rho M\alpha\left(\frac{u_1}{z_1^{\alpha}}\right)\left(\frac{s_0 - s}{z^{\alpha}}\right) \quad . \quad . \quad . \quad (4.12)$$

For $s = u$ we recover, of course, the velocity profile (4.6) with $F = \tau_0$, a consequence of taking A independent of the property s. There is, however, some doubt whether (4.12) gives a correct value for the local rate of evaporation at a saturated surface with s_0 taken from the surface temperature and with an observation of s at a fixed, *e.g.* screen, level; the doubt arising from the fact that the origin of z in equations (4.7) onward has been taken at level d. This uncertainty can, however, be resolved by eliminating s_0 from (4.12) through the use of two levels, z_1 and z_2, of observation for s, in which case

$$F = \rho M\alpha\left(\frac{u_1}{z_1^{\alpha}}\right)\left(\frac{s_1 - s_2}{z_1^{\alpha} - z_2^{\alpha}}\right)$$

and this equation is valid whether the surface is saturated or partly dry; it is in fact an appropriate method of evaluating the surface flux when the boundary condition is in doubt.

Calder's method and results are applicable to a depth of atmosphere for which the velocity profiles (4.3) or (4.4) are valid and in which τ may be assumed constant. How great a depth that may be is uncertain at present.

Frost (1946) had earlier considered some of the same problems as those dealt with by Calder, but with particular reference to the horizontal variation in temperature and humidity in advective conditions of a synoptic scale. His results are, in the main, similar in form to those of Calder, but his treatment is based on a purely intuitive expression for the mixing length (*cf.* § 3) and an unchecked power law of variation of wind with height. It is indeed well established that a power-law profile is inadequate near the surface, the exponent decreasing with height. The exponent also increases with surface roughness, while Frost assumes independence. This suggests that Frost's results, if valid for one scale of diffusion and underlying surface, will be more or less in error for other scales of diffusion and surface roughness, as is indeed indicated by his result that the decrease in the screen-level vapour pressure of air moving from our western coasts to Kew is to be accounted for by treating the intervening surface as one at which no evaporation takes place.

Another aspect of the diffusion equation which has bulked largely in the literature is the computation of values of K from observations of the diurnal variation of temperature or other entity as a function of height. In the past the treatment has commonly been to use mean hourly values, so that advection effects might be disregarded ($u(\partial s/\partial x) = 0$), to approximate the relevant equation with

$$\frac{\partial s}{\partial t} = \frac{A}{\rho}\frac{\partial^2 s}{\partial z^2} = K\frac{\partial^2 s}{\partial z^2},$$

by neglecting the term in $\partial A/\partial z$, and then to compute over rather thin layers, so as to avoid, it was hoped, serious error in the resulting A's or K's. Cowling and White (1941) have shown that this procedure is quite indefensible and may lead to very erroneous results, both as to the absolute magnitude of K at a level and the form of the K, z relation ; neglect of the temporal (diurnal) variation of K in the integration of the equation may also give incorrect and discordant results. They do, however, suggest a number of methods whereby satisfactory values of or limits for K may be deduced from the data when certain conditions are fulfilled, applying them to Johnson and Heywood's Leafield data for June days over the height range 1·2 m. to 88 m. Lettau (1941) has dealt with the same problem, criticising earlier workers' procedure, and himself uses Johnson and Heywood's data at the upper levels of observation for clear days in June, assuming A to vary linearly, but at unknown rate, with height. He gives the diurnal variation of A and $\partial A/\partial z$ at 45 m., the former ranging from about 1 to 180 g. cm.$^{-1}$ sec.$^{-1}$ and the latter from 0·1 to 3·2 g. cm.$^{-1}$ sec.$^{-1}$ m.$^{-1}$. The variation

is the same in form and phase for both, with a very sharp maximum at about noon and a broad minimum in the late-night hours. The similarity in form of the two curves is consistent with the reasonable assumption that A may be written

$$A(t, z) = p(t)\, q(z),$$

whence

$$\frac{\partial A}{\partial z} = p(t)\, \frac{\partial q(z)}{\partial z}.$$

Lettau decomposes the diurnal variation of A into harmonic and exponential components, the latter being referred to as the convexional component (of form $p_c 10^{-0\cdot10(t-11\cdot5)^2}$, where t is in hours from midnight and $p_c = 126$) as it provides the major fraction of the sharp maximum around midday, whereas the harmonic components may be mainly ascribed to the diurnal variation of wind speed, the accompanying effect of lapse-rate variation being mainly excluded. Since the data on which this analysis is based relate to rather quiet anticyclonic weather, the inferred behaviour of A and $\partial A/\partial z$ may be partly falsified by radiative flux and may otherwise lack generality due to the very light winds.

5. The Flux Equation and the Diffusivity as Function of Height near the Ground

We referred in Pt. I, § 3, to the observed diurnal variation in the form of the u, log z curve and of the corresponding behaviour of other profiles in levels near the ground. The period of the linear u, log z relation (neutral equilibrium) is then from Eq. (4.5) a period of linear K, z relation. Assuming the shearing stress to be effectively independent of height under other thermal stratifications—a quite safe assumption for unstable conditions, but one meriting examination for the stable case—the period of instability, during which the u, log z curve is convex to the u-axis, is one in which K necessarily increases more rapidly than the first power of the height, while the period of stability, with the u, log z curve concave to the u-axis, is one in which K increases less rapidly than the first power of the height—the case assumed in conjugate power law theory. These variations are readily derived from a consideration of the momentum flux equation, written in the form

$$A = \frac{\tau_0 z}{\partial u/\partial(\log z)},$$

in the light of the above u, log z relations. Similar consequences follow for other entities than momentum.

6. Diffusivity in Large-scale Flow Patterns in the Free Atmosphere

Rossby and Grimminger derived values of K of 10^9 to 10^{10} cm.^{2} sec.$^{-1}$ for the isentropic diffusion of momentum and specific humidity in broad patterns of flow in the middle troposphere. Such orders of magnitude for K have recently received support from Priestley (1946), who has shown that the isentropic diffusion of momentum from a steady zonal jet, between about 3 and 10 km. height over the British Isles, would provide reasonable values for the horizontal divergence and velocity of subsidence, in the particular situation examined, by taking $K = 5 \times 10^9$ cm.^{2} sec.$^{-1}$, the mean isentropic shear being $3{\cdot}5 \times 10^{-5}$ sec.$^{-1}$(3·5 m. sec.$^{-1}$/100 km.). If we assume K to be given in order of magnitude by the product of the effective eddy velocity with the effective "size of the eddies" we obtain for the latter a magnitude of order 100 km. for any reasonable value of eddy velocity.

7. The Heat Flux Equation

There has been a good deal of discussion recently on the form of the turbulent flux equation for heat, and on the behaviour and magnitude of the thermal diffusivity, particularly in conditions of unstable equilibrium. Ertel (1942; 1943*a*, *b*; 1944*a*, *b*) maintains that the flux of heat is determined not by the gradient of potential temperature, as for long assumed, but rather by the gradient of temperature itself. His argument is obscure, but is based on the quite obvious premise that the buoyancy forces provide a tendency for air with a positive anomaly of temperature to rise and with a negative anomaly to fall, independent of the sign of the lapse-rate of potential temperature. Alternatively, Ertel regards his result as deriving from the correlation between the vertical eddy velocity and the pressure fluctuation at any level. Prandtl (1944*a*, *b*) has combated Ertel's thesis strongly, and has produced some evidence that the effects of the buoyancy forces are mainly confined to very light winds and strong superadiabatic lapse-rates.

Priestley and Swinbank (1947) have written on the same subject, and we shall follow their treatment here. They too claim that the buoyancy forces modify the heat-flux equation in an important formal fashion, but their results are expressed rather differently from Ertel's. Like Ertel, however, they stress the significance of the reformulation of the flux equation in relation to meteorological processes on a "synoptic" scale.

In a mean horizontal flow the turbulent vertical flux of heat q at any level is given by

$$q = \rho c_p \overline{w'T'}$$

where c_p is the specific heat at constant pressure, w' the vertical eddying velocity, and T′ the departure of the temperature from the mean value T at the level, *i.e.* the temperature anomaly. Strictly, the density ρ should be included under the sign of the mean, but it is fairly certain that it may be treated as constant for the present purpose. With this reservation, the above expression for q is exact, provided that mean values of vertical velocity and temperature do exist independent of time for the time intervals relevant to the problem. If turbulence were purely mechanical in origin and an eddy came into existence,* that is, acquired a finite vertical velocity relative to the mean flow at the point, its temperature being the mean temperature at the level, the temperature anomaly T′ at vertical distance l from the level of origin due to this mass of air would be $-l(\partial T/\partial z + \Gamma)$, if no mixing occurred over the path. Here $-\partial T/\partial z$ is the prevailing lapse-rate of mean temperature, and Γ is the dry-adiabatic lapse-rate. If, however, the eddy, at the instant of formation, possessed a temperature anomaly T″, the temperature anomaly at distance l would be $-l(\partial T/\partial z + \Gamma) + T''$. Hence, the vertical flux would be

$$q = \rho c_p\left\{-\overline{w'l}\left(\frac{\partial T}{\partial z} + \Gamma\right) + \overline{w'T''}\right\} = \rho c_p\left(-\overline{w'l}\frac{T}{\theta}\frac{\partial \theta}{\partial z} + \overline{w'T''}\right)$$

where θ is potential temperature and $\overline{w'l}$ is essentially positive. The first terms of these expressions for q give a flux determined in sign by the gradient of potential temperature, the second by the nature of the correlation between w' and T″, and, if there is any overall tendency for the warmer air to rise, this flux is essentially positive, *i.e.* upwards, irrespective of the sign of $\partial\theta/\partial z$.

The "buoyancy term" $\overline{w'T''}$ is not a measurable quantity, and so its importance can only be inferred. Priestley and Swinbank make estimates of its magnitude in certain synoptic situations, using their own observations of T′, which cannot differ in order of magnitude from T″, and, where relevant, Best's observations of $\overline{|w'|}$ taken from a Taylor bi-directional vane. Quite a small correlation between w' and T″ would provide a flux in sunny weather comparable with that supposedly arising from mechanical turbu-

* This concept of an "eddy coming into existence" is one which cannot be entirely justified on physical grounds, but it is common currency in the literature and may be allowed to stand provided its limitations are realised.

lence—the term $\overline{w'l}$ is also inaccessible to direct observation. Moreover, they are thus enabled to account qualitatively for certain phenomena, *viz.* the subadiabatic lapse-rates of the mid and upper troposphere in clear non-subsiding air, the non-accumulation of heat in the layer of transition from superadiabatic to subadiabatic lapse-rate, and the slow rate of heating of the ground in a condition of anticyclonic gloom and subadiabatic lapse-rate, phenomena which are not explicable in terms of mechanical turbulence and normal mixing-length theory. Of what importance radiation, and in certain cases condensation, may be in these phenomena they do not consider, nor is it easy to say. They concede, however, that the flux of heat does take place in accordance with the gradient of potential temperature in certain conditions, such as the advective cooling of air flowing over cold ground.

The work of Ertel, and of Priestley and Swinbank, is valuable in directing the attention of meteorologists to a number of important but unsolved problems in heat flow in the atmosphere. Neither treatment is satisfying, but the work may be expected to lead to a more concentrated attack on the problem of the direct measurement of the heat flux through the fluctuations of wind and temperature and their correlation, a problem the importance of which has been stressed in Pt. I.

Before leaving the subject of the turbulent transfer of heat, reference should be made to recent papers by Montgomery *et al.* (1947) in a symposium, and by O. G. Sutton (1948), on convection. The former are largely concerned with the patterned (cellular) motion which occurs in an unstable layer, and such motion is not, properly speaking, turbulent, though it may determine the scale on which turbulence shall operate. The conditions under which these circulations occur and their forms are, therefore, of potential importance in the heat flux problem, and Woodcock and Wyman, in the papers referred to, have shown how smoke, and the "wind lanes" to be observed on a fairly calm sea, may be used to provide such information. Sutton deals with the transport of heat by convective turbulence near the ground (10 m. to 70 m.) in clear weather and light winds, *using the classical form of the heat flux equation.* He has analysed Johnson and Heywood's Leafield data for clear days in June (*cf.* Lettau, § 4) in the light of a theoretical treatment based upon buoyancy effects and the comparative constancy with height of the upward flux of heat around the midday hours when convexion is likely to be strongest. The intensity of the turbulent convexion currents is assumed, following Taylor, to be given by a balance between the rate of their dissipation into smaller eddies

and their rate of loss of potential energy. This leads to a relation between convective diffusivity K_H and the mixing length for heat l_H (defined by $T' = -l_H(\partial T/\partial z + \Gamma)$),

$$K_H^3 = \frac{g}{T} l_H^4 K_H \left(\frac{\partial T}{\partial z} + \Gamma\right) = \frac{gq}{\rho c_p T} l_H^4$$

where q is the vertical flux of heat. The Leafield data suggest $l_H \propto z^{1 \cdot 35}$, whence $K_H \propto z^{1 \cdot 8}$. Putting $q = 3 \times 10^{-3}$ cal. cm.$^{-2}$ sec.$^{-1}$ Sutton obtains $K_H = 1 \times 10^5$ and 5×10^5 cm.^{2} sec.$^{-1}$ at 20 m. and 60 m. respectively. These results are to be contrasted with those of Lettau from the same data, but bearing in mind that Lettau arbitrarily assumed a linear K, z relation.

Sutton's results depend strongly on the values of T′ provided from apparatus of large lag and designed primarily for the measurement of mean gradients. The results and the theoretical background to them cannot yet therefore be accepted without reservation. Moreover, the meteorological conditions of clear sky and light wind may impose a pattern on heat transfer which is not generally representative of an unstable atmosphere.

8. Richardson's Criterion of Turbulence

Richardson derived his criterion for the steady state of "just no turbulence" by equating the rate of working of the eddies against gravity (rate of working against buoyancy force) to the rate of production of turbulent energy by the turbulent shearing stress. This treatment lacks generality by neglecting certain sources (positive or negative) of energy, which may, indeed must under certain—*e.g.* dry-adiabatic—conditions, be important. One such term, for example, is the rate of working of the eddies against the local (fluctuating) pressure gradients. Ertel (1939, 1943*b*) and Babaud (1947) have considered the problem thermodynamically, and in his later paper Ertel has discussed the general equation of mechanical energy. While the Richardson Number, expressing the ratio of the above two rates of working, is undoubtedly of great importance in correlating the phenomena of turbulent flow in a stratified atmosphere, it cannot be expected that there should be one value only of the parameter determining the growth or decay of turbulence. Petterssen and Swinbank (1947) examined a large number of upper-air temperature and wind soundings, and found that the wind shear in the overwhelming majority of cases was less than that required to cause turbulence to grow in the associated (statically stable) lapse-rates, using the Richardson criterion which equates the coefficients K_H, K_M of turbulent transfer of heat and

momentum. Being unready to accept such a distribution of cases, they infer that the assumption of equality of the transfer coefficients is false, and suggest that K_H/K_M may have the value of about 0·6. From the above remarks, however, it will be seen that other interpretations are possible.

9. Conclusion

It is evident from the previous sections that no comprehensive theory of atmospheric turbulence begins to appear, though certain special problems have received solutions based upon empirical knowledge, *e.g.* of the observed form of the velocity profile in a boundary layer. This state of affairs is, however, not surprising since there is yet no integrated treatment of turbulence in the generally simpler (isothermal) flows studied in the laboratory and wind tunnel. The theoretical work of Tollmien and Schlichting and the related experimental work of Schubauer and Skramstad (1947)—the latter provide a good English summary of the Tollmien, Schlichting theory—on transition in flat-plate flow, promise an understanding of the growth process of turbulence, while Kolmogoroff, Batchelor and others are providing insight into the decay process. The atmospheric problem is complicated by statical stability and, perhaps, by scale, and is a problem of the free atmosphere as well as of the terrestrial boundary layer. The latter has received most attention in the past because of a number of very practical problems concerning it and because of the accessibility to observation of the layer of atmosphere near the surface. While theory plods steadily on, or while perhaps waiting for the sudden jump in physical understanding, there is much for the experimentalist to be observing, not only near the surface but in the free atmosphere also. Any potential advance in theory will quite certainly need many, so far unmade, observations of the properties of a turbulent atmosphere with which to test itself.

BIBLIOGRAPHY

Babaud, J., 1946, *La Mét.*, Ser. 4, 170.
Booker, H. G., 1948, *Quart. J. Roy. Met. Soc.*, **74** (in press).
Calder, K. L., 1939, *Quart. J. Roy. Met. Soc.*, **65**, 537.
—— 1949, *Quart. J. Mech. and App. Math.*, **12** (in press).
Cowling, T. G., and White, A., 1941, *Quart. J. Roy. Met. Soc.*, **67**, 276.
Ertel, H., 1939, *Met. Zeit.*, **56**, 109.
—— 1942, *Met. Zeit.*, **59**, 250.
—— 1943*a*, *Met. Zeit.*, **60**, 246.
—— 1943*b*, *Met. Zeit.*, **60**, 289.
—— 1944*a*, *Met. Zeit.*, **61**, 8.
—— 1944*b*, *Met. Zeit.*, **61**, 207.

FROST, R., 1946, *Proc. Roy. Soc.*, **A186**, 20.
LETTAU, H., 1941, *Beitr. z. Geophys.*, **57**, No. 2.
MONTGOMERY, R. B, *et al.*, 1947, Symposium on "Convection Patterns in the Atmosphere and Ocean," *Ann. New York Acad. Sci.*, **48**, 705–844.
MUNK, W. H., 1947, *J. Mar. Res.*, **6**, 203.
PASQUILL, F., 1943, *Proc. Roy. Soc.*, **A182**, 75.
PETTERSSEN, S., and SWINBANK, W. C., 1947, *Quart. J. Roy. Met. Soc.*, **73**, 335.
PRANDTL, L., 1944*a*, *Met. Zeit.*, **61**, 12.
—— 1944*b*, *Met. Zeit.*, **61**, 169.
PRIESTLY, C. H. B., 1946, *Quart. J. Roy. Met. Soc.*, **72**, 284.
PRIESTLY, C. H. B. and SWINBANK, W. C., 1947, *Proc. Roy. Soc.*, **A189**, 543.
SCHUBAUER, G. B., and SKRAMSTAD, H. K., 1947, *J. Res. Nat. Bur. Stand, U.S.A.*, **38**, 251.
SUTTON, O. G., 1948, *Quart. J. Roy. Met. Soc.*, **74**, 13.
SUTTON, W. G. L., 1943, *Proc. Roy. Soc.*, **A182**, 48.

GENERAL AND PHYSICAL CHEMISTRY.

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MOLECULAR INTERACTIONS IN SOLUTION.—*Hydrogen Bonds.*—In 1912, Moore and Winmill (*J. Chem. Soc.*, 1912, **101**, 1635) explained the fact that amines are relatively weak bases whereas the quaternary ammonium salts are strong electrolytes by supposing that fairly stable undissociated molecules of either types I or II can be formed

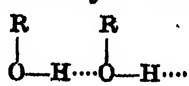
$$\underset{\text{I}}{R_3N\text{—}H\cdots OH} \qquad\qquad \underset{\text{II}}{R_3N\cdots H\text{—}OH}$$

so long as there is at least one hydrogen atom linked to the nitrogen atom in the ammonium ion. Thus they considered that in these substituted ammonium hydroxide molecules a hydrogen atom is linked to either the nitrogen atom or to the oxygen atom by primary valency forces, and to the other by secondary forces. Since that time views regarding the nature of the chemical bond have undergone considerable development, and much attention has been focussed on these so-called "Hydrogen bonds" in which one hydrogen atom serves in some way as a link between two other atoms.

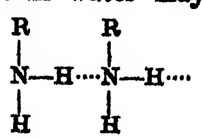
Such bonds provide the simplest explanation for the association in the liquid state of hydrogen fluoride, water, and ammonia, as well as other compounds containing hydroxyl or amino groups. These associated molecules can be written in the forms III, IV, and V. Similarly the high solubility of ammonia in water may be

$$F\text{—}H\cdots F\text{—}H\cdots$$

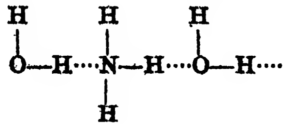
III



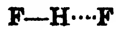
IV



attributed to the formation of mixed associated molecules, formed by hydrogen bonding, as shown in VI. These bonds similarly account for the occurrence and stability of the bifluoride ion (VII),

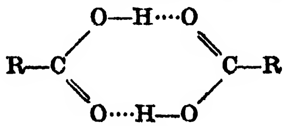


VI



VII

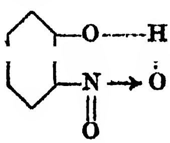
and for the fact that carboxylic acids tend to associate in solution in non-polar solvents to yield dimers of low dipole moment, the structures of which may be represented as in VIII. In aqueous



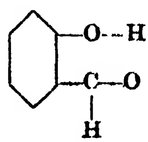
VIII

solution there will be the alternative possibility of mixed hydrogen bonding with water molecules and consequently less tendency for the formation of the dimeric molecules. This type of behaviour is now regarded as not being restricted to organic acids, but probably occurring also in other oxy-acids, thus accounting, for instance, for the complex nature of iodic and telluric acids.

Similar interaction occurs between hydroxyl groups and nitro- or aldehydic groups, leading, for example, to *p*-nitrophenol being an associated compound. On the other hand, in the case of the ortho isomers, there is a competition between the tendency towards intermolecular association of this type and intramolecular association between the groups within the single molecules by the formation of hydrogen bonds. This causes *o*-nitrophenol (IX) and salicylaldehyde (X) to be non-associated and hence much more volatile than the *m*- and *p*-compounds.



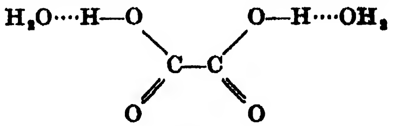
IX



X

Support for the existence of hydrogen bonds has been obtained not only from the low volatilities and high apparent molecular weights in solution arising from the association of molecules capable of forming them, but also from the inter-atomic distances between

the atoms linked by the hydrogen bond, as measured by the X-ray or electron diffraction methods. The usual interpretation of these measurements is that an approach of the atoms to within about 3·4 A. indicates a linkage between them. The shortest hydrogen bridge between two oxygen atoms (2·51 A.) occurs in oxalic acid dihydrate (XI), the oxygen atoms of this bridge being appreciably



XI

nearer than the carbonyl oxygen and the oxygen atom of the water molecule (2·83 A.). The distances between the fluorine atoms in the bifluoride ions of potassium and ammonium bifluoride are particularly short (2·26 and 2·32 A., respectively), and this reflects the stability of these ions.

The case which has been mentioned of the high solubility of ammonia in water is an extreme example of the general observation that compounds which tend to associate through intermolecular hydrogen bond formation are generally fairly soluble in solvents such as water and alcohols which contain donor groups which can compete with the donor groups of other molecules of the solute in hydrogen bond formation, whereas compounds which form intramolecular hydrogen bonds, which are much less readily broken, show a much lower solubility in these solvents.

The intensity of the interaction involved in the formation of hydrogen bonds should also be indicated by their energy values as given by the heat content change attending their formation. The available data is rather sparse, however, and their interpretation difficult. All these methods of detecting the hydrogen bond have been discussed in detail in reviews by Mansell Davies (*Ann. Rep. Chem. Soc.*, 1946, **43**, 5) and Hunter (*ibid.*, 141).

Whilst the existence of the hydrogen bond has now been established for some time, its precise structure has been the subject of considerable diversity of opinion. Although Moore and Winmill had visualised the probability that the hydrogen atom might be linked by stronger valency forces to one atom than to the other, there was some tendency to regard the atom as becoming divalent. On the electronic theory of valency this involved the expansion of the valency shell of the hydrogen from two to four electrons, much in the same manner as silicon or sulphur can expand their eight electrons shells to twelve electrons. This view, however, is quite

untenable owing to the very large amount of energy which would be needed to permit the hydrogen atom to become associated with four electrons.

With the development of the theory of resonance, another and, on the face of it, more reasonable view was to interpret the structure as compounded of the two forms XII and XIII. As, however, reson-

$$\underset{\text{XII}}{—X—H \quad Y—} \qquad\qquad \underset{\text{XIII}}{—X \quad H—Y—}$$

ance can only occur when the structures involved are such that the positions of the nuclei are almost identical, it would be anticipated that the hydrogen atom would in such cases be approximately equidistant between the two atoms X and Y. This is not the case, and it is now almost certain that if such resonance forms contribute at all to the structure their contributions are very small.

The present view is of a rather more general type, in which the bonding is attributed to an electrostatic attraction between an unshared electron pair of the donor atom and the proton, without the formation of any specific bond.

On the ground of this theory it would be expected that hydrogen bond formation would result in certain modifications in the electron distribution. Through the attraction of the electrons from Y the X—H bond should tend to be lengthened, whilst there will be a polarisation of the groupings linked to Y in such a sense that electrons are displaced towards Y. This would be expected to be reflected in a modification of the dielectric constant of the substance, or, if it is studied in solution, a modification of its apparent dipole moment.

This approach was first attempted by Earp and Glasstone (*J. Chem. Soc.*, 1935, 1710) to detect what they regarded as co-ordinate linkages. They supposed that, if compound formation in binary systems involved dipole association, the polarisation of a mixture of the two compounds would be less than would be expected from the values for the separate components, whereas the introduction of a co-ordinate link resulting from chemical union should result in a definite exaltation of the polarisation. They found that mixtures of ethers, acetone, or quinoline with halogen compounds, particularly chloroform, showed an increased molecular polarisation, which they attributed to what they called " partial co-ordination " of the hydrogen atom. Similar effects were shown, however, to a less extent, with carbon tetrachloride, where it is much more difficult to explain.

The assumptions made by Earp and Glasstone were criticised

severely, however, by Hammick, Norris, and Sutton (*J. Chem. Soc.*, 1938, 1755), who, from tests of various approaches to the matter, inferred that only from measurements made in ternary solutions in which the two reactive compounds are present in constant proportions can figures be obtained which give a true indication of the readiness of compound formation. By studying mixtures of chloroform with triethylamine, di*iso*propyl ether, and nitromethane in benzene solution, they obtained evidence that a complex is formed readily by chloroform with the amine, less readily with the ether, and not at all with the nitro-compound. These observations are in accord with the view that the compounds result from hydrogen bonding between the donor atoms of the amine or ether and the hydrogen atom of the chloroform molecule.

At an even earlier date Fairbrother had shown (*J. Chem. Soc.*, 1932, 43; 1933, 1539; *Trans. Faraday Soc.*, 1934, **30**, 862) that, whereas the molecular polarisation of hydrogen chloride in ethyl bromide or ethylene dichloride solution was approximately equal to the gas value at the same temperature, the value for hydrogen chloride in cyclohexane, or for either hydrogen chloride or hydrogen bromide in benzene or carbon tetrachloride, was more than 50 per cent. higher, indicating for hydrogen chloride an apparent dipole moment of about 1·3 D. as against 1·03 D. for the gas phase. In a theoretical analysis of the problem he deduced that the increase in moment can be accounted for, on the principle that the structure of any single bond between unlike atoms can be represented as a combination of covalent and ionic structures, by the lowering of the energy levels of the ionic state relative to the covalent state by the solvation effect of the medium (*Trans. Faraday Soc.*, 1937, **33**, 1507). In such cases, therefore, it seemed unnecessary to assume any specific formation of hydrogen bonds to account for the effects observed, but much more recently Weith, Hobbs, and Gross (*J. Amer. Chem. Soc.*, 1948, **70**, 805) have measured the apparent dipole moments of hydrogen fluoride, hydrogen chloride and hydrogen bromide in carbon tetrachloride, *n*-heptane, benzene, and dioxan. In accord with the observations of Fairbrother the values were all higher than in the gas phase, but particularly high values were observed in dioxan solution. This they compared (Table 1) with the abnormally high dipole moment of water in dioxan solution, and attributed the effect to "incipient hydronium formation" caused by the oxygen atom of the ether bonding the hydrogen of the halide by a hydrogen bond, thereby forming a complex which is stabilised by both the ionic and exchange forces.

The high apparent dipole moment shown by aniline in dioxan

TABLE I

APPARENT DIPOLE MOMENTS IN SOLUTION

Solvent.	HF (D.).	HCl (D.).	HBr (D.).	H_2O (D.).
Carbon tetrachloride	2·0	1·06	0·93	
n-Heptane	2·2	1·19	1·02	
Benzene	1·91	1·25	1·08	
1:4-Dioxan	2·34	2·12	2·85	2·64
Gas Value	1·91	1·03	0·78	1·84

solution has also been attributed to the effect of hydrogen bonding (Vassiliev and Sirkin, *Acta Physicochim. U.S.S.R.*, 1941, **14**, 414). In support of this view it has been found (Few and Smith, unpublished results) that an abnormally high molecular polarisation is also shown by methylaniline in dioxan solution, but that in the case of dimethylaniline the value is only slightly greater in dioxan than in benzene solution. The slight anomaly still shown with dimethylaniline suggests that there may be a slight tendency towards hydrogen bonding through the activated *para*-carbon atom, although hydrogen bonds between CH-groups and donor atoms are rare excepting where there are strong activating forces, as for instance in the case of chloroform. Evidence has also been obtained of a slight increase in the molecular refractions of aniline and methylaniline in dioxan solution as compared with its value for the pure liquids or for their solutions in benzene, an effect which would be anticipated on the basis of the modern picture of hydrogen bonding.

One of the most important results of hydrogen bonding in compounds such as water, ammonia, alcohols, hydrogen fluoride, and hydrogen cyanide is their high dielectric constants in the liquid state as compared with other compounds which have dipole moments of a similar order. Basically the effect arises from the low specific volumes of these liquids, which is associated in turn with the strong interaction between the hydrogen atoms and the unshared electrons of other molecules. This subject has been treated theoretically by Oster and Kirkwood (*J. Chem. Physics*, 1943, **11**, 175) on the basis of Onsager's theory (*J. Amer. Chem. Soc.*, 1936, **58**, 1486), and on certain assumptions they have been able to derive values of the dielectric constants of these liquids and their variation with temperature which are of the same order as the observed values.

Another effect of hydrogen bonding is to produce frequency shifts and intensity changes in the bands of the infra-red and Raman spectra associated with the group containing the hydrogen atom. This might be expected to yield the best method for studying the

energy changes associated with the interaction, but, unfortunately, similar but generally smaller changes of this type are brought about by other causes. Such measurements have served to support the view that hydrogen bonds exist in cases where other evidence has pointed in the same direction, but no satisfactory general relationship has been found between the change in heat content attending hydrogen bonding and these frequency changes. For the case of hydroxyl linkages, however, a rough empirical relationship has been developed by Badger and Bauer (*J. Chem. Physics*, 1937, **5**, 839; 1940, **8**, 288).

The important bearing of hydrogen bond formation on some of the problems of organic chemistry has been discussed by Hunter (*loc. cit.*), so one example only will be mentioned here as an illustration. It has been observed that alcohols which react directly with acetyl chloride to give the alkyl chloride show a completely different reaction in the presence of pyridine, when the product is the alkyl acetate (Balfe, Doughty, Kenyon, and Poplett, *J. Chem. Soc.*, 1942, 605). It has been suggested by Gerrard (*J. Chem. Soc.*, 1940, 218) that the function of the pyridine is to promote the removal of the proton from the hydroxyl group, rather than to cause ionic fission of the acid chloride, so the reaction can be interpreted as follows. In the carbinols involved, which readily undergo alkyl-oxygen fission, the R—O linkage has a considerable ionic component (XIV), and hence the carbonium cation combines with the anionic chlorine. In the presence of pyridine, however, there may be a hydrogen-bonding effect which will in turn increase the ionic character of the O—H linkage (XV). This will have the effect of diminishing or

$$\underset{+\quad -}{R—O—H} \qquad\qquad \underset{-\quad +}{R—O—H\cdots NC_5H_5}$$

XIV XV

eliminating the ionic character of the linkage between the alkyl group and the oxygen atom, thereby changing the system to one in which the acetyl group attacks the negative oxygen atom.

That there is a strong tendency for hydrogen bonding to occur between the hydroxyl group of alcohol molecules and the unshared electron pair of the pyridine molecule is supported by the observation that the apparent molecular polarisation of alcohols in benzene solution is increased considerably by the addition of small amounts of the base, the increase at constant alcohol concentration being approximately proportional to the amount of base added (Cleverdon and Smith, *Chem. and Ind.*, 1948, 29, and unpublished results). The readiness with which pyridine forms hydrogen bonds is also

emphasised by its miscibility with water and other hydroxylic solvents.

Molecular Interactions in Iodine Solutions.—It is well known that in some solvents iodine dissolves to give a distinctive purple solution, whilst in other solvents its solutions are of a reddish-brown colour. The difference has generally been attributed to the presence of complexes with the solvent in the latter solutions, which are generally associated with solvents of an oxygen-containing type, but the nature of these complexes has been little understood. Recent studies by Fairbrother have assisted very considerably, however, in throwing light on this behaviour and in correlating it with other phenomena which have been observed.

The fact that the iodine cation I^+ is capable of existence is well established. Faraday showed that liquid iodine monochloride on electrolysis yielded iodine at the cathode, and the existence of the I^+ ion in iodine monochloride in the liquid state or when dissolved in polar inorganic solvents has since been fully confirmed. Further, owing to its formal analogy with hypochlorous acid, HOI is commonly known as hypoiodous acid, but it is in fact rather in the nature of an iodous hydroxide, and as such is the active agent in the iodination of phenols. It has a higher dissociation constant as a base than as an acid.

In so far as solutions in non-polar solvents are concerned, Fairbrother found (*J. Chem. Soc.*, 1936, 847) that the molecular polarisation and apparent dipole moment of iodine monochloride are much greater in carbon tetrachloride or cyclohexane solution than in the vapour state, the dipole moment being 1·49 D. in carbon tetrachloride and 1·47 D. in cyclohexane as compared with 0·8 D. for the gas. He suggested that this increase in dipole moment was caused by an increase in the polar character of the valency bond, probably accompanied by a slight increase in the nuclear separation. The bond energy of iodine monochloride is higher than the mean of the bond energies of iodine and chlorine, indicating that the wave function representing the bond may contain ionic terms even when the gaseous state is considered. Consequently Fairbrother supposed that the polarising influences of solvation will increase the relative probability of the ionic structure, and that this increase will be the larger the greater is the polarisability of the solvent, and hence the greater the solvation energy. In carbon tetrachloride and cyclohexane solutions he considered this to reach the stage of a partial "ionisation" only. He deduced theoretically that immersion in a medium of dielectric constant 3·9 should yield electrolytic dissociation, and confirmed this by showing that iodine monochloride shows

a slight conductivity in chlorobenzene of dielectric constant 5·8 and increased conductivity in ethylene dichloride of dielectric constant 10.

By analogy with the case of iodine monochloride it might be expected that under certain conditions iodine itself might show a tendency to form I^+ and I' ions. Energy calculations indicate that only about 15 kilocalories per gram molecule more is required to convert a gaseous iodine molecule into ion pairs than to form ion pairs from a gaseous iodine monochloride molecule, which yields them fairly easily.

In this connection it was found by Waentig (*Z. physikal. Chem.*, 1909, **68**, 513) that when water was added to a solution of iodine in pyridine an unstable compound $C_5H_5NI_2$ was precipitated. Later Carlsohn prepared a series of crystalline oxy-acid salts of iodine by treating the silver or mercurous salt of the appropriate acid with a calculated amount of iodine and a slight excess of pyridine dissolved in a non-aqueous solvent. Half the iodine was precipitated as an insoluble iodide, the remainder forming the cation of the salt. Another point of evidence is the fact that a solution of iodine in nitrobenzene is non-conducting unless a trace of water is present (Brüner and Galecki, *Z. physikal. Chem.*, 1913, **84**, 513), but it yields an electrically conducting solution in dry pyridine (Audrieth and Birr, *J. Amer. Chem. Soc.*, 1933, **55**, 668).

Fairbrother (*J. Chem. Soc.*, 1948, 1051) explains this behaviour by the suggestion that the formation of the pyridine addition product results from the strongly electron-donor character of the basic nitrogen lone-pair electrons, which, as we have seen above, is evidenced by their very strong tendency to form hydrogen bonds. This electron-donor character, it is suggested, stabilises the I^+ ion, and hence it is reasonable to suppose that some polarisation may occur also in the other basic solvents which also yield brown solutions.

Atomic iodine is colourless, absorbing only radiation of wavelength less than 2300 A., but the combination of two neutral atoms to form an iodine molecule is accompanied by the development of colour. There is also an increase of refractivity, since measurements of the refractive index of iodine vapour at high temperatures indicate that atomic iodine has about the same atomic refraction as iodine in an organic iodide (13·76 c.c.), whereas the molecular refraction of the undissociated diatomic molecule of iodine is about 32·2 c.c., or 20 per cent. greater than that of the two separated atoms or of two single electron-pair bonded iodine atoms.

Both of these facts indicate that the iodine molecule is relatively easily polarised and it is probable that the ionic forms make an

appreciable contribution to the normal state of the molecule. If a second substance is present which has a sufficiently mobile electron pair, for instance a lone pair, its approach to the iodine molecule should stabilise one of the ionic structures. In the limit, if the $5p$ orbital of the acceptor atom is wholly utilised in forming the co-ordinate link, such interaction could lead to a wholly ionic structure X^{-}—I^{+} I', since this orbital is no longer available for the valency electrons of the iodine molecule. If, however, the wave function of the donor electrons is such as to overlap the $5p$ orbital of the acceptor iodine atom only to a limited extent, the iodine molecule will be polarised by the attachment to the donor molecule, *i.e.* the contribution of the ionic form will be increased. When the iodine is dissolved in a medium which is highly basic and polar some actual electrolytic dissociation may occur, the energy necessary for the ionisation of the neutral molecule being supplied by the energy of co-ordinate bond formation and solvation of the resulting ions.

According to Fairbrother's view, the form of the iodine " acceptor orbital " is labile, and the iodine molecule adjusts itself to accommodate the donor electrons, so that the strength of the solvent-solute bond will depend on the donor or basic character of the solvent. This would account for the solution becoming browner, *i.e.* the visible absorption band being displaced towards the violet, in basic solvents but being hardly displaced at all in highly polar but non-basic solvents such as nitroparaffins. That the solvent-iodine bond is often of a weak type is shown by the fact that in some cases the colour of the solution becomes more purple, or less brown, with rise of temperature.

These views have been confirmed in a convincing manner by Fairbrother as a result of dielectric polarisation measurements. Several measurements had been made previously of the molecular polarisation of iodine in various organic solvents, and in some cases the apparent dipole moments of the iodine molecule in these solvents were evaluated. Tentative suggestions were made by Williams (*Physikal. Z.*, 1929, **29**, 174) and Dewar (*J. Chem. Soc.*, 1946, 406) that there must be some degree of ionisation of iodine in benzene solution to account for its abnormal colour and dipole moment, Dewar ascribing the interaction to the partial formation of a complex with the π-electrons of the benzene ring. On the other hand, the actual data available was very conflicting, some authors reporting that the molecular polarisation of iodine in some solvents was equal to or even less than the molecular refraction, whilst others reported it as considerably greater.

Fairbrother has now repeated these measurements in a range of

solvents and finds that when iodine is dissolved in a medium of zero or low polarity, but which has bonds or groups with electron-donor character, the solution is red or brown in colour and the molecular polarisation is appreciably greater than the molecular refraction, a phenomenon attributed to a polarisation of the iodine molecule. In cyclohexane which has no electron-donor character, however, the molecular polarisation is approximately equal to the molecular refraction and the colour of the solution is purple. His results are summarised in Table II, where P_2 indicates the total molecular

TABLE II

MOLECULAR POLARISATION OF IODINE IN VARIOUS SOLVENTS

Solvent.	Colour of Solution.	P_2 (c.c.).	P_0 (c.c.).	μ (D.).
Cyclohexane	Purple	31	0	0·0
Benzene	Reddish purple	38·5	7·5	0·6
p-Xylene	Red	47	16	0·9
Cyclohexene	Red-brown	56	25	1·1
1:4-Dioxan	Brown	66·5	35·5	1·3
Di*iso*butylene	Red-brown	75	44	1·5

polarisation of the iodine, derived as the average value for a series of dilute solutions, P_0 the apparent orientation polarisation and μ the apparent dipole moment of the iodine molecule derived from these values. The polarisation increase in the solutions arises, of course, not only from electron shifts in the iodine molecule but also from electron shifts and any other changes brought about in the donor molecule. Hence all these figures are apparent values only, and, as there is no way of differentiating between the effects arising within the solute and solvent molecules, it would seem to be better to express the abnormality in terms of an " interaction polarisation," which, in the present instance, is given by the P_0 terms.

The evidence of molecular interaction in the aromatic and ethylenic solvents is of particular interest, as in these cases it seems to be most probable that π-electrons act as donor electrons, as has been suggested by Dewar (*loc. cit.*) to explain certain aromatic rearrangement and substitution reactions. This view is supported by the greater value of P_0 in the case of ethylenic solvents, where the π-electrons are less firmly bound than in benzene. Such interaction with the π-electrons may also be the explanation of the fact that the dipole moments of certain compounds are slightly higher in benzene solution than would be expected for a solvent of its dielectric constant. This is the reverse of the state of affairs which would be expected from the Raman-Krishnan-Subbaramaiya theory, as, in

view of the anisotropy of the benzene molecule, its molecular polarisation should decrease fairly rapidly with increasing dielectric constant of the solution, and hence the molecular polarisation of the solute, derived on the assumption that the value for the solvent remains constant, will be too low.

ORGANIC CHEMISTRY. By A. W. Johnson, Ph.D., A.R.C.S., The University, Cambridge.

Chromatography.—Of the methods of analysis and purification available to the organic chemist, none has undergone such marked changes in the past three or four years as chromatography. Fortunately a number of excellent reviews of the subject have been compiled recently, *e.g.* Brockmann (*Angew. Chem.*, 1947, **59**, 199); *Neuere Methoden der Organischen Chemie* (Verlag Chemie, 1943), p. 547; Strain, *Chromatographic Adsorption Analysis* (Interscience, 1942); Zechmeister and Cholnoky, *Principles and Practice of Chromatography* (Chapman & Hall, 1943); Williams, *Introduction to Chromatography* (Blackie, 1946), and special mention should be made of a recent symposium on chromatography (*Ann. N.Y. Acad. Sci.*, 1948, **49**, 141).

The story of the discovery of the technique of chromatography by Tswett (*Ber. deutsch bot. Ges.*, 1907, **24**, 316) and the subsequent latent period, finally broken by the separation of α-, β- and γ-carotenes by Kuhn and Lederer (*Ber.*, 1931, **64**, 1349) are well known to organic chemists. Since that time there have been numerous applications of the method and many recent isolations of natural products, *e.g.* the anti-pernicious anæmia factor (Lester-Smith and Parker, *Biochem. J.*, 1948, **43**, Proc., viii), would have been unthinkable without the application of chromatographic techniques. Nevertheless, the choice of suitable adsorbents and solvents is still guided largely by experience, a state of affairs which is partly brought about by the lack of commercial adsorbents of constant and guaranteed quality. In this connection, the technique of Brockmann and Schodder (*Ber.*, 1941, **74**, 73) for the grading of alumina and other adsorbents is of interest, the materials being graded according to their behaviour towards a series of standard dyestuffs: azobenzene, *p*-methoxyazobenzene, Sudan yellow, Sudan red, *p*-aminoazobenzene and *p*-hydroxyazobenzene. Alumina can be partially deactivated by the addition of small amounts of water followed by careful mixing to ensure that the product is homogeneous. On the other hand, the activity can be increased by rinsing with dilute solutions of calcium hydroxide, followed by heat treatment.

A variety of adsorbents for chromatography are now available and the following approximate list has been compiled in order of increasing activities: sucrose, starch, talc, sodium carbonate, calcium carbonate, calcium phosphate, magnesium carbonate, magnesium oxide, calcium oxide, magnesium silicate, activated alumina, activated carbon, Fuller's earth. It is also important that the particle size of the adsorbent should be fairly uniform or an uneven chromatogram will result. The successful separation of sugars and sugar derivatives on columns of magnesium or calcium silicates (Wolfrom *et al.*, *J. Amer. Chem. Soc.*, 1945, **67**, 527, 1789; 1946, **68**, 1449, 1720, 2169) is an interesting application of certain of the less common adsorbents. With regard to solvents, the following list in order of increasing powers of displacement is due to Strain (*loc. cit.*): petroleum ether, carbon tetrachloride, *cyclo*hexane, carbon disulphide, ether, acetone, benzene, organic esters, chloroform, alcohols, water (variations according to pH and concentration of salt), pyridine, organic acids, *e.g.* acetic acid. A large number of combinations of solvent and adsorbent are therefore possible and Williams (*loc. cit.*) recommends preliminary trials using a small amount of the adsorbent between two glass plates, *e.g.* photographic half plates, the upper one containing a small circular hole for the introduction of the compound and elution solvents. By this means the best experimental conditions for the separation of a given mixture can usually be determined in a short time.

Occasionally chemical changes may occur on the chromatogram and in certain cases these are due to an increase in the sensitivity of the compound in the adsorbed state to autoxidation, *e.g.* certain unsaturated fatty acids, or photosensitivity, *e.g.* 2:3-benzanthracene. Autoxidation can be countered by rigorous exclusion of air, and also by the introduction of potassium cyanide or hydrogen cyanide to neutralise the effect of traces of heavy metals, and photosensitive compounds should be treated in the absence of light. More often, however, the causes of chemical changes on the column can be ascribed to definite reactions which occur between the adsorbed compound and the adsorbent. The nature of these changes may not always be apparent, *e.g.* the irreversible change which chlorophyll undergoes when adsorbed on alumina, calcium carbonate, Fuller's earth, etc., and which is not observed on columns of sucrose, and similarly the biological inactivation of vitamin K_1 on magnesium oxide or alumina, but frequently the reactions which are observed are of a relatively simple nature. Thus acetone is partially converted to diacetone alcohol on alumina and dehydration of alcohols and elimination of acyl groups have been reported. These changes

which occur on the column may occasionally be put to useful purpose. Thus the hydrochlorides of certain alkaloids have been obtained by passing the free alkaloids or other salts through columns of alumina pretreated with hydrochloric acid. Plattner and his colleagues have shown that the picrates or trinitrobenzolates of triterpenes and azulenes are decomposed on alumina columns and the parent compound can readily be washed through the column while the nitro compound is firmly adsorbed at the top.

As the name implies, the technique of chromatography was originally applied to coloured substances, the position of which could readily be seen on the column, but the need for similar treatment of colourless compounds soon developed. Early solutions of this problem were to introduce coloured compounds of the same adsorptive properties into the mixture or to convert the colourless compounds to coloured derivatives, *e.g.* *p*-phenylazobenzoyl chloride was used with alcohols and phenols, etc., but these methods were obviously of limited application. The positions of compounds which fluoresce in ultra-violet light may be detected in a dark-room or under a black cloth, preferably using quartz chromatogram tubes. Alternatively, the admixture of a strongly fluorescent compound with the adsorbent is recommended for use with compounds which quench the fluorescence and therefore appear as dark bands (Brockmann *et al.*, *Naturwiss.*, 1946, **33**, 58; *Ber.*, 1947, **80**, 77; *Angew. Chem.*, 1947, **59**, 30; Sease, *J. Amer. Chem. Soc.*, 1947, **69**, 2242). Zechmeister (*loc. cit.*) has used a brush method, *i.e.* the column is extruded and streaked with a brush dipped into a suitable reagent, *e.g.* potassium permanganate for unsaturated compounds, sugars, etc., or alternatively reagent papers can be attached to the bottom of the column so that a positive reaction marks the appearance of a zone in the filtrate. Determination of the position of zones containing colourless biologically active substances can be made by taking samples from various parts of the column and carrying out appropriate biological tests. However, it is generally more convenient to wash the various fractions through the column and to take arbitrary fractions of the eluate. Besides a weight/volume graph, the progress of the separation can be followed by determinations of suitable physical constants of the various fractions, *e.g.* optical rotation, ultra-violet absorption spectra, or by microbiological tests if applicable, or by suitable chemical tests. This method was extensively used in the purification of crude concentrates of penicillin and streptomycin. The automatic fraction collector designed by Moore and Stein (*Ann. N.Y. Acad. Sci.*, 1948, **49**, 266) should prove to be very valuable in this type of

purification. Closely related to this method is the "Durchlauf-Methode" which has been elaborated in the laboratories of Reichstein and Ruzicka in Switzerland, and examples of its use are to be found in numerous papers in the *Helvetica chimica Acta* over the last few years. It has been used with success with steroids, sex hormones, diterpenes and triterpenes, and consists in eluting the column with a series of solvents or solvent mixtures of increasing developing or eluting power. A relatively small column is used and the fractionation is often satisfactory even if the various fractions are not present in distinct zones. The solvent is removed from the arbitrary fractions taken as before and the progress of the separation followed as in the previous method.

The more refined methods of Tiselius are also applicable to the separation of mixtures of colourless compounds, although in this case a special apparatus is required which is somewhat complicated in design. Details are included in the various reviews of the method (Tiselius, *Advances in Colloid Science*, 1942, **1**, 81; *Advances in Protein Chemistry*, 1947, **3**, 67; Cannon, *Ann. N.Y. Acad. Sci.*, 1946, **47**, 135; Claesson, *ibid.*, 1948, **49**, 183). The solutions are forced upwards through the adsorption column and the separation is followed by continuously measuring the refractive index of the solution leaving the column, the whole apparatus being enclosed in a thermostat. The method can be carried out either as a "Frontal Analysis" when no developing solvent is used, as an "Elution Analysis" when the chromatogram is developed with a solvent, or as a "Displacement Development" using a more strongly adsorbed substance as developer. In frontal analysis, the possible occurrence of "tailing" makes the purity of all but the first fractions open to question, and thus the elution analysis and displacement development methods are generally more suited to preparative purposes, although certain compounds, *e.g.* fatty acids and esters, appear to be adsorbed irreversibly in that they can neither be displaced nor eliminated, and in these cases the method of frontal analysis must be applied. Displacement development on carbon columns has been used by Tiselius with sugars, amino acids and peptides. The methods have also been applied to gases, *e.g.* the lower paraffins and fluorocarbons, using changes in thermal conductivity to follow the separation.

Partition Chromatography.—The various ingenious applications of chromatographic methods which are associated with the names of Martin and Synge (*e.g.* Martin, *Ann. N.Y. Acad. Sci.*, 1948, **49**, 249; Martin and Synge, *Advances in Protein Chemistry*, 1946, **2**, 1; Martin, *Endeavour*, 1947, **6**, 21) arose from experiments devised

to separate the various acetyl derivatives of the amino acids on the basis of the considerable variation in the partition coefficient values between chloroform and water. After a number of preliminary experiments, a method was devised whereby silica gel was used to hold the aqueous layer stationary, while the chloroform layer was allowed to flow past. Furthermore, it was shown that if methyl orange or other suitable indicators are dissolved in the water of the silica gel, the position of the acetylamino acids was revealed on the column. In general it was necessary to use a small quantity of an alcohol dissolved in the chloroform, *e.g.* 1 per cent. *n*-butanol, for the bands to move at a reasonable rate. By this means many of the amino acids resulting from the hydrolysis of a peptide could be separated. It was frequently necessary to increase the proportion of butanol in the chloroform in order to elute the more firmly bound amino acids, although even this procedure was not entirely satisfactory. The method has also been used with the 2:4-dinitrophenyl derivatives of certain amino acids (Sanger, *Biochem. J.*, 1945, **39**, 507; 1948, **42**, 287) and with fatty acids (Elsden, *ibid.*, 1946, **40**, 252; Peterson *et al.*, *J. Biol. Chem.*, 1948, **174**, 775). It is sometimes advantageous to vary the *p*H of the aqueous phase by the use of buffered solutions, *e.g.* in the case of penicillin (Levi *et al.*, *Nature*, 1946, **158**, 675; B.P., 569, 844). Attempts to give a quantitative interpretation of these methods have been hampered by the difficulties of obtaining silica of reproducible quality.

The problem of applying this method to the amino acids themselves led to the substitution of cellulose for silica, and the development of paper chromatography (Consden, *Nature*, 1948, **162**, 359). A large sheet of filter paper was used in a box saturated with water vapour, and the position of the amino acids, after separation, revealed by drying the paper and viewing in ultra-violet light, or better by spraying with ninhydrin, and then warming, when coloured spots develop. In later work it was found more convenient to use paper strips hanging from a trough containing the solvent saturated with water, and the whole apparatus was enclosed in an insulated vessel, the atmosphere within which was saturated with the vapours of the solvent and water. It was found that the ratio between the distance travelled by each individual compound and the distance travelled by the solvent was a constant for the compound, and this was designated the R_F value. Certain anomalous results obtained in early experiments were traced to the presence of copper in the filter paper and were overcome by the incorporation of hydrogen cyanide, or in certain cases coal-gas, into the mixture.

The most useful solvents have been butanol-acetic acid, *sym.*-collidine, phenol and benzyl alcohol. More decisive separations have been achieved by carrying out the chromatogram in two dimensions, using different solvents for each direction of the separation, and by this treatment as little as 0·3 mg. of the hydrolysate of a peptide or simple protein can be resolved into the individual amino acids, which are identified by comparison with standard substances. Williams and co-workers (*Science*, 1948, **107**, 481) have described a method of paper chromatography when the liquid ascends the paper, and claim that the technique is simplified. The most important applications of these methods of partition chromatography have been in the fields of amino acids and peptides, *e.g.* as criteria of purity and as a means of identification of new compounds, and special mention should be made of the structure of gramicidin S, a cyclic polypeptide antibiotic (Consden, Gordon and Martin, *Biochem. J.*, 1947, **41**, 596) which contained five individual amino acids in the order, L-valine, L-ornithine, L-leucine, D-phenylalanine, L-proline. Preliminary applications to certain other polypeptide antibiotics, *e.g.* aerosporin, gramicidin and tyrocidin, have been reported and some experiments have also been described on the analysis of the amino acids obtained from the hydrolysates of wool. It should be emphasised, however, that it is frequently necessary, when working with natural products, to apply preliminary purifications before paper chromatographic methods can be used to the best advantage. Numerous ingenious methods have been developed for the quantitative determination of amino acids on the paper, *e.g.* extraction as the copper complexes and then estimation of the copper colorimetrically or by polarographic methods. Another method has been the incorporation of radioactive elements into the amino acids or suitable derivatives, followed by estimation on the paper by counting. Microbiological methods have been evolved for the identification and determination of optical sign of several individual amino acids. Identification of certain of the simpler peptides has also been achieved by these methods, although the problem is more complex, as the compounds are more sensitive to conditions, and comparison with standards is essential. Further information can be obtained by hydrolysis to the amino acids which can then be identified by the above methods.

The technique has been applied to several other groups of compounds which are either water-soluble or readily form water-soluble derivatives, and considerable success has been achieved in the carbohydrate field. The original application to sugars was due to Partridge (*Nature*, 1946, **158**, 270; *Biochem. J.*, 1948, **42**, 238,

251) who developed the sugar spots with ammoniacal silver nitrate with subsequent heating. Other workers have used special reagents for various classes of sugars, *e.g.* pentoses and ketoses, and the fluorescent spots obtained from reducing sugars with *m*-phenylenediamine have also been recommended (Chargaff *et al.*, *J. Biol. Chem.*, 1948, **175**, 67). Quantitative methods are also available for sugars. Successful applications of paper chromatography to nucleotides and flavines, purines, anthocyanins, pterins, adrenalin and related compounds, penicillin, tannins and organic acids (after conversion to the corresponding hydroxamic acids) have been reported.

Separations such as can be achieved on paper can be carried out on a preparative scale by the use of starch columns (Elsden and Synge, *Biochem. J.*, 1944, **38**, 285; Moore and Stein, *Ann. N.Y. Acad. Sci.*, 1948, **49**, 265). The separation of amino acids and simple peptides has been described and it is certain that many other applications of the method will be reported in the near future.

GEOLOGY.

By G. W. Tyrrell, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

Igneous Rocks and Igneous Action—Great Britain, Europe, North America, Pacific Ocean, Australia.—"The Gabbroic Complex of Belhelvie in Aberdeenshire," described by F. H. Stewart (*Quart. Journ. Geol. Soc.*, CII, 1947, 465–98), consists of a mass of basic and ultrabasic rocks which intrudes members of the Dalradian Series and is exposed over an area of 7 sq. miles. It is younger than the regional metamorphism and older than an adjacent patch of Old Red Sandstone conglomerate. The general succession of rock types from west to east is: dunite-serpentine and peridotite, troctolite, hypersthene-gabbro, olivine-gabbro, and end-stage derivatives of the two gabbros. The evidence indicates that these types are due mainly to differentiation in place by crystal settling under the influence of gravity. Small-scale rhythmic banding and allied structures which have a nearly vertical dip lead to the conclusion that the Belhelvie intrusion is a layered concordant mass, probably of Bushveld type, which may have been tilted considerably since its consolidation. During the ensuing discussion on this paper it was pointed out that the hypothesis of tilting raised formidable tectonic issues, of which, however, the author was fully cognisant.

H. G. Midgley describes "The Geology and Petrology of the Cockburn Law Intrusion, Berwickshire" (*Geol. Mag.*, LXXXIII, 1946, 49–67). Cockburn Law is a small sheeted boss of late Caledonian age injected into the Silurian of the Southern Uplands of Scotland. The earliest activity was the intrusion of a basic igneous

rock which is now found only as xenoliths. This was followed by the main plutonic magma, which assimilated the basic rock and Silurian sediments to produce three types of hybrid diorite, characterised respectively by hypersthene, diopside, and by both pyroxenes. After the marginal hybrids had solidified, the remaining magma was still able to differentiate, giving rise to rocks ranging from adamellite, through granite, to granophyric micrograin ite, followed by some pneumatolysis, and the formation of aplitic and lamprophyric dikes from residual magma.

D. Balsillie found leucite in a basanite (Great Car) from the Carboniferous igneous rocks of East Lothian (1936), but as early as 1910 Sir E. B. Bailey suggested that leucite occurred in the kulaite of Blaikie Heugh. J. A. E. Bennett, however, has confirmed the presence of this mineral in several basic lavas ("Some Occurrences of Leucite in East Lothian," *Trans. Edin. Geol. Soc.*, XIV, Pt. 1, 1945, 34–52). He describes leucite-kulaite lavas from the Leeks, North Berwick, and from Blaikie Heugh; leucite-basanite from North Berwick; and leucite-melabasanite from Garvald. He also suggests that leucite, or a potash-rich analcite, previously existed in several of the East Lothian analcite-basanites.

E. M. Patterson's important paper on "The Teschenite-Picrite Sill of Saltcoats, Ayrshire" (*Trans. Geol. Soc. Glasgow*, XXI, Pt. 1, 1946, 1–28) carries the theory of the petrogenesis of these sills one stage farther. The sill shows four main subdivisions: a flow-banded teschenite at the top with included xenoliths and rafts of the overlying shale, intensely baked, along the outer and upper contact, and also along its lower contact with biotite-teschenite. The latter is fine grained at the contact but becomes coarser downwards; it is cut by veins of potash-lugarite. An identical rock forms the lower contact of the sill; but there is an intervening band of hornblende-picrite with ribs and nodules of hornblende-peridotite. Veins of lugarite traverse the picrite. The order of intrusion was first flow-banded teschenite, then biotite-teschenite, and finally picrite. The petrology and chemistry of the intrusion are dealt with by the aid of new analyses of the biotite-teschenite, and of a vein of potash-lugarite. Gravitative differentiation probably occurred in a pre-intrusion phase.

"The Composite Intrusion of Srón Bheag, Ardnamurchan," has been investigated by J. McMath (*Geol. Mag.*, LXXXIV, 1947, 257–69). The components are a central body of quartz-dolerite, and parallel flanking bodies of quartz-bostonite. The order of intrusion was, first, quartz-dolerite, and then the acid bodies on its flanks. The contacts between the two rock-types may be sharp

in one place, brecciated or xenolithic only a short distance away. Deepseated and contact types of hybridisation are described. Bailey's hypothesis is tentatively applied, *viz.* that the complementary magmas froze in the reservoir arranged in order of density, quartz-dolerite below and quartz-bostonite above. A subsequent rise of the isogeotherms would tend to mobilise the basic fraction first, enabling it to mix with and hybridise the acid material; and then to rise into the dike fissure where in time it would be joined by the acid magma rising along marginal planes of weakness.

The Knocksoghey Sill (Co. Antrim) is described by S. I. Tomkeieff and E. M. Patterson (*Irish Naturalists' Journ.*, IX, 1947, 89–96) as a tabular intrusion cutting obliquely through an ash-bed and overlying lavas belonging to the Lower Plateau Series (Tertiary). It is 125 ft. thick and consists of a coarse-grained olivine-dolerite with over 20 per cent. of olivine. A very full chemical analysis by E. M. Patterson shows that it is more basic than is usual with Irish Tertiary dolerites. It is cut by bands or veins of zeolite-rich rock and by dolerite-pegmatite which represent the residual magma of the crystallising dolerite. Unlike some of the comparable Scottish sills the Knocksoghey mass shows no signs of gravitative differentiation.

The paper by O. T. Jones and W. J. Pugh on "The Complex Intrusion of Welfield, near Builth Wells, Radnorshire" (*Quart. Journ. Geol. Soc.*, CII, 1946, 157–88) "deals with an area of dolerites intrusive into Llandeilo Shales and forming a prominent ridge . . . An analysis of the relations between the dolerites and the associated strata, strongly baked near the contacts, leads to the conclusion that the ridge is occupied by a core of dolerite having the habit of an elongated laccolith. The inferred main body is accompanied by many lenticles of dolerite on the flanks of the structure, and also sheets which appear to spring off from two dyke-feeders near the crest of the laccolith." The whole area has been tilted to the north-west in post-Silurian times. It is estimated that the laccolith was intruded under a cover of Ordovician at least 4500 ft. thick.

Describing the "Potash-rich Rocks of the Esterel, France," R. D. Terzaghi (*Amer. Min.*, **33**, 1948, 18–30) shows that, while a fresh vitreous obsidian has a normal chemical composition, devitrified obsidians and rhyolites are abnormally rich in potash and/or silica. The author suggests that these rocks owe their abnormal composition to hydrothermal enrichment in the above constituents.

An important memoir on "The Geology of the Morcote Peninsula, and the Petro-chemistry of the Porphyry Magma of Lugano," by K. P. Rode (*Schweiz Min. Petr. Mitt.*, XXI, 1941, 194–312) deals

with the Lugano Porphyries of Permian age. The fundamental rocks of the district are gneisses and schists of the Hercynian Basal Complex. They were followed by a thin series of conglomerates and sandstones of Permo-Carboniferous age, and then the porphyries and tuffs of the Permian with which the memoir is concerned.

The main rock types are diabases (basalts), pyroxene-porphyrites (andesites) of basic character, quartz-bearing porphyrites (dacites) and granophyres. There are also some quartz-porphyry lavas and dikes. Discussion of the petrochemistry and differentiation of the Lugano porphyries by the use of the various Niggli methods occupies the greater part of the memoir. The suite appears to represent a straightforward fractional crystallisation of a tholeiitic basalt magma, leading finally to an aplitic residual magma.

Another important memoir on Swiss rocks is that by M. Vuagnat "Sur quelques diabases Suisses. Contribution à l'étude du problème des spilites et des pillow lavas" (*Bull. Suisse Min. Pétr.*, XXVI, 1946, 116–228), which deals with three occurrences of the Alpine ophiolites, *viz.* the "diabases" of the Préalps romandes, of the Arosa zone, and of the Lower Engadine. These are so little metamorphosed that they still retain pillow and variolitic structures, and are associated with volcanic breccias and with albitised contact rocks (adinoles). It is probable that these rocks terminated the cycle of Mesozoic ophiolitic activity. The initial magma of essentially basaltic composition acquires under conditions still obscure a spilitic habit; the magma is enriched in soda, titania, water, and carbonic acid, with a diminution in the amount of lime. Thus, starting with augitic diabases, we have the successive mineral associations: albite-augite, albite-chlorite-augite, albite-chlorite, and albite-hæmatite.

Pillow and variolitic structures are ascribed to the drastic chilling consequent upon eruption on the sea floor. Vapour tension in the pillows often rises high enough to shatter the rocks to fragments and form volcanic breccias. Low-grade metamorphism converts the spilitic suite into green schists, prasinites, etc. The piling-up of the nappes results in a rise of the isogeotherms with differential solution of the constituents of the ophiolithic rocks, the feldspars going into solution first and giving rise to solutions of alkaline silicates. These find their way into fissures and help to form a characteristic suite of Alpine mineral veins. This paper also contains a clear and concise description (in French) of Niggli's molecular norms and other graphic methods.

In his paper "Beiträge zur Petrographie des Gesteinszuges Ivrea-Verbano I. Allgemeines. Die gabbroiden Gesteine von

Anzola" (*Schweiz. Min. Petr. Mitt.*, XXII, 1942, 326–66) H. F. Huttenlocher deals with a hornblende-gabbro which has a marked parallel banding, pegmatitic pyroxene-rich and feldspar-rich facies, and is cut by garnet-pyroxenite veins. The petrological discussion centres about three questions: Is the Anzola gabbro a primary flow-banded igneous rock, a subsequently metamorphosed gneissified rock, or is it an ultrametamorphic sediment? The consensus of evidence is in favour of the first alternative.

T. Einarsson propounds an original theory of the "Origin of the Basic Tuffs of Iceland" (*Acta Natur. Islandica*, I, No. 1, 1945, 1–75). He says, in effect, that the "Palagonite Formation," supposedly of Pleistocene age, which appears to cover the Tertiary basaltic foundation of Iceland and fills fault-hollows in it, does not exist as such. Einarsson adduces field evidence in favour of the view that palagonite-tuffs and associated fragmental rocks are an integral part of the Tertiary lava basement at at least two horizons, one interbedded with the main body of basalts and the other heaped up on the top of the plateau. He rejects the view that the pyroclastic formations in Iceland are inherently associated with sub-glacial conditions, and regards them as essentially of volcanic origin, *i.e.* agglomerates, mud-flows, sand-flows, and tumultuous boulder beds formed by incandescent blasts of Peléean and Lassen type. The writer of these notes, however, would point out that volcanic fragmental deposits of this type, so far as is known at present, are associated only with the violent explosive eruptions of volcanoes emitting highly viscous rhyolitic, trachytic, and andesitic lavas, never with the basaltic shield volcanoes and fissure eruptions of Icelandic and Hawaiian type.

The memoir "Igneous Rocks of the Ivigtut Region, Greenland. Part I. The Nepheline-syenites of the Grönnedal-Ika Area" (*Medd. om Grönland*, **131**, No. 8, 1943, 74 pp.), by K. Callisen, deals with a series of alkaline rocks with which the famous cryolite deposit of the area is believed to be genetically connected. The mass is about 9 km. long, but its width is not yet exactly known. It consists of a coarse nepheline-syenite with fine-grained border-facies. In the higher parts of the mass a transition to a coarse pulaskite can be followed. The massif is regarded as an irregular stock carrying large pendants of the local gneisses. As calcareous rocks are scarce in southern Greenland, Daly's limestone syntexis theory of origin of alkaline rocks is difficult of application.

M. P. Billings and J. C. Rabbit deal with "Chemical Analyses and Calculated Modes of the Oliverian Magma Series, Mount Washington Quadrangle, New Hampshire" (*Bull. Geol. Soc. Amer.*, **58**, 1947,

573–96). The six analyses used, of monzonites, quartz-monzonites, syenite and granite, show unusual richness in potash, which is throughout systematically higher than soda. The principal oxides give the usual type of variation diagram; alkalis and alumina reach a maximum in the syenite, but lime, magnesia, and total iron decrease towards the silica-rich end of the series. The analyses include spectrographic determinations of 44 trace elements. Although many of these show no systematic change with increasing silica, Cr_2O_3, NiO, SrO, V_2O_5, and BaO decrease, but ZnO increases, with augmentation of silica. A valuable discussion of the principles of calculation of modes of rocks from the chemical analyses is given.

A number of recent papers and memoirs deal with the geology and petrography of the Tertiary, Pleistocene and Recent lavas, mostly basalts, of the Hawaiian Islands. H. Winchell describes the "Honolulu Series, Oahu, Hawaii" (*Bull. Geol. Soc. Amer.*, **58**, 1947, 1–48) as a series of thirty distinct and separate eruptions of nepheline-basanite, nepheline-basalt, and nepheline-melilite-basalt, of Late Pleistocene and Recent age, in the south-eastern part of the Koolau Range of Oahu. Eight new analyses supplement the petrographical descriptions. The origin of these rocks is left an unsolved problem. No theory of differentiation seems to be satisfactory, but limestone syntexis is shown to be definitely out of the question.

The "Koolau Basalt Series, Oahu, Hawaii" is described by C. K. Wentworth and H. Winchell (*ibid.*, 49–78). The Koolau Dome, of Late Tertiary and Early Pleistocene age, occupies the eastern and larger part of Oahu. It is composed almost entirely of thin basaltic flows which were erupted from fissures running north-west to south-east parallel to the long axis of the dome, and now occupied by a dike-swarm in a belt 2–5 miles wide. The Koolau Series consists mainly of a uniform type of basalt which is slightly more siliceous than the average Hawaiian basalt; but some are olivine-rich (oceanite) and others contain numerous phenocrysts of labradorite and hypersthene.

In a paper entitled "Contribution to the Petrography of Haleakala Volcano, Hawaii," G. A. Macdonald and H. A. Powers (*Bull. Geol. Soc. Amer.*, **57**, 1946, 115–24) indicate that its purpose is to make six new analyses available to petrologists. Haleakala comprises three volcanic series known, in order of age, as the Honomanu, Kula, and Hana Series. The Honomanu Series consists mainly of olivine-basalt and less abundant basalt and picrite-basalt with large olivines (oceanite). The characteristic types of the Kula and Hana Series are andesites, and picrite-basalts with both olivine and

augite phenocrysts (ankaramite). The six new analyses are all of Kula lavas. A variation diagram of the rocks of Haleakala illustrates the alkaline trend of the series.

H. T. Stearns and G. A. Macdonald describe the "Geology and Ground-water Resources of the Island of Molokai, Hawaii" (*Hawaii Div. of Hydrography, Bull. 11*, 1947, 113 pp.). The island is the fifth largest of the Hawaiian Islands with an area of 260 sq. miles. It consists of two principal parts, East and West Molokai, each a major volcanic dome. East Molokai is built largely of basaltic lavas and has a thin cap of andesites and a little trachyte, with which cinder cones and bulbous domes are associated. The basalts are cut by two major rift-zones now occupied by dike-swarms trending west to east and north-west to south-east. West Molokai was built by basaltic lavas erupted along rift-zones trending north-west to south-east and north-east to south-west. It became extinct earlier than East Molokai, and its flank is partly buried beneath the lavas of the larger and later volcano.

The lower member of the East Molokai Series consists mainly of olivine-basalts with smaller amounts of basalt and picrite-basalt (ankaramite and oceanite). The upper member consists of oligoclase-andesite with smaller amounts of andesine-andesite and trachyte. West Molokai consists of olivine-basalts, picrite-basalts rich in olivine (oceanites), and ordinary basalts. The whole suite is typical of mid-oceanic volcanoes.

H. T. Stearns describes the "Geology and Ground-water Resources of Niihau, Hawaii" and G. A. Macdonald the "Petrography of Niihau" (*Hawaii Div. of Hydrography, Bull. 12*, 1947, 53 pp.). Niihau has an area of 72 sq. miles. The main mass of the island is composed of a deeply weathered remnant of a basalt dome of Tertiary age. During Pleistocene times the island was enlarged by the eruption of lava and tuffs from nine vents now visible, and from others now buried. The Tertiary lavas are olivine-basalts and ordinary basalts, with some picrite-basalt (oceanite) of "primitive type." The later lavas are olivine-basalts, and a single occurrence of melilite-nepheline-basalt has been reported.

The geology and petrography of the Wallis Islands are reported upon by H. T. Stearns and G. A. Macdonald respectively (*Bull. Geol. Soc. Amer.*, **56**, 1945, 849–72). The Wallis Islands lie about 200 miles west of the Samoa Group in the southern Pacific Ocean. They consist of Uvea, the main island, and 22 smaller islands. Uvea was built by the coalescence of lava flows from nineteen volcanic vents, comprising fifteen flat shield lava cones, three consolidated ash cones, and one cinder cone. Most of the lavas are

highly decomposed; they are thought to belong to the Middle Pleistocene, but there are two Recent lava cones. The lavas of Uvea include olivine-basalt with colourless monoclinic pyroxene; less abundant olivine-basalt with titaniferous pyroxene, and oligoclase-andesite. Other islands consist of olivine-basalts with palagonite-tuff.

Niuafo'ou, an outlying island of the Tonga Group, is mainly a basaltic shield volcano with a caldera 2¼ miles in diameter. Its volcanology and petrology are described by G. A. Macdonald (*Amer. Journ. Sci.*, **246**, 1948, 65–77). The shield volcano was built up in the usual way by quiet emissions of olivine-basalt lavas. More recent eruptions, of which the last was in 1946, built up a steep composite cone which has been almost completely destroyed during the formation of the caldera. The later eruptions consist of basalts with little or no olivine, and the island is therefore considered to belong to the circum-Pacific group of continental volcanoes.

A very remarkable suite of igneous rocks, the richest in leucite yet described, is the subject of a paper on "The Leucite-bearing Rocks of the West Kimberley Area, West Australia," by A. Wade and R. T. Prider (*Quart. Journ. Geol. Soc.*, XCVI, 1940, 39–98). They consist of plugs, craters, and fissure intrusions of post-Permian age, which are associated with structural planes in the underlying pre-Cambrian basement.

R. T. Prider has given a very comprehensive account of the petrography and a discussion of the petrogenesis of this unusual series. Four new rock types are described; fitzroyite, cedricite, mamilite, and wolgidite which, with transitional varieties, are comagmatic and form a well-marked differentiation series. Although leucite is developed to the exclusion of sanidine, these rocks are saturated types with more than sufficient silica to have formed sanidine in place of leucite. The original magma was of peculiar composition, its main features being high potash dominant over alumina, high titania and magnesia, and very low soda. It is considered to have been derived from a potassic mica-peridotite magma by the early crystallisation and removal of olivine. The suite is comparable with the wyomingite–orendite suite of the Leucite Hills of Wyoming, and the same mechanism of differentiation is invoked for this occurrence also.

In "A Note on some Leucite-bearing Rocks from New South Wales, with Special Reference to an Ultrabasic Occurrence at Murrumburrah," M. H. Harvey and G. A. Joplin (*Journ. & Proc. Roy. Soc. N.S.W.*, LXXIV, 1941, 419–42) describe an intrusion of leucite-monchiquite which has a late magmatic phase consisting of

veins, patches and schlieren of rocks rich in nepheline, analcite, titanaugite, and biotite. Xenoliths of dunite and chromite-bearing harzburgite, and worn xenocrysts of pyroxene, are of common occurrence. At the contacts with granite and granitic inclusions hybrid rocks have been formed, in which quartz with coronas of pyroxene are conspicuous.

A paper on "The Petrology of the Silurian Volcanic Sequence at Wellington, New South Wales," by Miss M. J. Colditz (*Journ. & Proc. Roy. Soc. N.S.W.*, LXXXI, 1948, 180–97) shows that the lavas are trachyandesites, andesites, basalts, and trachybasalts, associated with vitric and crystal tuffs, breccias, and agglomerates, which have genetic relationships with sills of augitic lamprophyre. The lavas are much altered, and are penetrated by many veins and vughs attributable to deuteric activity, and carry many cognate and accidental xenoliths. The series belongs to a mildly alkaline petrographical province which occupied the central part of New South Wales during Silurian times.

E. M. Basnett describes "Hybridisation in the Wuuluman Creek Intrusion" as the first part of a series of "Studies in Metamorphism and Assimilation in the Wellington District, New South Wales" (*Journ. & Proc. Roy. Soc. N.S.W.*, LXXIII, 1940, 161–89). The intrusion invades folded Silurian sediments, and consists of an earlier dolerite and a later keratophyre, which has been injected into and has hybridised the dolerite. The mineral changes in the hybridised rocks are shown to be due to the introduction of a sodic type of keratophyre, followed by a magma of similar type but richer in potash. Solutions containing material released during the hybridisation effected further changes around veins and interstices. The constant development throughout the process of albite and epidote is considered to be due to low-temperature conditions and abundance of water.

Port Cygnet, Tasmania, has long been known as a locality for some rare alkaline igneous rocks rich in nepheline, hauyne, and melanite garnet. A. B. Edwards has recently investigated this area in detail ("Alkali Hybrid Rocks of Port Cygnet, Tasmania," *Proc. Roy. Soc. Vict.*, LVIII, Pts. I–II (N.S.), 1947, 81–115), and reports that there is no differentiated stock as supposed by previous workers. Further, the unusual rock types reported by Twelvetrees and Paul, such as essexites, monchiquites, shonkinites, and jacupirangites, are not differentiates of a syenitic magma, but hybrid rocks of very local development formed by reaction of a potash-rich magma with a body of Mesozoic dolerite. Mapping has shown that a single type of fine-grained "alkali-porphyry" predominates in the district, and

that the rare rock types for which the locality is famous constitute less than 1 per cent. of the total volume of exposed igneous rocks.

BOTANY. By Professor W. H. Pearsall, D.Sc., F.R.S., University College, London.

Cell Wall Structure.—An interesting contribution to the study of cell wall structure has been made by A. B. Wardrop and H. E. Dadswell, *Bull. Coun. Sci. Ind. Res.* (*Aust.*), No. 221, pp. 32, 1947. These authors have investigated the behaviour of sections of wood which have been delignified by alternating treatments of chlorine water and a 3 per cent. solution of monoethanolamine in 98 per cent. ethanol, followed by dilute alcoholic acetic acid washes. The delignified material when treated with dilute alkali shows a marked separation of the cells and slight mechanical agitation of the sections was found sufficient to cause them to break up into small groups of cells, a process called by the authors maceration. No separation of cells occurred on treating similar sections with polyuronide solvents. Mackney, *J. Coun. Sci. Ind. Res.* (*Aust.*), **13**, 299, 1940, had shown the existence in a species of *Eucalyptus* of a dilute-alkali-solvent non-furfural yielding constituent to the extent of nearly 3 per cent. Thus there were some grounds for believing that the wood cells were bound together by a non-lignin, non-polyuronide intercellular bonding material, which could be removed by dilute alkali, permitting the maceration of the tissue. Wardrop and Dadswell have now investigated this problem again and have decided that any tendency for the cell walls to separate is due to the swelling that they show, although the alkali does take into solution part of the cambial wall. They consider that their evidence shows that the previous conception of a non-lignin, non-polyuronide bonding material, soluble in dilute alkali, must be rejected, and that the evidence suggests that intercellular adhesions in delignified tissues are to be attributed mainly to mechanical adhesions between cell-wall materials. In the second part of this Bulletin the authors go on to investigate and discuss the nature of the slip-planes and minute compression failures in wood in relation to their botanical properties, chemical and staining reactions and distribution in the growing tree. As a result of this investigation they conclude that a change in cell-wall composition is not the underlying reason for the susceptibility of these deformations to staining or to chemical attack. Rather it is thought that there is a micellar distortion which results in increased micellar surface (and hence preferential staining reactions), a greater ease of penetration of reactant molecules and an increased reaction rate because of the locally increased

micellar surfaces. Thus the cell-wall deformations are more readily susceptible to hydrolysis and acetylation than is the rest of the wall. The examination of the distribution of these features in a growing tree suggests that their formation is dependent upon the existence of growth stresses during the development of the tree. No evidence was found that slip-planes and minute compression failures might be regarded as successive stages in failure under compression.

ECONOMIC BOTANY.—From Northern Ireland comes a survey of the diseases of flax plants, by A. E. Muskett and John Colhoun, which is published by the Northern Ireland Flax Development Committee, and may be obtained at a cost of 21*s.* post free on application to the Bursar, Queen's University, Belfast. It has been found that a large part of the unsatisfactory yields in Irish flax crops is due to the diseases carried by the seed. These did not affect the germination of the seed, but attacked the young plant as it was becoming established. The present volume contains a general account with excellent illustrations of the various diseases encountered in Ulster and their causative organisms; and it also contains appendices which detail the methods employed in examining seed to detect the presence of seed-borne parasites. The development of this Ulster method of seed-testing was followed by an attempt to disinfect the seed commercially, which was so successful that it has shown that it is possible to test and disinfect the total seed supplies necessary for a particular crop of this sort. There is, finally, a reference to the practical measures necessary to ensure the establishment of uninfected and healthy crops in Northern Ireland.

A somewhat similar publication, entitled *Boron and Plant Life*, Part 6, by R. W. G. Dennis, has been issued by Borax Chemicals, Ltd., London, W.C.1, which deals with the development of the use and control of boron in agricultural and horticultural practice during 1943–46. This contains a great deal of detailed information and some 300 references relating to this subject. One section deals with the boron contained in different plants or plant organs, as well as organic manures, irrigation waters, rocks and soils. The second section deals with the effects and need for boron manuring in a great variety of crops, and this includes a list of plants in which boron is shown to be essential for normal growth, as well as a brief discussion and summary of recent views on the rôle of boron in the plant tissues.

A pamphlet on the uses of willows and poplars in soil conservation and river works, by A. P. Grant and G. R. Fenton, is published as the Bulletin No. 6 of the Soil Conservation and River Control

Council (Wellington, New Zealand). It contains a useful survey of the commoner willows and poplars so far introduced into New Zealand. Most of these are European forms. It outlines the methods of propagation, planting and arrangement which have been found necessary for protection work along watercourses and unstable soils. Although many of the methods are already known in this country, they have not been used to stabilise river channels exposed to such severe erosion as are many of those in New Zealand.

Taxonomy.—A considerable number of interesting taxonomic articles has recently appeared. One of the most important of these is a paper by Sir William Wright Smith and Dr. H. R. Fletcher on the genus *Primula* (*Trans. Roy. Soc. Edinb.*, **41**, 631, 1948) which continues a long series of publications on this subject. The present paper deals with the sections Cuneifolia, Floribundæ, Parryi and Auricula, all plants of which are now known to have involute leaves. The largest of these subsections, that of Auricula, has been adequately treated on the cultural side in English by Farrar and others, though other more strictly taxonomic aspects have been neglected. The pertinent botanical information has now been included in the present publication, which deals with a very large number of hybrids, subspecies and varieties, for the details of which it would be necessary to consult the original memoir.

A Portuguese paper has appeared from the Institute of Botany of Coimbra (A. Fernandes, J. Garcia and R. Fernandes, *Mem. Soc. Broteriana*, **4**, 1, 48) which deals with the flora of the Bragança district, the first part described being that of the Vendas Nova area. This includes a general account of the vegetation, as well as a list of the species collected. A noteworthy feature is that the chromosome numbers of many of the Monocotyledons are included, particularly those of some of the more critical forms of *Narcissus* and *Iris*, as well as of Liliaceæ.

Another Iberian contribution by F. B. Roderigez (*Annal. Gardin Bot. Madrid*, **1**, 197, 1946) is a critical revision of the species of *Hypocrepis* in the Peninsula and the Balearic Islands, which includes an analytical key and distribution maps of the more noteworthy species. The Spanish species of the genus *Agrostis* have been revised by Elena Paunero (*ibid.*, 561), and there has also appeared a description of the Spanish species of the genus *Euphorbia* (L.) by Mariano Losa Espana (*ibid.*, 357), each of these including keys and detailed distribution maps. The paper on *Agrostis* also contains useful drawings of the leaf and floral characters.

Biological Flora of the British Isles has issued parts dealing with *Sonchus* and the annual species of this genus, *S. oleraceus* L. and

S. asper (L.) Hill, which have been prepared by R. A. Lewin (*J. Ecol.*, **36**, 203, 1948).

A further study of the ecology and taxonomy of Primrose and Oxlip has been prepared by D. A. Valentine (*New Phyt.*, **47**, 111, 1948) and the results recorded suggest that the two species tend to occupy slightly different ecological niches, the Oxlip being in the damper places. Interspecific hybrids, probably mostly of the F_1 generation, are very frequent in the Oxlip areas. An examination of the data does not, however, suggest that the Oxlip is being hybridised out of existence, as Miller Christy supposed. Rather it appears that the two species are in a condition of complex ecological equilibrium. The author takes the view that the two types are ecospecies of a cenospecies, and a revised definition of the term "ecospecies" is suggested with the aim of pointing out that two sorts of ecospecies may occur, those in which the eco-geographical conditions are the principal isolating factors and those in which the genetical differences are mainly concerned in isolation.

H. G. Baker has continued his studies of the genus *Melandrium* with a survey of the ecotypes of *M. dioicum* (*New Phyt.*, **47**, 131, 1948), in which the British material is resurveyed. The author critically reviews the earlier investigations of Turesson, and questions the distinctness of the coastal and sub-alpine forms which that author distinguished. He also contests the view that the alpine ecotype is the oldest and that all the other forms have been derived from it.

We have noticed elsewhere the *Drawings of British Plants*, Parts I and II, which are being produced by Stella Ross-Craig.

A contribution of a very different type is a short paper by A. A. Pearson (*Naturalist*, 1948, July–September, p. 85) on fungi of the genus *Russula*, a common British genus whose species are often difficult to identify. Within the space of just over twenty pages the author has succeeded in compressing a descriptive key to the sixty-six British species and fifteen varieties or forms, an index and a summary of the significant details, distinctive of the various species. This can be done because it has been found possible to include a plate giving the colours of spore deposits and also the details of spore ornamentation as seen under a considerable magnification. This is certainly a noteworthy piece of work, and to the non-specialist at least it appears to be an extremely useful one.

ZOOLOGY. By William Holmes, M.A., D.Phil., The University, Oxford.

Homology.—The concept of Homology is deeply rooted in the history of Biology and is constantly used in its current practice.

Perhaps it is because of its antiquity and usefulness that it is so often misused and misunderstood: thus, for example, homologous organs are often defined as organs in two different animals which are derived from a single structure in a common ancestor, and then the occurrence of such organs is used as evidence in support of the theory that a given group of animals has a common evolutionary origin.

The year 1943 was the first centenary of the definition, by the anatomist Richard Owen, of the terms " homology " and " analogy " as used in comparative anatomy. It should be pointed out at once that these are pre-evolutionary definitions; so far as they imply any connection between animals of a different type, it is a formal connection as variations from a common plan, created by God perhaps, with no implication of phylogenetic relationship. Owen said: " A Homologue is the same part or organ in different animals under every variety of form and function. An Analogue is a part or organ in one animal which has the same function as another part or organ in a different animal." Professor Boyden made the centenary the occasion for a plea for a return to Owen's original definition of these terms (*Quart. Rev. Biol.*, 1943, **18**, 228). He believes that the various subsequent elaborations of the meaning of homology have confused the problems concerned and stultified the value of the concept. He accepts the evolutionary amendment that homologous structures are those derived by variation from a single structure in a common ancestor, but questions many others. Thus he points out that three types of homology are now recognised: the original type is " the same organ in different animals " —this is called " special homology "; then there is the comparison of serially repeated structures in a metamerically segmented animal —the limbs of a crustacean, for example; this is " serial homology." Thirdly there is " general homology ": any scale of the salmon is homologous with any and all of the scales of the trout. Boyden believes that by grouping all these different morphological phenomena under the same head we imply a similarity of the underlying morphogenetic processes; this is illegitimate. Special homologies, for example, are the result of a genetic relationship between individual animals; serial homologies, on the other hand, have an entirely different significance to the zoologist, and a different morphogenetic basis: they are intra-individual differences. He points out that the range of variation in serially homologous organs is far greater than that between the steps in an evolutionary series of specially homologous organs; this is an index of the fundamental distinction. Boyden concludes, therefore, that if we are to carry

the concept of homology into the realms of experimental zoology we must first clean it of its accretions and returns to Owen's earliest and strictest definition. We then see that the study of homology is the study of those genetic mechanisms which ensure that the relative positions and essential structure of complex organs are retained and transmitted through generations of evolutionary change.

The same desire to keep the homology concept in line with modern biological thought underlies an article entitled "Concepts of Homology and Analogy," by C. L. Hubbs of Michigan (*American Naturalist*, 1944, **78**, 289). But the author does not agree with Boyden. He first protests against the limitation of the homology concept to structures, since it gives a static morphological picture incompatible with the modern view of the living organism; the behaviour patterns involved in the flight of two birds can be compared as usefully as can the structure of their wings. He then denies that there is a difference in kind between special and serial homologies: the mechanism which produces the two sorts of homology must be similar in its genetic and developmental features. And it is clearly absurd, he says, to restrict the concept in such a way that it forbids us to recognise general homologies, that any scale of a trout is homologous with any scale of a salmon. How then can we deny that any one scale of a trout is homologous with any other on the same animal, and this state of affairs is equivalent to admitting serial homologies. "The only serviceable modern concept of homology is that of the essential genetic agreement of structures and functions in evolutionary derivation and in embryonic development." He then enquires whether this end can be served by expressing homologies in the terms of genetics, but he finds many difficulties. Thus individual genes have multiple effects, and a given character may be influenced by many genes. Mutations may alter the whole genetic mechanism and modifier genes come and go. And between the genes and adult structure is interposed the complex process of development. The discussion at this point calls to mind an earlier consideration of the theme, by de Beer (*Evolution: Essays Presented to Professor E. S. Goodrich*, Oxford, 1938). This author, too, was conscious of the difficulties which arise when we attempt to look at homology from a modern point of view. Our morphological studies are always of the homologies of phenotypes, the genotypes are never considered; genes may control more than one character; characters that are evidently homologous as phenotypes need not be controlled by genes at an identical locus. In this way an analysis in terms of genes breaks down; similarly

an analysis in terms of cellular correspondence of position in development is equally unhelpful. For example, we cannot successfully compare the forms of gastrulæ, but we can compare (homologise) the morphogenetic movements by which they are formed. Similarly we may homologise the organising substances under whose influence these and other changes of morphogenesis take place, but we cannot decide the homology of an organ by considering isolated parts of the developmental process by which it is produced, for structures can owe their origin to different organisers without forfeiting their homology.

A fourth contribution comes from G. B. Moment, with comments on the views of his predecessors (*American Naturalist*, 1945, **79**, 445). He believes that a consideration of the facts of genetics and morphogenesis simplifies the problem and does not complicate it as Hubbs suggested. " Both serial and special homologies, as well as the general homologies seen in feathers, scales and bones are all expressions of similar developmental forces. Sometimes these forces have acted within several different kinds of animals, sometimes within different parts of the same individual." " With the growth of genetical and embryological knowledge it is becoming increasingly evident that the homologies or, better, similarities between organisms or their parts, are of several different kinds." Moment believes that it would be best to discard the word " homology " altogether ; it is no longer profitable or legitimate to ask what homology " really " is ; we might just as well ask how many days there " really " are in a week. Better that we should substitute a neutral word such as " similarity," and proceed to study the causes of the various sorts of organic similarity. Moment suggests that much of the discordance of view amongst biologists in this matter comes from their differing special interests : one-to-one correspondences between special structures are far more important to the students of higher vertebrates than they are to those who work in other fields.

So far the discussion may be said to have elucidated the difficulties of the problem without providing any ready solution to them. We then have a contribution from W. Etkin and L. G. Livingston (*American Naturalist*, 1947, **81**, 468). These authors describe themselves as teachers concerned with presenting the principles of Biology to students, and they are not prepared to follow Moment in abandoning the word homology nor to follow Hubbs in giving it the vaguest of definitions. They define homology as a condition of similarity between characteristics of two organisms which results from their having had a common origin in evolution.

There are many different sorts of evidence which may lead us to postulate a relationship of homology. First, there are similarities in fundamental structural relationship; thus the wing of a bird may be homologised with the arm of a mammal. Second, there may be similarities in development; it is on such evidence that the pharyngeal system of fish is homologised with the derivatives of the comparable system in mammals. Third, there are similarities in functional mechanism, where functions are considered in detail; thus the capacity of the endostyle of the larval lamprey to store iodine may be taken as evidence for its homology with the thyroid gland of higher forms. Fourth, there are similarities in chemical composition: it is reasonable and useful to regard chromatin, when found in bacterial cells, as homologous with the chromatin of multicellular organisms. And, last of these main sources of evidence, it is often possible to establish homologies between phases in the life cycle of different organisms. This mode of comparison is perhaps most striking in its application to the study of the life cycles of plants.

The attempt to establish homologies is the attempt to establish the course of an evolutionary process which took place in the past and cannot be repeated experimentally. The relevant evidence can therefore only be assessed "unscientifically." We can only at the best succeed in establishing a probability. In fact, these two authors conclude that the concept of homology is useful and plastic; it can be used successfully and without danger if it is applied with trained and informed biological common sense, but it is not amenable to a precise analysis of the causal factors concerned.

It is appropriate to conclude with a quotation from de Beer (*op. cit.*). It seems that his conclusions, taken together with those of Etkin and Livingstone, provide the best guide for the practical use of the homology concept. "... the best criterion for homology is comparative anatomy, and it is still possible to hold as did Etienne Geoffroy de St. Hilaire more than a century ago: 'The only general principle which can be applied is given by the position, the relations, and the dependencies of the parts, that is to say, by what I name and include under the term of *connexions*.' These are now more usually referred to as morphological relations, and it is their general constancy which gives them their value. Variation in morphological relations of homologous structures sometimes occurs, as, for instance, in the skull. But in these cases it is usual to find some special reason for the departure from type. In any case it should be noted that, since the developmental mechanisms of homologous structures can become changed, the wonder is, not

that morphological relations may sometimes vary, but that they are usually so remarkably constant. . . . But the interesting paradox remains that, while continuity of homologous structures implies affinity between organisms in phylogeny, it does not necessarily imply similarity of genetic factors or of ontogenetic processes in the production of homologous structures."

"EVOLUTION."—A new periodical, with the title of *Evolution*, appeared in 1947 and is now in its second volume. It is published by the Society for the Study of Evolution and is edited by Dr. Ernest Mayr of the American Museum of Natural History. The subscription rate for the volume is $6.00, or membership of the Society, which includes the journal, is $5.00, and the business manager is Mr. K. P. Schmidt, Chicago Natural History Museum, Chicago 5, Ill., U.S.A.

The editorial intention is that this shall be "a journal exclusively devoted to the results of research in the field of evolution." This is a far wider and more alive branch of biology than might at first appear; its scope can be seen by the consideration of the titles of some of the articles from the parts that have already appeared. E. Mayr writes on "Ecological Factors in Speciation" (Vol. 1, p. 263); this is a new discussion of one of the principal themes of his 1942 volume on *Systematics and the Origin of Species*, the view that a new species can develop only "if a population, which has become geographically isolated from its parent species, acquires during the period of isolation characters which promote or guarantee reproductive isolation when the external barriers break down." He considers the various criticisms of this hypothesis that have been brought forward in the intervening years and, with its bibliography, this paper is an important landmark in the development of modern evolutionary theory. F. Weidenreich writes on "The Trend of Human Evolution" (Vol. 1, p. 221); this is a different sort of study in which the author considers the evolution of the two most important specialisations that distinguish man from his nearest relatives: adoption of the erect posture and expansion of the cerebral cortex.

T. Dobzhansky and B. Sparsky describe some of their studies on "Evolutionary Changes in Laboratory Cultures of *Drosophila pseudoobscura*" (Vol. 1, p. 191). There is no longer any need for scepticism as to the significance of laboratory studies of mutation and selection as indicators of the corresponding processes in nature, for laboratory mutations need not always be deleterious. These authors put forward a hypothesis, and support it with experimental

evidence, that favourable mutations may be observed in the laboratory if the animals used for the experiment come from strains carrying hereditary factors which reduce their vitality in the environmental conditions of the experiment. Dobzhansky reports other work along similar lines in an article on "Adaptive Changes induced by Natural Selection in Wild Populations of *Drosophila*" (Vol. 1, p. 1).

Yet another kind of evolutionary study is described by B. Lutz on "Ontogenetic Evolution in Frogs" (Vol. 2, p. 29). Some evolutionary changes may affect only the embryos or larvæ of an animal, leaving its adult state unchanged from that of its ancestors. Such a process, which has been called—by de Beer—"clandestine evolution," may be important either simply as a cause of progressive divergence of ontogenies or as a possible source of rapid evolutionary change if the process called neoteny takes place, the animal becoming adult in the morphologically larval stage. This paper is a study of the deviations from the "typical" life cycle within the amphibian Order Anura. They are considerable and interesting, and it is surprising to learn that, as yet, the life cycles are known only for one-third of the described Anuran species.

A. E. Wood writes on "Rodents—a Study in Evolution" and surveys the problems implied in our scanty knowledge of the palæontological history of this group (Vol. 1, p. 154). Other articles, the titles of which are a sufficient indication of their content and interest, are: D. Lack on "Natural Selection and Family Size in the Starling" (Vol. 2, p. 95); E. H. Colbert on "Evolution of the Horned Dinosaurs" (Vol. 2, p. 145); and B. Schaeffer on "The Origin of a Mammalian Ordinal Character" (Vol. 2, p. 164).

PHYSICAL ANTHROPOLOGY. By N. A. Barnicot, B.Sc., University College, London.

The previous article (Science Progress, **36**, 702, 1948) outlined some of the chief advantages of blood-group work for the study of racial origins, and gave an account of the Rhesus antigen system and its applications in this field. Since 1939 data has continued to accumulate from many parts of the world on the distribution of ABO blood-groups. Only a few of the publications on this subject can be selected for attention in this short review.

Many thousands of individuals throughout Great Britain were ABO-grouped by the Emergency Transfusion Service in the war period, and analysis of these very extensive data has revealed interesting regional differences. Fisher and Vaughan (*Nature*, **144**, 1046, 1939), using data from the Slough area, where there has been

a good deal of recent immigration, showed that the sample of donors with Welsh surnames was significantly low in groups A and AB. Fraser Roberts (*Nature*, **149**, 138, 1942) pointed out a similar situation in his Bristol data, in which the group with Welsh, Highland and Irish surnames was relatively low in A and high in O. He gives figures showing the higher frequency of O in the North of England (48 per cent.) than in the South (45 per cent.) and the still higher frequencies in Scotland (52 per cent.) and Ireland (56 per cent.). He also found that the A frequency in South Wales was as high as in the South of England, while in North Wales it was lower. Fraser Roberts (*Ann. Eug.*, **14**, 109, 1948) has recently analysed data from the south-western counties of England. He found no significant differences from the south-eastern counties; group A was 44 per cent. In confirmation of his previous work he showed that in counties adjacent to South Wales the donor group with Welsh names did not differ from the rest of the sample. Nor was Cornwall, at one time a Keltic-speaking area, and one with high frequencies of dark hair and eyes, found to be distinguishable from the other counties. Breaking the data into age-groups from 18 to 60 years, he found no evidence of a change of ABO frequency distribution with age, such as would presumably be evident if there were a selective mortality in this stretch of life. It must be assumed, of course, for this argument to be valid, that no major population shifts involving different age-groups selectively is likely to have occurred. Fisher and Taylor (*Nature*, **145**, 590, 1940) draw attention to the fact that the Scandinavians of today have some of the highest A frequencies in Western Europe, while the frequencies of A in Scotland and Iceland, both of which are known to have been settled from Norway, are much lower. They conclude that the modern Scandinavians do not represent the population which emigrated to Scotland and Iceland at all closely. Some of Fisher and Taylor's figures for $\frac{A}{A+O}$ per cent. are reproduced below:

Norway . .	58·0 per cent.
Southern England	48·8 ,, ,,
Holland . .	48·6 ,, ,,
Scotland . .	39·7 ,, ,,
Iceland . .	36·6 ,, ,,

Haldane (*Human Biol.*, **12**, 457, 1940) makes a similar observation, but also points out that Irish immigrants may have played a more important part in the ancestry of the Icelanders than is generally supposed. He also notes the high O frequencies in Scotland, Wales

and Ireland, and associates Sardinia and Sicily with them in this respect. The Basques, too, have a high O frequency (50·7 per cent. as calculated from Chalmers, Ikin and Mourant's recent data on persons of unmixed Basque descent; *Nature*, **162**, 27, 1948), but in addition they are extremely low in group B (1·9 per cent.), and some North African peoples, such as the Shluh of the Atlas Mountains and the Saharan Tebu, are fairly close to them in ABO pattern. It is thought that all these populations are peripheral relics of peoples who occupied Europe prior to the influx of B from the East and perhaps as early as the Upper Palæolithic. The Rh data on the Basques, which were mentioned in the previous article, go to support this conception, though there appears to be no somatological resemblance between the Basques and the known skeletal remains of the post-glacial period. Coon, however (*Races of Europe*, 1939), presents some evidence of the survival of Upper Palæolithic and Mesolithic morphological features in Iceland, the West of Ireland, parts of Wales and in some Berber peoples of North Africa (though the Shluh are said to be classical Mediterraneans in somatology). The various sources of evidence, therefore, seem to converge to a common interpretation, and it is remarkable to find that, even within the British Isles, the remnants of such archaic populations can still be detected.

Blood-group data from Africa are still insufficient, but Elsdon-Dew's summary of the existing ABO material (*Publ. S. Afr. Inst. Med. Res.*, No. 44, 1939), to which he adds his own on the Southern and Eastern Bantu, reveals some interesting and perplexing facts which are not always easy to harmonise with prevailing views on the affinities of African peoples. He presents a number of co-ordinate plots of the frequencies of the three genes (p, q, r) involved in the ABO system, which makes the comparison of populations easy to the eye, and he represents each sample as a circle of 70 per cent. probability. His statistical treatment is more thorough than unfortunately obtains in many racial studies on blood-groups even today. To mention only a few salient points; the Lacustrine Bantu and certain of the Southern Bantu are remarkably low in A and B and high in O. If they are regarded as hybrids between a negro population and some caucasoid one, as the skin colour and certain facial features suggest, then none of the populations of negroes considered here (for example, from the Congo, the Guinea Coast or the Sudan) could be ancestral to them, since all these are too high in A and B. Certain North African peoples, however, such as the Shluh, Tebu and Toureg, already mentioned, are rather similar to these Bantu in ABO groups, and some such stock might

perhaps have introduced the European morphological features without changing the ABO frequencies appreciably. The Bushmen and Hottentots are rather widely separated, the latter having much the higher B frequency. The Western Sudanese are intermediate between the West Coast negroes and the Egyptians, which is consistent with the conception of a westward movement of caucasoids through the grasslands of the Sudan. The Bambuti, pygmies of the remote Ituri forest zone, lie close to the Egyptians with high frequencies of both A and B, so that presumably there were at least two centres in Africa from which the gene for B may have spread. Jadin (*Mem. Inst. Roy. Coll. Belge*, **10**, 3, 1940), in a detailed study of the widely scattered pygmy populations, finds that sedentary groups associated with Bantu tribes may resemble the latter fairly closely in blood-groups, and diverge widely from the Bambuti who are still primitive hunters.

Turning now to South-Eastern Asia, there are various groups of people living in the mountainous regions of Southern China and Indonesia who are considered to represent earlier inhabitants of these parts. Woo (*Amer. J. Phys. Anthrop.*, N.S., **5**, 429, 1947) examined the serology of four of the isolated mountain peoples from South-Western Kweichow; of these the Chung-Chia, who differ from the Chinese linguistically and are said to be derived from the Shan of Burma, are similar in ABO frequencies to the Annamites and to Chinese in Canton (Dormann), which appears consistent with such an origin; this also applies to the Chinese-speaking Lung-Chia. The Pa-Miao, with their higher O and B, lie closer to Northern Chinese, which confirms their own traditions of origin. Farinaud (*C.R. Soc. Biol.*, **131**, 1238 and 1236, 1939) examined several of the so-called Moï tribes from the mountains of Annam. These are peoples in whom mongoloid features are much attenuated, and the hair is sometimes wavy. They have sometimes been associated with certain other groups of this region as the Nesiot race. Some, such as the Dies, Sedang, Banhar and Cham, again lie fairly close to the Annamites and Cantonese, but the Khmers-speaking Mnong are closer to the Northern Chinese, though higher in A. A pooled group of Naga from Assam (*Res. Comm. Brit. Ass., Nature*, **144**, 714, 1939) together with the Keh-lao of Kweichow and Djarais of the Annam mountains are close to Yang's sample of Southern Chinese. On the whole, and judging only from the tribes studied in these works, the Moï show considerable heterogeneity in blood-group pattern, but none of them lies far outside the range contained within China itself.

MacFarlane and Sarkar's studies on some of the primitive peoples

of India (*Amer. J. Phys. Anthrop.*, **28**, 397, 1941) reveal a very remarkable diversity in blood-group pattern, which is not at present interpretable in any obvious way, but emphasises the ethnic complexity of India. The Paniyan (O = 20·0, A = 62·4, B = 7·6), for example, one of the typical broad-nosed, dark-skinned jungle tribes, are most readily comparable in serology with the Australians, and it is reported that the Veddah of Ceylon have a somewhat similar pattern, but no other tribe considered here is at all close to them. At different extremes are the Oraon (O = 47·1, A = 12·9, B = 34·8) and the Bhil (O = 18·6, A = 23·6, B = 41·4) in whom B probably reaches a world maximum. The authors discern an increase of B from South to North, and on the east side of India a similar gradient in A.

Boyd (*Amer. J. Phys. Anthrop.*, **25**, 215, 1939) has summarised work on the American Indians. He found that only a few tribes (5 per cent.) were pure O. He calculated that the amount of European admixture necessary to produce the observed frequencies of A and B in the majority of tribes was too great to be credible, and concluded that the Amerinds already possessed some A and B when they entered America. Matson (*Amer. J. Phys. Anthrop.*, **27**, 263, 1940) found that the ABO groups of the Cree, Beaver and Slave Indians living in territories close to the Blackfeet (A = 83 per cent.) were nevertheless very different from the latter, group A being only about 20–30 per cent.

Space does not permit much discussion of a number of papers which have appeared on the blood-grouping of extinct peoples by tests on bones and occasionally remains of muscle. The principle of the technique is the mixing of the powdered bone or muscle with sera containing anti-A and anti-B agglutinins, and the subsequent testing of these sera to see whether the agglutinins have been removed by antigens remaining in the test material. A test for group O cannot be successfully accomplished, so that a negative result is ambiguous. Boyd and Boyd (*Amer. J. Phys. Anthrop.*, **25**, 421, 1939) found that material from 1st dynasty (*circa* 4500 B.C.) Egyptian graves sometimes gave reactions for both A and B. Bones preserved in a very dry condition are most favourable, and dried or mummified muscle is better material than bone.

Refined serological technique has shown that the antigen of group A is not equally potent in all individuals. A weaker antigen, A_2, was thus recognised and now other rarer antigens, A_3 and A_4, can be distinguished by quantitative differences in their agglutination properties. Antigen A_2 has been shown to depend on a gene p_2 which is an allelomorph of the gene p_1 for the stronger-reacting

antigen A_1. The genes controlling the production of the A antigens are thus seen to comprise a typical series of multiple allelomorphs. The distribution of A_2 has already yielded information of interest to anthropologists. Some of these results are tabulated below, A_2 being expressed as a percentage of total group A, *i.e.* $(A_1 + A_2)$:

Ikin, Prior, Race, Taylor (1939)	England	21·9
Race *et al.* (1948)	Latvia	7·25
Boyd (1937)	Basque	39·0
Wiener, Sonn, Belkin (1945)	Negro, U.S.A.	35·4
Wiener, Sonn, Yi (1944)	Chinese, U.S.A.	0·0
Levine and Wong (1943)	*ditto*	3·0
Wiener, Sonn, Belkin (1945)	Indians (Moslem seamen)	8·0
Simmons *et al.* (1944)	Australian aborigines	0·0
ditto	Papuans	0·0
ditto	Fijians	0·0
ditto	Javanese	0·0
ditto (1946)	Maori	0·0
Landsteiner, Wiener, Matson (1942)	Amerinds (Washington State)	0·0
Matson, Piper (1947)	Amerinds, Ute	0·0

The high value for the Basques is again a notable feature of these serologically remarkable Europeans. The Latvians on the other hand are found to be remarkably low in A_2 and emerge as another rather distinct group; Rh results, however, show no striking divergence from the rest of Europe. It is to be noted that Wiener and his co-workers reported in another paper a value of only 18 per cent. for a group of Negroes in U.S.A. which they considered particularly free from white admixture, so that further data from Africa itself is desirable.

The MN system of antigens is inherited independently of the ABO and other known systems, and depends on two alleles, m and n; no corresponding agglutinins occur in the serum. The heterozygote, mn, is intermediate in the sense that both antigens are produced. Taylor and Prior (*Ann. Eug.*, **9**, 97, 1939) collected the available data on the racial distribution of MN and made a statistical examination of the results.

If the hereditary basis of the MN system is as stated above, and the sample is drawn from a random-mating population, then the frequencies of the phenotypes should be in an equilibrium such that the heterozygous class does not exceed 50 per cent. of the total. Taylor and Prior found that it differed significantly from this value in several sets of data, and they attributed this to errors in serological technique. The same result might occur if the sample was not homogeneous and, in fact, included an immigrant group with

a different MN frequency pattern. Furthermore, the possibility of selective differences between the MN genotypes cannot be excluded, for Haldane (*Proc. Roy. Soc. B.*, **135**, 147, 1948), examining the progeny from matings of the type MN × MN, found a significant deficiency in number of offspring and suggested that prenatal elimination of some of the homozygotes might have occurred. He notes, however, the uniformity in the racial distribution of MN in comparison with ABO and feels that the question of selection in the former needs further elucidation. In spite of these complications significant differences between races with regard to MN undoubtedly exist. In general the frequency of genes m and n in Western Europe is approximately equal; Race and his co-workers (*Ann. Eug.*, **14**, 134, 1948) have recently shown, however, that m is considerably higher than n in Latvia, and, they state, in other Eastern Baltic countries. This region is well known in anthropology for the high frequency of ash-blondism. n is high in Australian aborigines, Melanesians and Ainu, while conversely m is high in Amerinds, Eskimo, Indians and some Bedouin. The table presents some of the more recent and striking results in terms of the gene-frequencies expressed as percentages.

		m.	n.
Taylor, Prior (1938)	English	52·4	47·6
Race *et al.* (1948)	Latvian	67·15	32·85
Wiener, Sonn, Belkin (1945)	Whites, U.S.A.	54·0	46·0
ditto	Negro, U.S.A.	58·0	52·0
Levine, Wong (1943)	Chinese, U.S.A.	50·6	49·4
Wiener, Sonn, Belkin (1945)	Indian (Moslem)	62·2	37·8
Fabricius-Hansen (1939)	Eskimo (Eastern Greenland)	91·3	8·7
Landsteiner, Wiener, Matson (1942)	Amerinds (Washington State)	76·3	23·7
Birdsell, Boyd (1940)	Australian aborigines	17·8	82·2
Simmons *et al.* (1944)	Papuans	9·0	91·0
ditto	Fijians	27·5	72·5
ditto	Javanese	55·0	45·0
Kubo (1936)	Ainu	43·0	57·0
Waller, Levine (1944)	Japanese	56·7	43·3

Birdsell and Boyd (*Amer. J. Phys. Anthrop.*, **27**, 69, 1940) found that the area of highest n lies in Central Australia and coincides roughly with the region of highest A frequency. They suggest that a gradient once existed in East Asia with high n in the South and high m in the North, so that the Ainu and Australians represent remnants of the southern end of the gradient, the Amerinds and Eskimo of the northern end. The Ainu and Australians have, of course, been associated before by some physical anthropologists

on rather slender morphological grounds. The rather high B in the Ainu contrasts strongly with the Australian values, and is perhaps due to Japanese admixture which may also have raised the M frequency. MN data for such peoples as the Veddah, the Malayan Semang and the Indian jungle tribes might be of great interest.

It remains to mention two genetically simple characters which have not yet been used so extensively in anthropological work, namely, "non-secretor" and "non-taster." In Europe about 80 per cent. of individuals belonging to groups A, B or AB have the antigens which they carry on the red-blood cells in the saliva and other body-fluids also. The remaining 20 per cent. do not have them, even though they are present on the red-blood cells, and they are called "non-secretors." Genetical analysis shows that the latter are homozygous for a recessive gene, s, the normal allele of which is denoted by S. Schiff (*Amer. J. Phys. Anthrop.*, **27**, 255, 1940) found that a sample of New York Negroes, which he considered particularly free from white admixture since their ABO frequencies were close to those of the Yoruba of Nigeria, consisted of 38·8 per cent. non-secretors in comparison with 17·6 per cent. for New York Whites. In the Ute Amerinds, Matson and Piper (*Amer. J. Phys. Anthrop.*, N.S., **5**, 357, 1947) found no non-secretors.

Another trait determined by a single recessive mutation is the inability to taste the very bitter substance Phenyl-thio-carbamide (P.T.C.). Non-tasters are homozygous for the recessive gene t. A number of racial groups have been tested. While in Europe 30–40 per cent. of the population are non-tasters, in Chinese, African negroes and Amerinds the proportion is lower, ranging from 5 to 10 per cent. It appears, however, that the technique of different investigators has not always been uniform, some using dilute solution of P.T.C. and others a crystal placed on the tongue. Since non-taster is not an absolute category, but individuals vary in the threshold for taste, it is important to use a standard technique, preferably with a range of concentration if valid comparisons between different populations are to be made.

NOTES

"Australian Journal of Scientific Research"

The Australian Council for Scientific and Industrial Research, together with the Australian National Research Council, has assumed responsibility for the establishment of a new scientific journal, the *Australian Journal of Scientific Research.* The journal is to be used for the publication of research papers of outstanding merit, and is to be printed in two series : Series A (Physical Sciences) and Series B (Biological Sciences). The journal is open to receive contributions from all research workers, irrespective of the country or organisation to which they belong.

The first issue (March 1948) of the Physical Sciences series is to hand, and contains nine papers on various subjects. The standard of the publication is extremely high and compares very favourably with similar publications in other countries. One paper describes an investigation into the effect that a horizontal glass plate has on the motion of a small drop of oil falling towards it. This work is of importance in connection with the oil drop method of determining the electronic charge, and the range for which the Lorentz formula holds is discussed.

Another paper describes the deformation and recrystallisation of a duplex brass alloy and contains eight plates of photomicrographs and X-ray photographs. The subjects of the other papers include the lubrication of metal surfaces by silicone films, observations on a variable source of cosmic radio frequency radiation, and a study of the diffraction of light from sources of finite dimensions, including a discussion on the application of the theory to the optics of the telescope and the spectroscope.

This new journal covers a wide field and should prove of interest to many : it is certainly a valuable addition to modern scientific literature.

"The German Hydrographic Journal" (J. P.)

The first number of *The German Hydrographic Journal* was published in January 1948, and it is the intention of the publishers

to issue about six parts per year. It is published by The German Hydrographic Institute at Hamburg, with the authority of the Allied Control Council. It appears to continue, in a measure, the purposes served by the *Annalen der Hydrographie und Maritimen Meteorologie*, which was published by the *Deutsche Seewarte* at Hamburg, and which came to an end with the issue of the 72nd annual volume in 1944.

The new journal covers the same subjects as the Institute, and these are: navigation; nautical geodesy; cartography; sea-surveying; dynamical, physical and chemical oceanography; and terrestrial magnetism. This list differs from that covered by the former *Annalen* in the omission of maritime meteorology. The *Journal* is a channel for the publication of the results of research in these subjects, and it is much to be hoped that it will be able to maintain the high standard of the *Annalen*. The writers of the main articles in the first four parts are: P. Christoph, F. Errulat, H. C. Freiesleben, W. Hansen, W. Horn, F. R. Jung, K. Kalle, O. Meyer, F. Model, G. Neumann, F. Nusser, O. Pratje, E. Tams, G. Tomczak, G. Wüst. Among the articles giving the results of research there appears to be a slight tendency to include articles of an encyclopædic nature.

British oceanographers will envy German oceanographers the possession of such a channel of publication, with the stimulus to research and the consolidation of a subject which it brings. In Great Britain there was no periodical corresponding to the *Annalen*, and there is still no periodical corresponding to the *Journal*. A British periodical for the results of research in dynamical, physical and chemical oceanography and related subjects is much to be desired. Though the excellent media of the *Proceedings* and *Transactions* of the Royal Society, the *Geophysical Supplement* of the Royal Astronomical Society and the *Philosophical Magazine* are open, yet there are great advantages in more specialised journals.

Conseil Permanent International pour l'Exploration de la Mer (E. F.)

Faxa Bay, on the west coast of Iceland, constitutes an appreciable proportion of the nursery area for halibut, plaice and haddock in Icelandic waters. Unfortunately, a considerable amount of commercial trawling and seine-netting, mainly by English and Icelandic fishermen, takes place in the Bay, which undoubtedly takes great toll of the young of these valuable economic species. A Sub-Committee appointed by the International Council has recently

issued a most informative and valuable report,* comprising 26 short papers, together with the recommendation that the Bay should be closed to trawling and seine-netting for a period of 15 years. During the first 10 years, the Bay would be under the protection of international police, and during the final 5 years the effects of the temporary closure would be used as a basis for ratification of any agreement concerning permanent closure or a restoration to open fishing. It is clear from the report that this recommendation is beset with difficulties. There is the question of compensation for the fishermen debarred from trawling and seining during the experiment; the assurance of effective international policing; as well as some division of opinion as to the need for the closure of the whole Bay. The report, however, provides most weighty evidence, which certainly commands serious attention by all the fishing nations concerned.

The *Annales Biologiques* serve a most useful function in bringing together in one volume each year an account of the work of each of the fishing nations represented on the International Council with regard to the fluctuations of fish stocks and fishing conditions in the different fishing regions. The present volume † includes a set of diagrams showing the variation in length and age from year to year for the sexually mature component of the Arcto-Norwegian stock of cod—the "skrei" as they are called. The length data, collected yearly without a break from 1913 to 1947, show the successive waves of incoming recruits, with their varying intensities expressed as deviations above or below a datum line for the whole period of years covered. It is evident that fish of the year-class 1937 are now dominating. They appeared as 6-year-old fish in the stock of the year 1943, and became the predominant element by 1946. They are likely to remain of first importance for some years to come.

In a comprehensive account of the English Plaice Fishery in the southern North Sea and English Channel during 1946, it is estimated that a total of 2130 trawl fishermen from Grimsby and Lowestoft caught 55,033,000 plaice, weighing 457,550 cwt., giving a catch per 100 hours' fishing of 88 cwt. This means that each fisherman was responsible for catching 210 cwt. of plaice.

* *Rapports et Procès-Verbaux des Réunions*, Vol. CXX, *North-Western Area Committee, Report of the Sub-Committee on Faxa Bay.* [Pp. 129, with 36 figures.] (Copenhagen: A. F. Høst & Sons, 1948. Kr. 14.–.)

† Vol. III, 1946. [Pp. 115, with numerous figures.] (Copenhagen: A. F. Høst & Sons, 1948. Kr. 12.–.)

The re-appearance of the *Journal du Conseil* * after the war will be generally welcomed. It retains its old form and brings up to date its valuable Bibliography, classified under 11 headings, namely, General Oceanography, Hydrography, Plankton, Organic Production, Bottom Fauna and Flora, Fish, Marine Fisheries, Marine Fishery Investigations, Freshwater Fisheries and Fishery Investigations, Shellfisheries, and Miscellaneous. Dr. J. N. Carruthers, of the Hydrographic Department of the British Admiralty, makes practical proposals for a continuous programme of thick-layer current measuring in all weathers, and pictures the perfect lightship from the viewpoint of the student of water movements in the sea. One sad thing about this first issue is the three-page list of distinguished scientists who died during the war period—men known to marine workers everywhere, and who will be greatly missed, especially by the International Council.

Wood Destroying Insects (F. W. J.)

Several papers by J. M. Kelsey on wood boring insects in New Zealand have been published in the *New Zealand Journal of Science and Technology*. In Vol. 27 (May 1946) the results of investigations with three water-soluble and three oil-soluble preservatives are recorded, the test insects being the Furniture Beetle (*Anobium punctatum*) and a native Termite (*Calotermes brouni*). The tests indicate that water-soluble preservatives are ineffective as repellants during oviposition in *Anobium*, while the oil soluble ones are satisfactory for this purpose and were likewise toxic to the beetles. Certain preservatives like kerosene and orthodichlorobenzene are toxic to insects in timber, but ineffective in preventing future infestation. For both the beetle and the termite brush treatment with oil-soluble preservatives was satisfactory. Oil-soluble preservatives, however, do not stop tunnelling immediately, for it was found that *Anobium* larvæ continued to tunnel in timber so treated for nearly three weeks, although the wood does not appear to have been eaten but merely chewed. Experiment also showed that some larvae of the furniture beetle can live for over a month without food; hence in poisoned timber larvæ may not be killed at once, but only later, when forced by hunger to eat the wood. Two subsequent articles (Vol. 28, Nov. 1946) give the results of tests with specific preservatives on the wood of Montery Pine (*Pinus radiata*).

A long paper on insects attacking milled timber, poles and

* Vol. XV, No. 1. [Pp. 118, with 22 figures.] (Copenhagen: A. F. Høst & Sons, 1947. Kr. 16.– per volume.)

pests appears in Vol. 28 (Sept. 1946). It aims at giving rapid field methods for the identification of the commoner xylophagous insects in New Zealand—termites and beetles. Identification of the termites is confined to the three habit groups—dry wood, damp wood and subterranean termites—but the beetles are treated in greater detail, and the important genera, such as *Anobium*, *Ernobius*, *Lyctus* and *Platypus*, are described in all their stages, while details of the type of boring and of the contents of the burrows also help in identification. The paper also deals with methods of control of these pests.

Miscellanea

The Lord President of the Council has appointed the following to be members of the Advisory Council for Scientific Research from October 1, 1948: Mr. James Bowman, Vice-President of the National Union of Mineworkers, Mr. E. Fletcher, Secretary of the Research and Economic Department of the T.U.C., and Dr. H. W. H. Warren, Managing Director of Associated Electrical Industries, Ltd. The new members succeed Mr. J. Benstead, C.B.E., and Mr. E. Thornton, who retired on completion of their terms of office on September 30, and Sir Clifford Paterson, who died on July 26, 1948.

Dr. W. A. Macfarlane, director of fuel efficiency in the Ministry of Fuel and Power, has been lent by the Ministry to the Department of Scientific and Industrial Research for about two years for appointment as director of the United Kingdom Scientific Mission in the British Commonwealth Scientific Office, and as attaché for scientific matters in the British Embassy, in Washington. He succeeds Dr. F. N. Woodward in this office.

Dr. A. E. Trueman has been appointed to be chairman of the University Grants Committee in succession to Sir Walter Moberly, who retires on October 1, 1949. Dr. Trueman is at present deputy chairman of the Committee.

Sir John Russell, O.B.E., F.R.S., has been elected president of the 1949 British Association meeting at Newcastle-on-Tyne.

It is with much pleasure that we have learnt of Dr. W. O. Kermack's appointment to the chair of biological chemistry in the University of Aberdeen. Dr. Kermack is a valued contributor to SCIENCE PROGRESS, and we wish him every success in his new position.

We have noted with great regret the announcements of the death of the following scientific workers during the past quarter: Mr. T. R. D. Bell, C.I.E., formerly chief conservator of Forests,

Bombay Presidency; Mr. H. Brearley, the inventor of stainless steel; Mr. S. G. Brown, F.R.S., known for his work on the gyroscopic compass and numerous other mechanical instruments; Mr. H. E. Burton, recently principal astronomer and head of the Equatorial Division of the U.S. Naval Observatory; Mr. J. R. Cowie, chairman of Council of the British Electrical and Allied Industries Research Association; Major Sir Thomas Crozier, formerly H.M. Chief Inspector of Explosives at the Home Office; Dr. W. Cullen, well known for his work on mining engineering and explosives; Prof. Beatrice Edgell, emeritus professor of psychology, Bedford College, University of London; Dr. P. R. Lowe, O.B.E., formerly keeper in charge of ornithology, British Museum (Natural History); Prof. A. Orlov, professor of petrography and chemical mineralogy at the Charles University of Prague; Sir Clifford Paterson, O.B.E., F.R.S., director of the Research Laboratories of the General Electric Co., Ltd.; Prof. A. F. C. Pollard, emeritus professor of physics, Imperial College of Science and Technology; Dr. A. Smith, assistant mycologist at the Plant Pathology Laboratory, Harpenden; Prof. S. W. J. Smith, F.R.S., emeritus professor of physics, University of Birmingham; Prof. D. A. Welsh, emeritus professor of pathology, University of Sydney.

Many accidents can be attributed to the slippery state of pavements, corridors, floors in buildings, etc. While haste and carelessness on the part of the users is often a contributory factor, it has recently been felt in America that the number of accidents could be reduced and that data should be compiled with a view to providing safer surfaces. A report of the work done and the results obtained is given in the May 1948 issue of the *Journal of Research* of the U.S. National Bureau of Standards.

As the correlation between the coefficient of friction as commonly measured and the slipperiness as actually experienced is poor, particularly for wet surfaces, it was necessary to develop a simple but reliable instrument which would give a value of the slipperiness of surfaces as actually experienced. The effect of different types of footwear material had to be considered, and it was important that the tests should be made on surfaces which were actually in use.

The design of the instrument was based on the assumption that slipping is most likely to occur as the edge of the heel meets the ground. The instrument consisted of a rigid pendulum pivoted in a framework which could be placed on the surface under test. The angle that the arm of the pendulum made with the vertical could

be read directly on a scale mounted behind the pendulum. The bob of the pendulum was very carefully designed and was known as the mechanical heel. By means of it one edge of a 1½-inch square test piece of heel material was made to drag along the surface during the lower part of the pendulum's swing, the test piece being forced against the ground by a spring in the heel itself. The angle at which the test piece met the ground could be adjusted to simulate the characteristics of different peoples' walk, which had previously been studied by means of slow-motion pictures. By noting the angles that the arm of the pendulum made with the vertical at the beginning and end of its swing, the slipperiness of the surface could be deduced.

It was found, as might be expected, that rubber heels gave higher anti-slip properties than leather ones. Some surfaces when wet were distinctly dangerous for both rubber and leather heels. Good anti-slip properties under wet conditions were found for surfaces where rough particles projected through the film of water and prevented its action as a lubricant. The effect of waxing and polishing floors was also noted, as well as the way in which the slipperiness of such surfaces varied when exposed to normal traffic for a few days. The results often confirmed the impressions of employees of the establishments where the tests were made, and should prove of value in making future recommendations for safer surfaces.

In an article in the July 1948 issue of the Indian *Journal of Scientific and Industrial Research*, P. V. Mehta reviews many of the modern instruments which are in common use in America for recording and controlling industrial processes. Mehta, who is himself employed in American industry, frequently points out in his article ways in which the Indian industrialist can profit by adopting similar up-to-date methods.

Temperature is one of the process variables most commonly measured and controlled, usually by means of the self-balancing electronic potentiometer. This instrument measures temperature by means of a thermocouple, the E.M.F. of which is balanced along a slide wire in the usual way. If there is any lack of balance, the resultant unbalanced voltage is amplified and, by means of a small motor, made to move the slider to the balance point. The movement of the slider is recorded and this movement can be made, with the help of electrically or pneumatically operated mechanisms, to keep the temperature constant. Such a system will correct immediately any small fluctuations in temperature, is a great

improvement on manual control and results in higher efficiency and more uniformity of product.

Other instruments have been designed to record and control pressure, as well as the rate of flow of liquids or gases. The method used in the latter case is to insert in the flow line a small, accurately bored orifice plate which creates a differential pressure across itself. The rate of flow is proportional to the square root of the differential pressure, which is measured by means of a mercury manometer. A small float resting on the mercury can be used to record the pressure directly as in the "Mechanical" Meter, or remotely by electrical means.

U.S. textile plants have made full use of instruments both for keeping the humidity of the air at the correct level and also, by means of a *p*H meter, in controlling the acidity or alkalinity of the process liquors. Few instruments are at present used in the Indian textile industry, and this is but one example where their adoption would be of great assistance and lead to higher efficiency and output.

Many boilers in the U.S. are fitted with instruments for automatic control. A meter in the output measures the carbon dioxide content, and, by controlling the air draught dampers, maintains the value at 14 per cent., which is the criterion for complete combustion. Oil-fired boilers use photo-electric cells to detect the presence or absence of burner flames, and these cells are made to operate relays to shut off the fuel supply in the event of a flame failure. Most boilers are also fitted with temperature, pressure and flow recorders to ensure correct operation and the most economical use of fuel.

The introduction of these modern methods of control in India may possibly be handicapped by the cheap labour available in that country. In the U.S. the accent is on the smooth running of mass-production processes at the utmost possible efficiency, with labour required only for very simplified work and supervision; in fact, in one distillery plans have been drawn up for running the whole plant with one operator and a control panel 2 ft. by 2 ft.! Perhaps for India such mechanisation may be a very distant goal, but improvement in the operation of the more important unit processes upon which depend the quality and quantity of the final product is a practicable possibility.

A very neat and simple method of forming points on to the ends of very fine tungsten wires has recently been developed in the Bell Telephone Laboratories ("Electropointed Tungsten Wires," by W. G. Pfann, *Bell Laboratories Record*, May 1948). It is a marked improvement on the older method of first grinding a point on to

the wire with a suitable abrasive and then electrolytically polishing the point to remove burrs and small irregularities. The problem arose in connection with the mass production of silicon and germanium point contact rectifiers. In these rectifiers a pointed tungsten wire, thinner than a human hair, makes a delicate contact on the surface of the semi-conductor. The quality of the rectifier depends upon the nature of this contact.

In the new process, the tungsten wire is made the anode of an electrolytic cell, and is supported vertically above the cathode. As the immersed portion of the wire is eaten away, it remains cylindrical in shape except for the small region near the surface of the liquid which is wetted by the meniscus. In this region the wire is not eaten away so rapidly, and as a result it tapers to the thinner cylindrical shape which is below the surface of the liquid. As the process continues this cylindrical section disappears, leaving the end of the wire tapering to a sharp point. Any degree of sharpness can be obtained by switching off the current at the appropriate time.

The point formed has a high lustre which can be improved by increasing the voltage for a fraction of a second just before the completion of the process. The electrolyte used is an aqueous solution of potassium hydroxide, to which is added a little copper to improve the stability of the meniscus. The method was used extensively during the war, and should be suitable in principle for use with other metals.

After many years of service it seems probable that the vacuum type of radio valve will be replaced, at least in many of its applications, by a much simpler device known as the Transitor. The physical principles upon which this new device depends were discovered in the course of a programme of research into the electrical properties of solids, particularly semi-conductors, at the Bell Telephone Laboratories. An account of this new development is contained in the *Bell Laboratories Record* for August 1948.

The Transitor, which can serve both as an amplifier and an oscillator, bears little resemblance to the type of valve at present in use. It consists simply of two very fine wires touching a small piece of a solid semi-conductor, about the size of a pinhead, which has been soldered to a metal base. The whole unit can be made extremely small, and the outer metal case is little larger than a shoe-lace tip. That it requires no vacuum, no glass envelope, no grid and no heated cathode are obvious advantages over the older type of valve.

The two fine wires, which are made to touch the semi-conductor

at points only a few thousandths of an inch apart, form the terminals of the input and output circuits, the other ends of which are connected to the metal base. The input terminal is held at a small positive potential, the output at a larger negative potential, relative to the base. With this system, input power can be amplified as much as a hundred times, while the power consumed by the Transitor itself is extremely small, being about a tenth of that consumed by a flashlamp bulb. The underlying principle of the Transitor is explained by the discovery of Dr. Bardeen and Dr. Brattain that for a very small area around the input point the electronic structure of the semi-conductor is modified by the input current. If the output contact is placed in this area, then the output current is controlled by the input, which is the essential requirement for an amplifier.

Demonstrations indicating some of the possible uses to which the device may be put included small amplifiers and oscillators built with Transitors, as well as a superheterodyne receiver in which all the vacuum type of valves had been replaced by Transitors. The Transitor will probably find some applications for which the vacuum type of valve is not suitable. At the present time the limitations of the Transitor are an upper frequency limit of a few megacycles and a comparatively small power output.

The research and development work on microwave propagation, aerials, filters, and repeaters, carried out during the last ten years by the Bell Telephone Laboratories, is summarised in an article by H. T. Friis in the *Bell System Technical Journal* for April 1948. Particular emphasis is laid on the engineering design of a complete microwave communication link, of bandwidth suitable for a television channel, over a distance many times greater than the line-of-sight path and thus requiring intermediate repeater stations. Propagation studies at wavelengths from 1 cm. to 2 m. are reviewed, and the band 5–10 cm. is chosen as most suitable for a repeatered circuit. Repeater gain and output are considered in terms of distance and noise characteristics. Research on suitable aerials is described, including metal strip lenses and conducting lattice lenses, as well as orthodox parabaloids and horn reflectors. Repeater equipment described includes frequency-changers, I.F. amplifiers, microwave amplifier and the waveguide and cavity analogues of bandpass and bandstop filters and hybrid coils.

The behaviour of the public when using the telephone, or rather when attempting to use the telephone and finding the line engaged,

was studied in New York during the war and an account of the results obtained is given by Charles Clos in the July 1948 issue of *The Bell System Technical Journal.*

During the war it became necessary to lower the standard of service provided, including the number of lines available for a given volume of traffic, and this survey was made to discover the overall effect of the reduction. It was concerned not so much with operator-handled calls, where congestion can be controlled, but with subscriber-dialled calls. With the latter the subscriber can make repeated attempts to get through and is, of course, not controlled in any way at all.

A special record was made in the New York City area of over a thousand calls where the line was found to be engaged on the first attempt. The time interval between successive attempts was noted as well as the final fate of the call. It was found that new attempts were made sooner and much more often than was generally realised: in fact, one subscriber was found to make twenty-five attempts in about one hour.

The results obtained, when plotted to show the percentage of subscribers who made additional attempts within a given time interval, were found to lie on smooth curves. 90 per cent. of those who found the line busy redialled their calls, the other 10 per cent. abandoning their calls altogether. The pattern of time intervals between successive attempts and other data obtained during this survey will probably have direct application in the development of techniques for handling subscriber-dialled traffic when line shortages occur in the future.

ESSAY REVIEW

THE TURBELLARIAN THEORY OF THE CNIDARIA. By G. S. CARTER, M.A., Ph.D., Lecturer in Zoology, University of Cambridge. Being a Review of "**Turbelarijska Teorija Knidarijev,**" by J. HADZI, *Slov. Akad. Znan. Umet., Ljubljana, mat. prir. Razred*, 1944, Pt. 3, 1–238 (in Serbo-Croat, with a 40-page German summary).

IN the article here reviewed we have a reconsideration by Prof. Hadzi of the phylogeny of the simpler groups of the Metazoa.

Fifty years ago the current ideas of phylogeny were based very largely on the assumed general truth of Haeckel's theory of recapitulation, which stated that the phylogeny of an animal is recapitulated in its ontogeny. The egg was believed to represent a unicellular phylogenetic stage, the blastula a simple spherical colony, and the gastrula a two-layered, diploblastic individual (gastraea) in much the form of a cœlenterate polyp. The early stages of the evolution of animals were believed to have followed this course. The simplest and earliest Metazoa were thought to have been diploblastic organisms which evolved from some colonial protistan form, such as a colony of Volvox may be taken to represent. The theory thus led to belief in direct evolution of the cœlenterates from unicellular forms, and in evolution of the platyhelminths and other triploblastic groups of the Metazoa from diploblastic forms at the cœlenterate level of organisation. Among the cœlenterates, Hydra and similar forms were taken to be the most primitive on the ground of their simple life-history, though it was always disputed whether radial symmetry originated in adaptation to sessile or planktonic life, whether the polyp or the medusa was most primitive.

This theory was not without its difficulties. It required the evolution of a new and complete single individuality in the metazoan to replace the multiple individualities of the cells of the protistan colony. Also, doubts were often felt of the soundness of regarding forms such as Hydra as the most primitive cœlenterates; both in their histology and their behaviour they stand nearer the top of the group than the bottom.

Prof. Hadzi first separates from the cœlenterates not only the sponges, which are nowadays almost always treated as a separate line of evolution from the Protista and called the Parazoa, but also the ctenophores which he, with many zoologists, believes to resemble the cœlenterates only by parallel evolution, having had a separate origin from platyhelminths. He is left with the true cœlenterates, the Cnidaria, the Metazoa that form nematocysts.

Hadzi then points out that in recent years the part that recapitulation is believed to have played in determining the ontogenies of animals has been much reduced. It was never denied that ontogenesis is modified to some extent in all animals by adaptive change, but nowadays we believe that recapitulation—together with other types of ontogenetic change—plays a smaller part than it was formerly thought to do, and adaptation a much larger part. It is not clear that the blastula and gastrula represent ancestral stages in the evolution of Metazoa; rather they may represent stages in the direct course of elaboration in the developing organism towards later structure—the egg develops in this way because it is the simplest and most direct path towards the structure it is later to attain. If this is so, the whole foundation of the classical theory of the phylogeny of the earliest Metazoa is destroyed, and a new approach is needed.

Hadzi approaches these questions from an angle different from that taken in the classical theory. He says first—what must surely be true—that in an aquatic environment the natural way of life for an animal of the size and simplicity of an early metazoan is on the bottom. For an animal of this size planktonic life needs considerable adaptation; only the smaller Protista can float without either elaborate structure adapted to enable them to do so or considerable expenditure of energy. We must think, then, of the ancestral metazoan as a bottom-living form, and not as a floating spherical colony.

Such a simple, bottom-living form would be at first much more like a turbellarian than a cœlenterate polyp. It would not be sessile, for sessile habit is never primitive among animals. It would be slow-moving to find its food, and would have evolved a mouth and gut, but not at that stage an anus. It might have evolved some tendency towards bilaterality in association with its locomotion. It would develop an intermediate cell-layer, a parenchyma, between the ectoderm and the endoderm, to give solidity and support to the body.

For such an animal two chief lines of advance are open. First, its activity might increase. With this would go better development

of the nervous system, more active muscles and better-defined bilaterality. The animal would be on the line of evolution leading to the modern platyhelminths. Secondly, it might become sessile, with loss of its original bilaterality and replacement by radial symmetry, reduction of the parenchyma and, in later stages, colony formation. This path would lead to the cœlenterates. A third possible line of advance, towards planktonic life, would need too much adaptation to be open to so simple an animal.

Hadzi believes this to have been the true phylogeny, the cœlenterates having evolved from this simple ancestral platyhelminth. When he considers which of the cœlenterate groups is nearest to the bottom-living, turbellarian-like ancestor, he finds that it is the Anthozoa, the sea-anemones and corals. In their imperfect radial symmetry, retaining traces (so he thinks) of the original bilateralism, simple histology, lack of a medusoid generation, and in other features, he concludes that they are the most primitive of the cœlenterates. From an ancestral form of the Anthozoa he believes the Scyphozoa and Hydrozoa were evolved.

This is his theory in outline. He discusses the changes that took place in each of the organs of the body as the result of the sessile habit, and attributes the characters we find in the present-day cœlenterates to these conditions. We cannot discuss the evidence he adduces for his theory from each of the organ systems. A medusoid phase he regards as a relatively late adaptation to dispersal.

Thus, Hadzi exactly reverses the direction of evolution that was accepted on the classical theory. His views differ most fundamentally from that theory in the following points: (1) he denies the probability of a spherical, blastula-like ancestral stage; (2) he does not regard the cœlenterates as primitively diploblastic—where they are so, this character is secondary; and (3) he regards the Anthozoa as the most primitive and Hydra as the least primitive of the cœlenterates. His theory requires that evolutionary change may be in the direction of simplification and not elaboration, especially where it is associated with abnormal habits such as sessility. That is undoubtedly true, for we have plenty of evidence of it from other animal groups. Whether his detailed conclusions will be accepted, especially the conclusions that the ancestral metazoan was a turbellarian-like animal and that the cœlenterates were evolved from it as a result of sessile habit, is a question that cannot now be answered; it must be left for the consideration of zoologists expert in these groups. He has at least performed useful service in emphasising that simplicity in a sessile group such

as the cœlenterates is not necessarily primitive, and also in insisting that our views of phylogeny must take account of our knowledge of the biology and ecology of animals as well as of their structure.

Hadzi attacks the classical theories of phylogeny at one other point, the relation of the multicellular metazoan to the unicellular protistan. In the classical theory it is assumed that a protistan colonial stage was interposed in the evolution of the Metazoa from the Protista, and that the Metazoan was formed by evolution of unique individuality in the colony. Another course of evolution is possible: the multicellular condition may have arisen by internal division in a single, polyenergid protistan body. Hadzi argues in favour of this view so far as the true Metazoa are concerned—there seems little reason to doubt that the sponges were colonial in origin. There is much to be said for his view, though also, some may think, much to be said on the other side. At least, the "polyenergid" theory avoids the necessity for the evolution of a new individuality, and it makes it possible for the "organelles" of the protistan to be carried over as the organs of the metazoan.

All the matters that Prof. Hadzi discusses are questions of "high phylogeny." They have been discussed for many years, and it is not likely that zoologists will soon reach general agreement on them. That is no reason why discussion of them should be discounted. They are fundamental to an understanding of animal evolution; they are therefore questions in which zoologists cannot avoid interest. It is refreshing to read a discussion of these questions in which the results of recent zoology are taken into account. For this we have to thank Prof. Hadzi.

REVIEWS

MATHEMATICS

A Philosophy of Mathematics. By LOUIS O. KATTSOFF. [Pp. x + 266.] (Ames, Iowa: Iowa State College Press, 1948. $5.00.)

IN addition to the well-known material that is included in most works on the foundations of mathematics, this book contains such things as an account of Pasch's definition of number and a summary of Gödel's proof of his famous theorems. For this reason it may be of some interest to readers who are sufficiently familiar with the subject not to be put off by the technicality of the terminology and symbolism or misled by the frequent slips and inaccuracies. It is hard to see, however, how the book can be successful as "an attempt to give the beginning student an introduction to the many problems raised by the 'queen of the sciences,'" as the author describes it.

The first thirteen chapters constitute an account of what has been said by other people about the nature of mathematics, and the method adopted is for the most part that of summary and quotation. While some of the quotations are illuminating and suggestive, many of them are quite trivial; and the uninformed reader is likely to be overawed by the sheer number of authorities appealed to. It is unfortunate that the author has merely brought together opinions about mathematics, without apparently having had any definite questions in mind when he selected and arranged them.

The last three chapters are devoted to the author's own philosophy of mathematics, and this turns out to be superficial. In the chapter headed "The Structure of the Mathematical System" we find little more than a description of the external form of an unspecified textbook of mathematics, and the following is a typical conclusion: "Mathematical proof seems to be a manipulation of sets of symbols in certain definite ways in order to attain the proposition desired" (p. 214). It would have been more profitable to take one of the acknowledged classics of mathematics and examine the processes of thought that are represented by the printed words and symbols.

The reader who is not a trained mathematician should perhaps be warned that the mathematical sections of the book, such as the chapter on "Extension of the Number System," are so confused as to be dangerously misleading. The same is also true, up to a point, of the non-mathematical sections; thus although formula 30.01 of *Principia Mathematica* is quoted correctly (p. 35) as

$$R‘y = (\iota x)(xRy) \quad \text{Df},$$

it is translated into words as follows: "we shall define $R‘x$ as the class of those x's which have the relation R to y."

G. T. KNEEBONE.

Integration in Finite Terms. By JOSEPH FELS RITT. [Pp. x + 100.] (New York: Columbia University Press; London: Oxford University Press, 1948. 15s. net.)

MOST mathematical students who know that sines and cosines can be expressed in terms of exponentials must have wondered whether other functions, such as the Gamma and Bessel functions, were really new functions or whether they could be similarly expressed in simpler terms. This question was first successfully attacked by Liouville between 1830 and 1840, and the present book makes Liouville's theory available to mathematicians, while bringing in modern standards of rigour, simplicity and generality.

Ritt starts by defining a scheme of "elementary functions." Algebraic functions are those obtainable as roots of equations with polynomials as coefficients and are called "zero-order" functions. No new functions can be obtained from these by additions, divisions, roots or other algebraic operations. The exponential or logarithm of an algebraic function is a "first-order" function, and so are any new functions obtainable by algebraic operations. The exponential or logarithm of a first-order function is of the second order and so on. In this way we obtain all "elementary" functions of finite order; for example, exp exp x is, as expected, of second order. A study of their properties shows that, among others, Gamma and Bessel functions (excepting those of half-integral order) are not elementary. A similar investigation is conducted into related questions, such as whether a differential equation can be solved by a finite number of integrations or implicit equations.

Besides the general theory, each equation studied requires its own special approach, and often much ingenuity. Thus the theory, though very interesting, is difficult to apply to specific problems.

A knowledge of the simplest properties of Riemann surfaces and algebraic functions is assumed and is necessary to follow the argument but, granted this, the methods adopted are simple and the argument is attractively and concisely set out.

CEDRIC A. B. SMITH.

Vector and Tensor Analysis. By LOUIS BRAND, Ch.E., E.E., Ph.D. [Pp. xvi + 439, with 187 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 33s. net.)

THE preface to this book and the presence of exercises at the end of each of the chapters suggest that it is meant to be a textbook for students, particularly for those interested in applied mathematics and engineering. Therefore, a presentation of the theory is indicated of the most unified character and with the least possible number of special devices in pure mathematics. The author has adopted the opposite plan: he employs the maximum number of mathematical processes and incidentally introduces as many changes of nomenclature as possible. In Chapters I and III we find excellent accounts of vector algebra and differentiation, but in Chapter II localised vectors are dealt with through the notions of dual numbers and vectors, quite unnecessary complications, and the theory is given the highly inappropriate name of "motor algebra." Again in Chapter IV dyadics are treated at length, whilst the reader has to wait until Chapter IX to discover that they are certain kinds of tensors of order two and that he need not, after all, have studied the Gibbs method in such detail. Chapters V and VI are devoted to differential invariants and to integral theorems, and Chapter X

to a concise account of quaternions. Applications to hydrodynamics and to the differential geometry of surfaces are found in Chapters VII and VIII respectively, and it may be argued that the use of dyadics instead of tensors serves to increase, rather than to diminish, their difficulty.

A long chapter on tensor analysis suffers nevertheless from an attempt to be too brief. For example, it is not explained that an "affine connection" is possible in a non-metric space and it is therefore not true, as stated on p. 362, that the coefficients of an affine connection are necessarily the same as the Christoffel symbols. It is also doubtful if a beginner will grasp, from the way in which Riemannian geometry is introduced in § 169, that a Riemann space may have a non-Euclidean geometry and that it is not merely a question of using curvilinear co-ordinates in a Euclidean space.

Some further curious features of the nomenclature may be noted. "Right-handed" and "left-handed" are self-evident terms: the author's "dextral" and "sinistral" are not; nor is there any point in calling the algebra of matrices "matric" algebra; and, finally, both in this country and in America the number of indices of a tensor denotes its "order" or its "rank": yet, Brand prefers the term "valence."

G. C. McVittie.

Computing Mechanisms and Linkages. By Antonín Svoboda. Edited by Hubert M. James. [Pp. xii + 359, with 178 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1948. 27*s*. net.)

Immense strides have been made in recent years in the development of computing machines. The spectacular advance in the design of digital machines, made possible by electronic devices, has been paralleled by remarkable achievements in the design of continuously acting mechanisms, which compute instantaneously the solution of specific problems, and can be made to produce a desired mechanical action functionally related to that solution. The present work, which is one of a series of valuable monographs published by the Radiation Laboratory of the Massachusetts Institute of Technology, deals exclusively with the fundamentals of design of continuously acting mechanisms. Although the author, in his preface, states that, under the pressure of war, he has been forced to concentrate on finding practical methods to meet immediate needs, he has undoubtedly succeeded in developing a fundamental and systematic analysis of the subject.

In the first chapter illustrated descriptions are given of the more standard elements of mechanical computers, including various types of differentials, slide multipliers, resolvers, cams of two and three dimensions, and integrators. A computer for a specific purpose can usually be built by suitably assembling these standard elements, but it is often possible to design a bar-linkage mechanism for the same purpose. Though the mathematical design of bar-linkage computers is relatively difficult, they possess many mechanical advantages, which make their detailed study well worth while. The remaining chapters are, therefore, devoted to the theory of bar-linkage computers, with special reference to harmonic transformers, three-bar linkages, linkage multipliers and generators of functions of one or two variables. Two valuable conceptions introduced by the author are (i) the operator formalism, leading to valuable graphical techniques, and (ii) the "grid-structure" of a function and its topological transformation, which forms the basis of the author's novel technique for mechanising a function of two independent variables.

Mention should also be made of the novel geometric and nomographic methods for first obtaining an approximation to the dimensions of a mechanism suitable for a given purpose, and then reducing the structural error of the design. The book is extremely well written and profusely illustrated. The literature in this field is small, and this book will be a valuable addition to it.

A. T. P.

METEOROLOGY

La Foudre. By CH. MAURAIN. Collection Armand Colin, No. 248. [Pp. 214, with 7 figures.] (Paris: Librairie Armand Colin, 1948. Frs. 120.–.)

THIS attractive and reasonably priced book, which might better perhaps have been titled "Thunderstorm Electricity" since the author deals with all electrical aspects of the thunderstorm, can be confidently recommended for physics libraries and to readers who have reached Intermediate B.Sc. standard in physics. Following a short chapter on atmospheric electrical phenomena in general, the author gives in the next hundred pages a descriptive account of thunderstorm phenomena—fields and field charges, the structure and properties of lightning, the direct exploration of thunderclouds and the distribution of charge within them, and the nature and electrification of precipitation. Then in the second half of the book he passes in review the various theories which have been advanced on the separation of charge in thunderclouds, on the mechanism of the lightning flash, and on the relation between thunderstorms and the maintenance of the earth's fine-weather field. He examines the relations between atmospherics and lightning, describes the location of thunderstorms from atmospherics, and closes with a short discussion on lightning conductors.

There were considerable accessions to our knowledge of thunderstorm electricity during the inter-war years, and Meek's theory of the lightning discharge goes some way towards providing a rational explanation of the many data obtaining on lightning, mainly from the beautiful work of Schonland and his collaborators. But the problem of the generation of thunderstorm electricity remains quite unsolved, though it has become evident that the ice particles in the upper portions of the cloud are intimately concerned. It is apparent from Maurain's account that the theories so far advanced all lack conviction because they are unable to provide orders of magnitude of the charge separation and its rate of replenishment—a first charge on any theory. More data will probably be required, from laboratory experiment as well as from exploration of the thundercloud itself, before that stage is reached. A recent treatment by Frenkel has indeed made a notable advance in a quantitative sense, but Maurain has evidently missed this in his otherwise very up-to-date account of the subject.

One is a little surprised at some of Maurain's statements, mainly on points of detail, for example, that supercooled water drops freeze spontaneously at $-10°$ C., that there is a clear distinction between "heat" and "cyclonic" thunderstorms, and that small ions in the atmosphere are clusters of O_2 molecules. But such comment is not intended to detract from the view that here is a very successful little volume which would have been even more acceptable had the author, or publisher, allowed himself a little more space for diagrammatic illustration.

P. A. S.

PHYSICS

One, Two, Three . . . Infinity. By George Gamow. [Pp. xii + 340, with 8 plates and 128 figures.] (London: Macmillan & Co., Ltd., 1947. 24s. net.)

In this book Prof. Gamow writes in his now well-known manner of some of the latest "facts and speculations" (not, however, clearly distinguished) of science. There are sections on the foundations of arithmetic and the properties of numbers; the mathematical conceptions of space, leading up to an account of the theory of relativity; the structure of atoms and of living cells, with some account of statistical work and the theory of probability; and finally, the universe as a whole. The text is enlivened, as usual, by the author's own clever illustrations. The book was originally intended, he tells us, for children, but he finally decided (rightly) that it was not for them, but rather for the layman who brings intelligence rather than previous knowledge to the reading of it.

The rapid and abstruse developments of science in the last few decades make the explanation of the predominant ideas and results in generally intelligible terms a very difficult as well as a very important task. It cannot be performed satisfactorily by the non-scientist, however great his mastery of the art of exposition, for he cannot have the knowledge necessary to give a trustworthy account of such subtle material. It must be left to the very few practising scientists who have both the ability and the time to place themselves mentally in the position of the ordinary man and speak to him in language that he can understand without falsifying the story. Among these Prof. Gamow occupies a foremost place. Another factor, however, is necessary for success, which is so obvious that one is surprised at having to mention it—namely, the preparedness to take at least the minimum of pains required to make the account free from ambiguity and self-contradiction so that if the reader finds a passage apparently absurd he can feel satisfied that the fault lies with him and not with the author. Of this factor one finds no evidence in the book under review. I am not referring primarily to the numerous typographical and grammatical errors, which the publishers might have corrected, but to the still more numerous errors in arithmetic, in argument, lack of correspondence between text and illustrations, illegitimate unannounced changes of meaning of symbols, and statements which can only be described as simply incorrect. In a single reading I have noted 67 such blemishes, counting only the major ones which must either directly mislead the reader or leave him in complete confusion. It is impossible to attribute these faults to ignorance; they are all too obviously the effect of a carelessness which could hardly have been maintained more consistently if it had been intentional. This characteristic far outweighs the merits of the book and makes it regrettable that it was ever published in its present state.

Herbert Dingle.

High Vacua. Principles, Production and Measurement. By Swami Jnanananda, D.Sc., Ph.D., F.Inst.P. [Pp. xiii + 310, with 145 figures.] (New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1947. 30s. net.)

The first impressions of this book are definitely pleasing. It is well bound, well illustrated, clearly printed, and the plan of the text is good. It begins

with a substantial chapter on Kinetic Theory followed by chapters on Vacuum Pumps, Measurements of Pressures, The Techniques of High Vacua, Preparatory Operations for High Vacuum Work, and Production of High Vacua by Physico-Chemical Methods.

Unfortunately, first impressions are quickly revised on reading the book. The author does not seem to be very sure of the kind of reader for whom he intends the book. For example, after giving a detailed and elementary account of the beginnings of kinetic theory, he provides a derivation of the Maxwell distributions of velocity and energy which is almost incomprehensible to a reader not conversant with the methods of kinetic theory. There are unfamiliar or obsolete names or descriptions instead of those in general use: *e.g.* Chapter II, Section V, is entitled "Pistonless High-Vacuum Pumps with Oils or Paraffins or High-Boiling Phlegmatic Liquids as Pumping Media"; vapour *tension* is used instead of vapour *pressure*; the symbol sp_g is used in equations for the density of mercury. There are numerous misprints and loose statements. Some figures do not correspond with the text (and Fig. 3·15 has been rotated through about 30°). Some of the theory presented is very sketchy and does not bring out the physical principles involved (*e.g.* "this jet of mercury vapour draws in gas molecules from the receptacle and passes them down to the outlet . . ." p. 116). There are a few references, particularly to the work of the last ten years, and no suggestions for further reading.

A statement such as "The heaters (of a Radiometer Gauge) are raised to a higher temperature by providing a regulated heating current from the battery *C* consisting of a carefully calibrated Weston Standard Cell . . ." (p. 195) is so astounding as not to mislead any reader who has been taught elementary physics, but much more serious is the misleading view of the present-day practice which the book as a whole presents. It is quite out of date in many important respects. It describes many interesting pumps, gauges, etc., which are mainly of historical interest, without stressing sufficiently that they have been superseded, and takes little account of the rapid progress within the last ten years. Some of these deficiencies may have been due to delays in publication under present conditions, but there is no such excuse for the omission of all mention of the Philips (Penning) Gauge, of recent work on ionisation gauges and the interpretation of their readings, the use of palladium and silver in admitting hydrogen and oxygen, the recent applications of Pirani gauges, etc. Only one page is devoted to the detection of leaks. There is no description of the best way to set up a vacuum system, where to put the liquid air traps (if used); indeed, there are few of the hints and tips which can be so useful to the newcomer to vacuum work.

F. A. V.

Electron and Nuclear Physics. By J. Barton Hoag, Ph.D. Third edition revised by S. A. Korff, Ph.D. [Pp. xii + 522, with 268 figures.] (New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1948. 27*s*. 6*d*. net.)

Little comment is called for on the third edition of this excellent book. One of the pleasing features of this edition is that it has been brought up to date not only in respect of new matter, such as nuclear fission and the betatron, but also in more detailed matters, as is noted, for example, in the enlargement of the section on light units or in the addition of diagrams (such as that on

p. 85) to increase the clarity of the exposition. Furthermore, diagrams in the experimental sections have, at times, been altered in order to bring them more into line with present-day practice.

These few examples are sufficient to indicate the quality of the book and it can still be highly recommended as a textbook on the more experimental side of atomic and nuclear physics.

There is one mild criticism that should be made. Although the index has been revised to take account of the changed page numbers of the contents of the earlier edition, one searches in vain for the indexing of new topics introduced into this edition. The words synchrotron, betatron, mesotron (or meson!) are not found there, although quite amply treated in the text.

A final word of commendation is on the fact that numerical values of physical quantities have been brought up to date not only in the comprehensive tables but also in the body of the text.

W. E. DUNCANSON.

Applied Atomic Power. By E. S. C. SMITH, M.A., A. H. FOX, Ph.D., R. T. SAWYER, B. of E.E., M.E., and H. R. AUSTIN. [Pp. xii + 227, with frontispiece and 31 figures.] (London and Glasgow: Blackie & Son, Ltd., 1947. 20s. net.)

THE purpose of this book is not clear, since in no instance to date has atomic power been applied to useful industrial work. The contents are, therefore, mainly speculations and its dedication to Industry is no more than a hope.

Part 1 consists of an elementary qualitative account of radioactivity and a description of the ores of radioactive materials. Some account of the physical background of atomic energy production is given in Part 2. The energy of the oxygen nucleus in Fig. 5 does not agree with the text. Part 3 is a summary of the Smythe report on the development of atomic energy leading to the atomic bomb.

Various suggestions are put forward in Part 4 for possible methods of converting atomic energy into a source of mechanical power. Most of these are naïve. For example, in Fig. 1, the radioactivity produced in the water used as a direct coolant presents a fundamental difficulty. The closed-cycle gas turbine power plant using helium, suggested by Dr. Keller, may prove a feasible source of power when used in conjunction with a uranium heater.

In general, too many facile and incorrect analogies are drawn between physical processes used to solve problems in macroscopic physics (familiar to engineers) and those requiring solution in nuclear physics.

Part 5 gives a useful account of the knowledge which has accrued to industry from the solution of the many problems in metallurgy, in diffusion processes and so on, which accompanied the manufacture of the atomic bomb.

No reference is made to the process which may be of the greatest industrial importance, namely the manufacture of U-233 as fissile material from thorium. However, this information was probably not released at the time of publication of the book.

While secrecy accompanies developments in atomic energy, with the attendant impossibility of obtaining accurate figures, for example, concerning pile output and efficiency, the writing of a satisfactory book on applied atomic power is almost impossible.

F. C. CHAMPION.

Principles of Radar. By DENIS TAYLOR, Ph.D., and C. H. WESTCOTT, Ph.D. Modern Radio Technique Series. [Pp. x + 141, with 5 plates and 52 figures.] (Cambridge: at the University Press, 1948. 12*s*. 6*d*. net.)

THIS book is intended to bridge the gap between popular and specialist accounts of radar, and assumes a graduate standard of general scientific knowledge, and some acquaintance with normal radio practice.

After an introductory sketch of early developments, the generation and reception of pulse modulated signals is considered. Then follows a very complete account of factors affecting the maximum range of a radar set. Subsequent chapters deal with range, azimuth, and elevation finding, scanning systems, ground and cloud reflections, numerical data relating to typical radar stations, and identification of aircraft by secondary radar.

Two appendices provide a summary of principal formulæ, and some calculations of the absorbing and echoing areas of simple objects.

The treatment is clear and the book is well illustrated. One might expect to find more space devoted to microwave radar; there is a tendency for the authors to make use of longer wave-length equipments in illustration of the principles they expound. On p. 27 the Poynting vector, given as $c[\mathbf{E} \wedge \mathbf{H}]$ should be $\frac{c}{4\pi}[\mathbf{E} \wedge \mathbf{H}]$ in the "mixed" units used. It is unfortunate that the authors did not follow their preference (expressed on p. 49) for a self-consistent set of units. Also on p. 27, the authors state that all (normally) incident radiation would be absorbed by a thin film of resistance 377 ohms between opposite sides of a square, if the space beyond it were removed! The liveliest imagination may be expected to boggle at this conception. A medium of infinite intrinsic impedance is less inconceivable and would have the desired effect.

However, the book gives a lucid and very readable account of the fundamental physical principles of radar, and can be recommended as a useful introduction to the subject.

A. L. CULLEN.

One Story of Radar. By A. P. ROWE, C.B.E. [Pp. xii + 207, with 7 plates.] (Cambridge: at the University Press, 1948. 8*s*. 6*d*. net.)

THE author of this excellent book was the war-time Chief Superintendent of T.R.E., Telecommunications Research Establishment—and is now Vice-Chancellor of the University of Adelaide. The outline of the story of radar is, I suppose, a familiar one to most scientists. The many applications of radar principles used so successfully during the recent war, however, make the detailed story a most complicated one. It is the merit of Mr. Rowe's story that he has kept to the major currents of activity; the result is dramatically interesting and gives an inside view of a great establishment in embryo, in development, and at the height of its powers. It is now almost a commonplace to state that radar won the war; the reader of Mr. Rowe's story of radar at least cannot fail to appreciate the enormous importance of radar; more than anything I have yet read, this book gives the spirit of the individuals and the team of T.R.E. Mr. Rowe traces the development of important ideas and achievements from the early experiments of Watson-Watt, and the activities of the Tizard Committee, to the later dramatic achievements made possible by the use of centimetre waves. In paying tribute to the value

of radar and to the services of the scientists who contributed to its success, we should remember that Mr. Rowe somehow contrived to build a most successful team out of men of highly individualistic temperament and got them to work together with a common purpose. The human glimpses which the author gives us of the working of his establishment are not the least interesting part of his story.

J. T. RANDALL.

Velocity-Modulated Thermionic Tubes. By A. H. W. BECK, B.Sc.(Eng.), A.M.I.E.E. Modern Radio Technique Series. [Pp. x + 180, with 56 figures.] (Cambridge : at the University Press, 1948. 15s. net.)

THE author of this book was a member of the team of scientists and engineers who worked at Bristol University under the auspices of the Admiralty during the War years and contributed so much to the advancement of valve technique. A considerable proportion of their effort was concentrated on velocity-modulated tubes and Mr. Beck is therefore well qualified to deal with this subject. He has chosen primarily to treat principles rather than applications, which he uses to demonstrate how a particular idea has been translated into a practical arrangement. The work is not a textbook, but it is likely to prove of special value to research engineers and to teachers. Others who are concerned more especially with the engineering problems may not find quite all they need, particularly in regard to the conditions of operation of the various types of tube, but that kind of information is readily available from other sources.

The first chapter reviews the various forms that velocity-modulated tubes take, and this gives an opportunity to introduce a picture of the physical process involved in each case. Chapter 2 deals with cavity resonators. As the author points out, some of the theory given is only strictly applicable to simple configurations of a kind not generally met with in practice. The analysis does, however, give the key to many experimental observations which otherwise might remain obscure. Chapter 3 lays the foundation-stones of much of the edifice that is built subsequently, and in this treatment of the theory of velocity modulation certain very useful approximations are used to simplify the problem. Heavy current electron beams are given special consideration in Chapter 4, and the restrictions arising from space charge effects are clearly defined. The succeeding four Chapters 5, 6, 7 and 8 discuss the applications of the general theory to specific arrangements, including amplifiers, frequency multipliers, oscillators with particular reference to the reflex type, and a number of tubes of miscellaneous form. Chapter 9 summarises the limitations and defects of velocity-modulated tubes in general, so far as these factors can be assessed on the basis of present knowledge, and the final Chapter 10 gives much valuable information for design purposes about the principal components required to make valves of this type. There are two Appendices, the first of which provides a brief note of some of the calculations leading to the design of a high-power klystron, and the second a useful outline of the travelling wave tube.

In spite of present restrictions the book is very well produced. Typographical errors are few, but there is one of some importance on p. 36, where in the definition of Q a 2π has been omitted.

The designation of different wave-modes as TE or TM instead of H or E

with the appropriate subscript is regarded by the author as more precise, but he does not give his reasons for this point of view, and indeed opinions differ widely on the matter.

At first approach the book gives the impression that it is largely concerned with theoretical argument, but the reader will find sandwiched into the analytical work a very considerable and valuable part devoted to practical problems. The author, the Editor and the publisher are all to be congratulated on having jointly produced a most valuable treatise, in a very readable form, which will well repay careful study.

H. M. Barlow.

Report on Colour Terminology. By a Committee of the Colour Group of the Physical Society. [Pp. x + 56, with 4 figures.] (London: The Physical Society, 1948. 7*s*. net.)

In an age remarkable for the emergence of a wealth of new physical concepts, linguistic reform tends to be regarded as a sterile activity: and yet there can be no doubt of the inhibitory effect of a defective terminology. The terminology of colour relationships viewed as a whole, contrasted with that of chemistry for example, must appear extremely unsystematic, and could this be otherwise? In scientific and artistic pursuits and in diverse technological activities, man has gradually evolved closed, *ad hoc* systems of colour nomenclature, totally unco-ordinated except for the common bond of crude everyday notions and familiar loosely defined words. The analysis, co-ordination and critical presentation of over 200 numbered and defined concepts from Colour Physics, Vision and other specialised fields (*viz.* the printing and dyeing industries, the paint and pigment industries, photography and glass manufacture, decoration and art), as has here been attempted, represents a very considerable piece of work, which in Britain and for the English language the Colour Group alone, by its wide interests and membership, would have been in a position adequately to perform; and the work has been adequately accomplished. Graded type indicates in some cases degrees of preference or deprecation of synonymous terms, and occasional explanatory notes are added. Specific colour names are, of course, excluded from consideration. A survey is given of the colour terms of the Munsell and Ostwald systems, the ultimate insufficiency of the latter being pointed out. In ensuing sections an attempt is made at correlation and unification around a basis of terms considered generally acceptable.

There could of course be no question here of a puristic revision of terminology or definition on etymological or phenomenalistic grounds. By changes small in number, harmony is introduced into the industrial terminology and discordance with basic conceptions is for the most part removed. Agreement, with some exceptions, is found with the more restricted Terminology Report of the Optical Society of America, but the revisions incorporated affect a wide field. Thus, "light" in its wider sense (visible and invisible) is deprecated, "brightness" becomes "luminance," and "visual sensitivity" replaces "visibility." Among photometric terms we note that, contrary to established practice (*cf. The Analyst*, 67, 164 (1942): Report of Panel on Terms and Symbols), "extinction coefficient" is referred to the base *e* instead of the base 10: there appears no reference to "gloss" or "lustre." Widespread ambiguity results at present from the use of such terms as "shade." This is here studiously remedied. There is in fact nobody whose colour

vocabulary is not potentially affected by this first and necessary step toward a clarification of the terminology. It deserves wide and critical study in the interests of linguistic and scientific precision and the better appreciation of colour relationships which must inevitably follow.

J. W. PERRY.

Discharge Lamps for Photography and Projection. By H. K. BOURNE, M.Sc., M.I.E.E., F.R.P.S. [Pp. xvi + 424, with 186 figures.] (London: Chapman & Hall, Ltd., 1948. 36*s*. net.)

THIS book is primarily concerned with the lamps themselves rather than—as the title would suggest—with their applications in photography and projection. As a book on discharge lamps it provides what has been needed for many years: an account of the subject in the English language by one who has the first-hand knowledge and experience which alone make it possible to produce a valuable contribution to the literature. The author has, in fact, for many years been engaged in the research, development and application of discharge lamps, and this fact will be quite obvious throughout the book to the discerning reader.

The main bias, as the author's own work would lead one to expect, is towards high-pressure mercury vapour lamps, although there are adequate chapters on incandescent filament lamps, carbon arcs, the fluorescent low-pressure mercury vapour lamp and photographic flash-lamps. Applications of the lamps are mentioned in appropriate places throughout the volume, and in addition there is a long chapter on their uses in photography.

As well as those characteristics about which a user usually requires data, a considerable amount of information is given relating to the actual construction of the lamps and to those properties which are of main concern to the lamp-maker. This, however, should all be of interest even to those primarily interested in applications, and the author has not fallen into the prevalent error of obscuring the things he wishes to explain by a mass of unnecessary detail.

The book is not free from mistakes and omissions, although few of them are serious. Readers perhaps are not generally interested in historical accuracy, but the statements on p. 77 and p. 190 that the modern high-pressure mercury vapour lamp and the compact source lamp were first developed in Germany are quite wrong; although interest in attempts to make such lamps originated in Germany, practical lamps were first produced both in experimental forms and commercially in England. Some indication too of the history of the low-pressure fluorescent lamp would not be out of place.

The printing, diagrams and production generally are excellent, and the bibliographies are adequate.

V. J. FRANCIS.

Television. By M. G. SCROGGIE, B.Sc., M.I.E.E. Second edition. [Pp. x + 77, with 28 figures, including 9 plates.] (London and Glasgow: Blackie & Son, Ltd., 1948. 6*s*. net.)

As stated in the preface, the aim of this little book is to explain the underlying principles of high definition television to a reader with only a general knowledge of technical matters. This is not an easy task, but Mr. Scroggie has achieved a large measure of success.

The book states from first principles the requirements which must be satisfied in the transmission of television images and then describes the various earlier attempts to achieve practical television. Then comes a description of the system used by the B.B.C. for the present high-definition television service, with particular reference to the process of scanning and the nature of the television signal, followed by a general popular account of the actual equipment used at Alexandra Palace, and of television receivers.

The difficulty of presenting a highly technical subject to the non-technical reader must not be allowed to excuse even minor inaccuracies, and it may, therefore, be well to refer to one or two which have crept into the text. It is not strictly true to state, as on p. 41, that flicker is not noticeable at 50 frames per second "however bright the picture," whilst, notwithstanding a statement on p. 45, the suppression of 28 lines out of 405 does somewhat affect the calculation of the necessary bandwidth.

Two other points require mention. It is perhaps unfair to expect the non-technical reader to know without explanation what is meant by the polarisation of light, and on p. 43 the impression is given that black level as transmitted from Alexandra Palace corresponds with the radiation of 30 per cent. of the maximum carrier power, whereas in actual fact it is 30 per cent. of the maximum current or voltage which is involved.

Such minor criticisms of detail do not, however, detract from the merit of the book as a popular introduction to the subject of television, for which purpose it may be confidently recommended.

A. B. Howe.

Plasticity in Engineering. By F. K. Th. van Iterson. [Pp. x + 174, with 136 figures.] (London and Glasgow: Blackie & Son, Ltd., 1947. 8*s*. 6*d*. net.)

As its title implies, this book treats the subject of plasticity from the engineering standpoint and is to a large extent concerned with applications of the theory to typical problems of engineering.

The first third of the book is devoted to the two-dimensional case, with short chapters on the well-known problems of the hollow cylinder under internal pressure, and the plastic mass compressed between parallel planes, inclined planes or between concentric circular cylinders. Hencky's theorem on the variation of the principal stresses along a shear trajectory is given, and applications of the "plastic sector" method to problems arising from the presence of sharp-cornered grooves or cavities in the material.

The next four chapters are concerned with three-dimensional stress distribution and include discussions of the Maxwell-Huber-Hencky and the Coulomb-Guest criteria for plastic yield, and the value to be assigned to the mean principal stress. The disc plastometer, the Brinell hardness test and the grooved test bar are the subjects of the next three chapters, followed by a summary account of the theory of plastic torsion as given in Nadai's *Plasticity*, a discussion of slip planes and brief chapters on work-hardening and rupture. The last chapter but one deals with applications of the theory to some two-dimensional problems, and the author's version of the theory of plastic torsion. There is a final chapter on the buckling of bars when plastic yield is taken into account, and an index.

In the opinion of the reviewer the value of this book to the engineer will

depend largely on the latter's previous acquaintance with the subject, particularly in the applications of the theory to special cases. In many cases shear-stress patterns are shown for two-dimensional problems involving both elastic and plastic zones, with merely perfunctory remarks on the methods of obtaining them and without reference to the question of continuity conditions at the boundaries of separation. Again, the author's theory of torsion in the plastic state (Chapter 23), in which it is supposed that there is only one principal stress, is not consistent with the usual assumptions that the stress components are zero for axes in the plane of the cross-section.

There are, however, many useful references to experimental results and a bibliography is given in most chapters. The book seems to be very free from misprints and in this and other respects is well up to the standard expected from the publishers.

L. A. W.

Radon: Its Technique and Use. By W. A. JENNINGS, B.Sc., A.Inst.P., and S. RUSS, C.B.E., D.Sc., F.Inst.P. [Pp. x + 222, with 14 plates and 49 figures.] (London: John Murray, for the Middlesex Hospital Press, 1948. 18*s*. net.)

THE extensive use of radon as a substitute for radium and as a therapeutic agent and scientific tool in its own right dates in this country from about 1924. Prof. Russ has been intimately concerned with this work over since that time, and a book on radon from his hand clearly bears the imprint of authority. This is fully borne out in this lucid collection of information on the subject, much of it unobtainable elsewhere. To serve its maximum usefulness, this book should have appeared 10 or 15 years ago. For example, it is regrettable that the section on radon source preparation will now have a limited appeal in this country, in view of the new national policy of concentrating this work in a limited number of centres.

The book is described by the authors as a reference book on radon. It does not entirely fulfil this description by reason of a certain irregularity of emphasis. Part I gives a short and very clear account of the properties, measurement and some of the uses of radon. The section on uses might well have been considerably expanded. Approximately half of the book is devoted to a detailed description of the problems involved and of the elegant techniques devised over 25 years for the preparation of radon sources. Unfortunately the description of the processes is limited to one technique of production of glass capillary containers. In some countries gold capillaries have long been used, and these are now being adopted in this country. This technique is relegated to a short and critical footnote.

Part III of the book deals very effectively, with the aid of some excellent diagrams, with the physical problems involved in the computation of dosage in radon therapy. In the chapter on some medical considerations, great stress is laid on a radon seed chain technique which, though having obvious advantages, is not yet widely accepted. Such emphasis, which also occurs elsewhere in the book, is clearly undesirable in a reference volume.

In spite of such criticisms, there is a very great deal in the book which will prove invaluable to all users of radon in medicine, and in scientific and industrial research.

J. E. R.

CHEMISTRY

Chemistry in the Service of Man. By ALEXANDER FINDLAY. Seventh edition. [Pp. xx + 390, with 74 figures.] (London: Longmans, Green & Co., 1947. 12*s*. 6*d*. net.)

To recommend a book that has passed through seven editions in thirty-two years not only is a work of supererogation but also gives little credit to the author for his ability or to the publishers for their perspicacity. Findlay's *Chemistry in the Service of Man* remains what it always has been, a model for the presentation to a lay public of a scientific discipline and its results. Prof. Findlay is much too good an academic wholly to reject the "pure knowledge" field of science and much too shrewd a Scot to believe in the exclusive privilege of those who dwell in ivory towers. It is, perhaps, this balanced attitude, along with the wealth of his chemical interests and the straightforward simplicity of his prose, that mark a book to which there have been many successors, few rivals and no superiors of its kind.

The book follows a logical plan, as far as this is possible in presenting a science so diverse in its applications as chemistry. After an introductory chapter that presents to the reader the concepts of atoms and molecules, the author takes us immediately into a chapter on radio-activity and atomic structure; the reader, unlike the examination candidate who was told in the mechanics problem to "ignore the weight of the elephant," is given some comments on the atom bomb. Chapter 3 is concerned with the gases of the atmosphere and Chapter 4 with fire, being naturally succeeded by Chapter 5 on "Fuels and Illuminants," whence we proceed to "Matter, Energy and Explosives" in Chapter 6. Then follows a series of eleven chapters, beginning with one on cellulose and its products, passing through metals, reaction velocities and catalysis, agriculture, glass and the products of the heavy chemical industry, electro-chemistry and colloids to three chapters in which are discussed "Molecular Architecture" and a large number of examples from synthetic organic chemistry, including pharmaceuticals and plastics. A penultimate chapter on fermentation and micro-organisms is succeeded by one on vitamins and hormones, this bringing to an end a book as comprehensive as it is scholarly and as accurate as it is readable. It is not often that we meet with so sound a combination of a classical approach with an up-to-the-moment technological outlook.

A. L. BACHARACH.

Theory of the Stability of Lyophobic Colloids. By E. J. W. VERWEY and J. TH. G. OVERBEEK. [Pp. xii + 205, with 54 figures.] (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. 22*s*. 6*d*. net.)

UP to some ten or fifteen years ago the treatment of certain colloidal systems had progressed little from the purely descriptive accounts or elementary analyses of the early workers. Lyophobic colloids, typified by such systems as gold or silver halide sols in water, posed two much debated questions: were they thermodynamically stable, and what was the explanation for the different coagulating powers of salts as set out in the well-known Schulze-Hardy rule? Most workers inclined to the view that such sols were not thermodynamically stable, but owed their stability to the charges which

stable systems were found invariably to possess. The advent of the Debye-Hückel theory led naturally to a fresh examination of the problem, but up to 1940 there was no unanimity amongst the various theories put forward; in fact, some very diverging conclusions were reached.

During the war years Drs. Verwey and Overbeek were able to continue with the problem, and they have set out in this small monograph not only their own conclusions, but also a detailed analysis of other contributors, tracking down the reasons for many of the earlier discrepancies. By and large the picture now presented is much more satisfying to the practical colloid chemist than most of its predecessors, although certain points may need extension. In the present theory the repulsive energy term, which tends to prevent aggregation, is ascribed to the electric double layer around the colloidal particles; attraction arises from the van der Waals–London attractive forces.

The attractive and repulsive terms are worked out in considerable detail for a variety of systems, particularly two parallel flat plates and two spheres. The equations for the overall interaction energy so deduced appear to give an adequate explanation for the coagulating powers of salts of different valence types. The same general approach has also been used to interpret certain other colloidal features such as gelation, thixotropy and the stability of emulsions, but this would appear to be much less satisfactory.

The style is extremely clear and the mathematical parts have been set out in detail. Printing is good and mistakes appear to be few. Altogether an excellent and very timely publication.

A. E. Alexander.

Mechanical Behavior of High Polymers. By Turner Alfrey, Jr. *High Polymers*, Vol. VI. [Pp. xiv + 581, with 246 figures.] (New York and London: Interscience Publishers, Inc., 1948. 57*s*. net.)

This book represents the first really serious attempt to correlate the mechanical properties of high polymers with molecular structure and arrangement. No matter whether a high polymer substance is used as a fibre, rubber, foil, it is an almost invariable circumstance that the use for the high polymer is determined by its mechanical properties. The chemical properties, while important, play a secondary role. Yet the mechanical properties are controlled by the chemical architecture of the macromolecule and by its interaction with other molecules. Not only do mechanical properties determine the application of the polymer, but they have important repercussions on the fabrication of the polymer as initially produced into the article of commerce.

This correlation is one of the most difficult fields of high polymer science. On the one hand, the chemist is not usually familiar with the essentially engineering approach to mechanical properties, and conversely the engineer is even less familiar with the intricacies of modern high polymer chemistry. Dr. Alfrey has set out to bridge this gap in language that can be understood and appreciated by a very wide variety of scientists. He has taken care to educate the chemist by setting down in sufficient detail and thoroughness the theory of the mechanical properties of matter under consideration. This is a welcome section, for the information is not easily found by a chemist whose prime interest is in high polymers. The following sections of the book deal with systems not classified chemically, which would be confusing, but

classified according to molecular morphology. Thus the amorphous linear high polymers are thoroughly reviewed as linear high polymers. Next follows a discussion of cross-linked polymers, vulcanised rubber being included in this section. Crystalline high polymers naturally follow and thereafter fibres come under especial review here. The problems associated with plasticiser action form the fourth of these large sections of the book. The vexed problem of the ultimate strength of materials is carefully reviewed, bringing together what new knowledge has accumulated since these matters for high polymers were discussed by Houwink many years ago.

Mathematical appendices close a book full of interest and a fruitful source of problems whose solution is a pressing need.

This volume is a worthy member of the high polymer series and comes at an opportune time in this branch of engineering, physics and chemistry.

H. W. M.

An Introduction to Metallic Corrosion. By Ulick R. Evans, M.A., Sc.D. [Pp. xxxvi + 211, with 66 figures.] (London: Edward Arnold & Co., 1948. 12*s*. 6*d*. net.)

Dr. Ulick Evans is so well known for the excellence of his writings on all aspects of corrosion that the quality of any new book from his pen is practically a foregone conclusion. Nevertheless, so soon after producing the 1946 edition of his monumental work *Metallic Corrosion, Passivity and Protection* one may be excused for wondering why another book is necessary at this time. Dr. Evans anticipates this question and answers it fully. He states that for 15 years there has been no scientific book on the Corrosion of Metals of a length suitable for continuous reading, the books which have appeared being long books intended for reference purposes or shorter books covering only part of the subject; that the book is intended to be read, not merely by students, but by experienced scientists who have hitherto taken no interest in the subject of corrosion; and that each of the eight chapters of the present book can probably be finished by a determined reader in a single evening, so that the book as a whole should be capable of being absorbed in a few weeks.

In attempting in so short a book to cater for the needs of those who are experienced in the ways of corrosion and also for scientific men to whom the subject is a new study, the author has set himself no easy task. It is impossible to please every reader and some will probably find parts of the book difficult, but there can be few, if any, who could claim that the book holds nothing that is new to them or that the treatment is too elementary. Thus Dr. Evans has nicely solved his problem in the positive certainty of having provided something worth while for everyone. As for the idea of a determined reader finishing a chapter a night, the reviewer (with some determination!) tried it and can vouch that interest does not flag.

The book begins with an historical note on the study of corrosion, which is in itself a most useful contribution to chemical literature. There follows a brief Electrochemical Introduction in which those principles of electrochemistry which figure prominently in corrosion studies are explained. Then comes the book proper, consisting of eight chapters on Film Growth; Electrochemical Corrosion; Corrosion by Acids and Alkalis; Influence of Environment; Effect of Stress, Strain and Structure; Prevention of Corrosion by Soluble Inhibitors; Prevention of Corrosion by Protective Coverings;

Statistical and Mathematical Treatment. This gives a good balance of the study of the causes and effects of corrosion with the study of the means of prevention. Moreover, there is a good balance of theory, experiment and practical application in each chapter. The chapter dealing with stress corrosion and corrosion fatigue is remarkable for its simplicity of treatment, and the chapter on statistical analysis will be useful to workers in many other branches of science. The book is well produced and its comparatively low price brings it within reach of all; it is, indeed, excellent value.

E. S. Hedges.

Isomerism and Isomerisation of Organic Compounds. By E. D. Bergmann. [Pp. xii + 138.] (New York and London: Interscience Publishers, Inc., 1948. 21*s.* net.)

This booklet contains the subject-matter of six lectures delivered by the author at a seminar held at the Polytechnic Institute of Brooklyn in 1946. The topics discussed in six brief sections may be inferred from the following chapter headings (with some additional comments by the reviewer): I. "Resonance Phenomena in Organic Molecules" (a discussion of a qualitative nature dealing mainly with structural conditions conducive to resonance and physical manifestations of the phenomenon). II. "Cis-Trans Isomerism and Cis-Trans Interconversion" (including arguments for the assignment of structure and an outline of interconversion mechanisms). III. "Isomerisation of Olefinic Structures" (and the stereochemistry of addition reactions, etc.). IV. "Mechanism of Substitution Reactions; Racemisation and Walden Inversion" (a mediocre discussion in the reviewer's opinion). V. "Isomerisation of Paraffins and Related Phenomena" (including a brief discussion of the course and mechanism of the alkylation of olefinic compounds). VI. "Mechanism of Intramolecular Rearrangements" (a sketchy treatment of a wide subject).

The reviewer can well believe that the lectures delivered by Dr. Bergmann proved most interesting and stimulating to the audiences. Presented in book form, however, the discussions appear incomplete and often disappointing, while the references cannot in any way be regarded as comprehensive or up-to-date. Nevertheless, the contents of the booklet culled from the author's wide experience in the field of discussion, include valuable material, and it can be recommended for students and research workers. The printing is entirely satisfactory and the subject index included is adequate.

E. D. Hughes.

Organic Syntheses. Vol. XXVII. R. L. Shriner, Editor-in-Chief. [Pp. vi + 121.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 13*s.* 6*d.* net.)

The present addition to the series of *Organic Syntheses* maintains the high standard already set by the previous volumes of this series. The thirty-nine preparations given comprise β-alanine, β-aminopropionitrile and *bis*-(β-cyanoethyl)amine, benzalacetone dibromide, biallyl, α-bromobenzalacetone, *tert.*-butylamine, carboxymethoxylamine hemihydrochloride, decamethylenediamine, diethylaminoacetonitrile, dihydroresorcinol, 3:5-dimethyl-4-carbethoxy-2-*cyclo*hexen-1-one and 3:5-dimethyl-2-*cyclo*hexen-1-one, 1:5-dimethyl-2-pyrrolidone, 2:3-diphenylindone (2:3-diphenyl-1-indenone), 2:4-diphenylpyrrole, ethyl α-isopropylacetoacetate, 4-ethylpyridine, glycolo-

nitrile, 5-hydroxypentanal, isatoic anhydride, 6-methoxy-8-nitroquinoline, 1-methyl-2-imino-β-naphthothiazoline, N-methyl-1-naphthylcyanamide, 1-methyl-1-(1-naphthyl)-2-thiourea, mucobromic acid, *m*-nitrodimethylaniline, 3-penten-2-ol, γ-*n*-propylbutyrolactone and β-(tetrahydrofuryl)-propionic acid, pseudothiohydantoin, rhodanine, stearolic acid, tetraiodophthalic anhydride, *m*-thiocresol, *o*-toluic acid, *p*-toluic acid, *o*-toluidinesulphonic acid, 1:3:5-triacetylbenzene. As is the usual practice in this series, the present volume contains a collective index for volumes XX–XXVII and, for the convenience of those readers who may wish to make a complete literature survey of any of the compounds described in the book, the *Chemical Abstracts* indexing names are given as subtitles, whenever they differ from the title of the preparation.

It is always a pleasant discovery in synthetic work to find that a particular preparation is described in *Organic Syntheses*, for one is then assured that at least that stage of the projected synthesis will present no unexpected difficulties. The volumes have an assured place in any library of organic chemical literature.

A. W. JOHNSON.

Synthetic Methods of Organic Chemistry. A Thesaurus. Vol. I, 1942–44. By W. THEILHEIMER. Translated from the German by HANS WYNBERG. [Pp. x + 254.] (New York and London: Interscience Publishers, Inc., 1948. 30*s*. net.)

As the wartime Swiss edition was not reviewed in SCIENCE PROGRESS, it may be pertinent to outline briefly the general aims of this projected series of volumes by Theilheimer. A systematic survey of the literature of organic chemistry can be based either on the nature of individual compounds as in Beilstein's *Handbuch der Organischen Chemie*, or on the nature of reactions as in Houben-Weyl's *Die Methoden der Organischen Chemie*. Unfortunately the latter volumes have now become rather dated and in the Theilheimer volumes an attempt is being made to continue this important method of abstracting the literature. Since the publication of Houben-Weyl, the need for reviews on methods of organic chemistry has been partially met by articles in *Chemical Reviews* and *Annual Reports of the Chemical Society*, together with volumes such as *Organic Reactions* and *Newer Methods of Preparative Organic Chemistry*, although these are naturally limited in their scope.

The present volume, the first of the series, covers the literature from 1942 to 1944 and a second volume, already available in the Swiss edition, covers the 1945–46 period; and further supplementary volumes are then projected annually. Selected individual reactions are systematically classified and a general appraisal of each reaction is given, including the number and nature of the reaction steps, the yield, the literature reference and, in general, sufficient details for the abstract to be of immediate laboratory value. The author has adopted a system, originally proposed by Weygand, of classifying organic reactions according to the nature of the bonds formed, and the four main methods of obtaining bonds are expressed by symbols: addition ($\Downarrow$), rearrangement ($\curvearrowright$), exchange ($\downarrow\uparrow$) and elimination ($\Uparrow$). The complete classification of a reaction is given by (*a*) the nature of the bond formed, (*b*) the method of obtaining the particular bond and (*c*) the type of bond

which is destroyed in the reaction. In order to arrive at the elements to be shown in (*a*) and (*c*), the "principle of the latest position" is employed, using the order of elements adopted in *Chemisches Zentralblatt*, *viz*. H; O, N, S, Halogen and C always being placed last. Examples of the classification are as follows: Addition of hydrogen bromide to a carbon-carbon double bond: Hal C $\Downarrow$ CC. Beckmann rearrangement: OC $\curvearrowright$ ON. Ketone synthesis by the Friedel-Crafts synthesis: CC $\downarrow\uparrow$ Hal. Dehydrogenation CC $\Uparrow$ H.

The classification is more complex than the current methods using such trivial names as "Oxidation" and "Friedel-Crafts reaction" or by author-naming, but it is more formal and does not require to have certain reactions inserted under several headings. On the other hand, the new system tends to group together various unrelated reactions.

The index is fairly adequate, although several omissions have been noted, *e.g.* there is only one reference to pyrimidines, although three or four methods of pyrimidine synthesis and reactions are discussed in the book. It is to be hoped that indexes of future volumes in the series will also cover the earlier volumes.

The present English translation has unfortunately introduced a number of additional errors—possibly one of the most obvious is the translation of *J. Soc. Chem. Ind.* as *J. Indian Chem. Soc.* (Nos. 23, 157) and *J. Chem. Soc. Ind.* (No. 236) and it must therefore be concluded that the references were not checked after translation. Other errors of translation are "consequently" for "subsequently" (No. 72), "reduced" for "reacted" (No. 100), "octanol," "heptanol" and "octanol" for "octanal" and "heptanal" (No. 511). Exception might also be taken to the representation of spiro compounds (No. 783) in a manner similar to polyphenyl compounds (No. 680) and to the inconsistent use of double bonds in aromatic rings. A number of errors in formulæ (Nos. 192, 619, 695), compound names (No. 152) and author names (Nos. 140, 147, 750) were also noted.

The value of any volume which systematically arranges a portion of the scientific literature, in contrast to the complete surveys, depends on the author's critical faculties and judgment in his selection of examples, and in this respect the present volume is to be recommended. Nevertheless, those readers who desire information on a particular reaction will feel that recourse to the standard volumes of Abstracts will still be necessary, especially as the "gap" between Houben-Weyl and Theilheimer will have to be covered from the Abstracts in any case.

A. W. JOHNSON.

The Chemistry of Acetylene and Related Compounds. By ERNST DAVID BERGMANN. [Pp. viii + 108, with 1 figure.] (New York: Interscience Publishers, Inc., 1948. 18*s*. net.)

THIS small volume comprises three lectures delivered by the author to the Polytechnic Institute at Brooklyn in the fall of 1946, although the text includes a few additional references from the 1947 literature. The limitations of space have made it impossible to cover the whole field of acetylene chemistry and emphasis has been given to modern developments and especially those which have found industrial applications. In consequence, the wartime German contributions to the subject are justly given considerable prominence, and relatively full accounts are also given of the Russian research on the

chemistry of monovinylacetylene and divinylacetylene, and the contributions of Weizmann and his colleagues on the interaction of acetylene and ketones, together with the reactions of the products. It is unfortunate that the outstanding contributions of Heilbron and Jones and their co-workers are not given more prominence, especially as this fundamental work formed the basis of the recent synthesis of crystalline vitamin A. The author puts forward a number of novel theories and predictions, many of which are undoubtedly incorrect, and it may be occasionally difficult to discriminate between the statements of fact and the theories, especially if one is not thoroughly familiar with this branch of organic chemistry. The nomenclature employed is not always consistent and a few outstanding errors have been noticed, *e.g.* propiolic acid for acetylenedicarboxylic acid on p. 89, as well as a number of misprints in the names of authors and in the formulæ, *e.g.* p. 79.

The general format of the book is excellent and it can be safely recommended to those who wish for a concise and readable account of the many recent developments in the field of acetylene chemistry.

A. W. Johnson.

Advances in Carbohydrate Chemistry. Vol. 3. Edited by W. W. Pigman, M. L. Wolfrom and S. Peat. [Pp. xxiv + 424, with 18 figures.] (New York: Academic Press, Inc., 1948. 46*s*. 6*d*. net.)

To those scientists who have been fortunate enough to have studied the first two volumes in this series no greater recommendation of this, the third volume, can be given than an assurance that it maintains in all respects the high standard of its predecessors. The policy of the executive committee of inviting universally recognised experts to contribute chapters on their own topics, and of allocating sufficient space to enable such articles to be logically developed and comprehensive, has resulted in this series becoming an essential part of the equipment of everyone interested in carbohydrate chemistry, as is evidenced by the fact that it has been necessary to print extra copies of the first volume.

An interesting feature of the book is the judicious blending of reviews of two types, dealing on the one hand with the fundamentals of monosaccharide structure and on the other with current advances. C. S. Hudson's account of the way in which planar stereo-formulæ for the monosaccharides were devised and later improved until there was no room for dubiety in their interpretation should be invaluable to those who, having entered this complex field at a stage when order has been established, have been content to employ these formulæ without questioning their validity or their meaning. Articles of a similar type are those dealing with the use of trityl ethers in structural and synthetic studies (B. Helferich) and with oxidation of sugars by halogens (J. W. Green). Among the interim reports on current advances are surveys of the contributions of isotopic tracers to the study of carbohydrate metabolism (S. Gurin), the investigation of the polysaccharides of *Mycobacterium tuberculosis* (M. Stacey and P. W. Kent) and the chemistry of streptomycin (R. U. Lemieux and M. L. Wolfrom). In all there are eleven excellent reviews, and, in addition, an appreciation by M. L. Wolfrom of the life and work of R. M. Goepp, Jr., whose untimely death has robbed us of an active member of the executive committee of "Advances."

The book is carefully edited, attractively bound and well indexed. The

reviewer feels, however, that an effort should be made to improve the method of printing the cyclic perspective formulæ of the sugars.

E. J. B.

The Chemistry of the Polysaccharides. By ROBERT J. McILROY, M.Sc. (N.Z.), Ph.D., F.R.I.C. [Pp. viii + 118, with 5 figures.] (London: Edward Arnold & Co., 1948. 10*s*. 6*d*. net.)

DURING the two decades which have passed since the monograph on *The Constitution of the Sugars* was written by Sir Norman Haworth, a very wide extension of our knowledge of the chemistry of carbohydrates has taken place. The bases of monosaccharide structure remain as set forth in his book, later developments having been mainly in the polysaccharide field. It is fitting that the advances which have followed the establishment of the fundamental structure of the sugars should be summarised by a former member of the Birmingham School.

A desire to limit the cost of this volume has not permitted a full logical approach to the subject and in consequence the book will probably appeal more to the post-graduate worker than to the undergraduate. The numerous references which accompany the reviews of the structures of some 40 polysaccharides, gums and mucilages should be of value to those who desire to delve more deeply into the subject. The natural functions of the carbohydrates have not been forgotten, one chapter being devoted to a discussion of carbohydrate metabolism in plants and animals, while another deals with immuno-polysaccharides. Perhaps the most useful part of the book is the analytical section, which contains full experimental details for the estimation of polysaccharides in natural tissues and fluids and for the identification of their monosaccharide building units.

After such a commendable condensation of a vast field of research into a small compass minor imperfections in literary style and arrangement can be overlooked. It is unfortunate that the delay between the completion of the manuscript and its publication has necessitated the inclusion of an appendix to bridge a period of several years. No doubt these blemishes will be remedied in future editions of the book.

E. J. B.

Qualitative Analysis by Spot Tests: Inorganic and Organic Applications. By FRITZ FEIGL, Eng., Dr.Sc. Third English edition translated by RALPH E. OESPER, Ph.D. [Pp. xvi + 574, with 45 figures.] (Amsterdam and New York: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1947. 43*s*. net.)

THE recent edition of Feigl's treatise contains many new features. Seven years have elapsed since the previous translation of the book by Dr. Janet Matthews appeared. During the major portion of this period Prof. Feigl has been working in the Department of Agriculture of the Brazilian Government. This change from the academic atmosphere of Vienna University to the more practical plane of a government post has manifested itself in the character of the book. Thus the chief change from the previous edition is the inclusion of a long chapter on the application of spot reactions to tests for purity, examination of technical materials and studies of the composition of minerals and, as an addendum to this, there is a very full bibliography of original papers relating to such applications. Also included in the book for

the first time is a chapter on working methods and special apparatus to be used in conjunction with these spot reactions, and a special section on detection of free elements.

The general format of the book follows the pattern of previous editions. In fact, although the present translation from the German is by Dr. Oesper, much of the text of Dr. Matthews' rendering is retained. Spot tests are outlined for metals, acid radicals, organic analysis and systematic inorganic analysis. Firstly the chemistry of the test is outlined, then the procedure is given, and finally instructions for the preparation of reagents and apparatus required is discussed in detail. The sensitivity limit of the test is always given.

The book is to be recommended. Every analyst can frequently use it. The reviewer freely acknowledges assistance and inspiration from its pages, in his work on the elaboration of quantitative micro-chemical methods of analysis, over a wide and varied field. The organic chemist will find it particularly helpful in assisting in identification of new compounds, and the inorganic chemist will be aided by its schemata on systematic analysis of mixtures.

Finally, the technique of micro manipulation advocated should be cultivated by all chemists. Much preliminary information may be obtained and time saved by investigation on spot test scale, no matter how large the sample may be.

R. F. M.

A Scheme of Qualitative Organic Analysis. By F. J. SMITH, Ph.D., F.R.I.C., and E. JONES, M.Sc., F.R.I.C. [Pp. viii + 320, with frontispiece and 5 figures.] (London and Glasgow: Blackie & Son, Ltd., 1948. 17*s*. 6*d*. net.)

IT may be said at once that this book sets out an orthodox and reliable approach to the identification of simple organic compounds such as are used in teaching up to F.R.I.C. standard. Its scope is thus in one sense a little wider than is usual in works of this type. The treatment of artificial mixtures and the description of practical organic procedures are given, reasonably, a minimum but sufficient notice, while understanding of the characterisation reactions involved is quite properly and consistently ensured by means of description and numerous structural formulæ.

Three features which contribute much to the usefulness of this book deserve special mention. The first is the unusual wealth of detail, seemingly reliable as far as has been ascertained, presented in 88 clear tables. Another is the inclusion of many simple reagents and reactions which have in the past been overlooked or which have only recently been devised. The third feature comprises informative general and substance indexes. There are but few matters which occasion some slight regret in the text when viewed from a student's viewpoint, but even these few might disappear under the guidance of a teacher. One might instance at one end of the scale the invariable mention of potassium instead of sodium in element tests, while at the other there is the virtual absence of any mention of the literature of organic chemistry. Students so frequently approach organic and inorganic qualitative analysis from the one viewpoint of acquiring a routine that no opportunity of stressing the research nature of organic identifications should be lost. The arrangement of the groups of compounds leads to certain anomalies

from the teaching point of view, anomalies which of course are dictat. the system adopted. Thus esters and lactones appear before alcohols, wh. such complicated compounds as carbohydrates and glycosides figure among the first compounds mentioned in the book.

This work is excellently and durably produced, as is required of a text intended for laboratory use. It can confidently be recommended for all those students who already have some appreciation of the methods of organic qualitative analyses, as well as for many reference purposes.

A. H. COOK.

Semi-micro Qualitative Organic Analysis. By NICHOLAS D. CHERONIS and JOHN B. ENTRIKIN. [Pp. xiv + 498, with 67 figures.] (New York: Thomas Y. Crowell Company; London: Constable & Co., Ltd., 1947. 25s. net.)

ACCORDING to the authors, the object of this book is to expand the scope of qualitative organic analysis by the application of semi-micro technique. Among the advantages gained are: saving of time, economy of reagents, the possibility of analysis of smaller amounts of substances, and the instillation into the worker of care, cleanliness and manipulative skill. The book should have some appeal to all workers in organic chemistry, but is written primarily for students. The inculcation of semi-micro technique into the student is claimed to make the subject more interesting, to involve a greater use of chemical theory and to allow easier assessment of the individual's capabilities.

After a preliminary discourse on procedure for the identification of organic compounds, there follows a detailed chapter on semi-micro chemical technique used for the purification of compounds, and in the determination of physical constants. The modified apparatus advocated for working on a semi-micro scale is illustrated at the same time as the technique is expounded. Technique and apparatus for crystallisation, distillation and sublimation are described in detail, as are methods for determination of boiling point, melting point, refractive index, density and optical rotation.

Most of the apparatus described may be readily constructed, but it is surprising that the authors omit to advise the use of interchangeable joint apparatus, particularly necessary in semi-micro technique if contamination is to be avoided.

A chapter is given to important organic reactions, *e.g.* nitration, oxidation, reduction, condensation, etc., and, although these are rather sketchily treated, the authors do indicate the special precautions necessary when applying these reactions on a semi-micro scale. The major portion of the book is devoted to preparations of derivatives. These are described in great detail. Points emphasised are:—

(*a*) conditions necessary for completion of reaction,
(*b*) minimum crystallisations necessary to obtain a pure product,
(*c*) exact quantities of solvent and conditions for satisfactory crystallisation,
(*d*) need to obtain sufficient product for several melting-point determinations.

After a discourse on the separation of mixtures and chromatographic technique specifically, there follow comprehensive tables of physical constants of compounds and their derivatives, covering most substances likely to be

encountered. Finally, there is a table on the characteristics of a wide range of physiologically active substances.

The reviewer believes that all analysis should be taught on a quantitative basis, and further agrees with the authors on the value of early instilling into the student ideas of care, dexterity and precision, inevitably acquired after practice in semi-micro technique. The book has much to recommend it from this viewpoint.

R. F. M.

Volumetric Analysis. Vol. II. **Titration Methods: Acid-Base, Precipitation, and Complex-Formation Reactions.** By I. M. Kolthoff and V. A. Stenger. Second edition. [Pp. xiv + 374, with 14 figures.] (New York and London: Interscience Publishers, Inc., 1947. 36*s*. net.)

This volume is the second of a three-volume treatise on volumetric analysis, itself a successor of *Die Praxis der Massanalyse*, published by I. M. Kolthoff and G. Menzel in 1928. The first volume gave theoretical fundamentals, and the third, in preparation, is to be devoted to oxidation-reduction methods, now so numerous and important that they merit a volume to themselves. The appearance of an up-to-date and authoritative work on volumetric methods is most welcome, and these volumes promise to fill an evident gap in the modern literature of analytical chemistry.

Technical methods of analysis frequently employ volumetric techniques, and in a textbook the extent to which technical methods are selected for detailed description is a matter requiring considerable discretion. In this book the authors have very wisely concentrated on fundamental methods, and restricted their excursions into applied chemistry to outstandingly important procedures; moreover, these are given as examples of the application of basic methods, and not segregated according to the field of use. Besides being sound scientifically, this method of treatment gives the book a coherence which is often lacking in works on methods of analysis.

The three main subdivisions of the present volume are devoted respectively to apparatus and general principles, acid-base reactions and their use, and quantitative precipitation and complex-formation reactions. The practical details given in the first section afford an elegant exposition of the craftsmanship of volumetric analysis, and indicate well the accuracy which may be expected from skilfully performed titrations. The strong emphasis on selection and testing of standard substances is a valuable feature of this section. The methods described in the two following sections cover a wide field, including all the classical procedures and a judicious selection of those more recently developed. References to the original literature are given plentifully throughout, and the authors' soberly critical evaluation of original research will commend itself to any analytical chemist who is worthy of the name.

The theoretical basis of every procedure is well laid throughout the volume, appropriate parts of Vol. I being cited to cover the more general principles. In this respect the book sets forth a view which constantly needs reiteration—that an analyst is still a chemist, and cannot afford to neglect the broad chemical principles of which any analytical method is a refined application.

Kolthoff and Stenger should find its way into every analytical laboratory and chemical reference library; and once there it will not rot on the shelves.

A. J. E. Welch.

Calculations of Analytical Chemistry. By LEICESTER F. HAMILTON, S.B., and STEPHEN G. G. SIMPSON, Ph.D. Fourth edition. [Pp. xiv + 387, with 14 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 17*s*. 6*d*. net.)

THIS is an exhaustive treatise containing no less than 1032 problems for working by the student; these problems are, from the viewpoint of the British reader, the principal virtue of the book, for the descriptive material scarcely lends itself to the courses and methods of teaching normally practised in this country.

The main sections deal successively with general analysis (incorporating some mathematical operations of chemical interest, chemical equations and calculations based upon them, equilibrium constants, and a treatment of redox potentials), gravimetric analysis, volumetric analysis, electrometric methods, and gas analysis. These fields are covered with considerable thoroughness and attention to detail, but so much material normally covered elsewhere (particularly in regular courses in physical chemistry) is included that the book appears somewhat laboured in its treatment, and it conveys the impression that chemical calculations are inherently difficult. There is some tendency, too, to introduce rule-of-thumb methods where a clear and logical application of principles would be more convincing. Nevertheless, the book affords many illustrations of the use of physico-chemical principles in analysis, and the numerous worked examples are well set out.

As a source of problem material, and a general work of reference on a wide range of calculations adopted in chemistry, this book can be recommended; it is unlikely to become popular for routine use by students.

A. J. E. WELCH.

Chemical Process Principles. Part II: **Thermodynamics**; Part III: **Kinetics and Catalysis.** By O. A. HOUGEN and K. M. WATSON. [Part II: pp. i–xvi, 437–804, xvii–xlviii, figs. 98–166; Part III: pp. i–xvi, 805–1107, xvii–xlviii, figs. 167–230.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. Part II: 30*s*. net; Part III: 27*s*. net.)

IN 1931 these two authors wrote a book on *Industrial Chemical Calculations* and the present series of three, of which the first volume was on *Materials and Energy Balances*, is to some extent an outcome of the previous work.

Vol. II is a concise account of thermodynamics and includes much useful data, such as molal entropies, temperature-entropy diagrams and the Merkel-Bosnjakovic enthalpy-concentration chart for ammonia, which is so useful in calculations of ammonia absorption refrigeration plants. In view of the present developments in the production of enriched air, diagrams for air and oxygen would be a welcome addition.

The clear chart of the standard free energies of formation of hydrocarbons is ascribed to Parks and Huffman, although actually it first appeared in a paper by A. W. Francis in 1928.

The authors say: "Because of the general absence of complete data for the solution of process problems a chapter is devoted to the new methods of estimating thermodynamic properties by statistical calculation." The need for this data and the methods of overcoming the lack of it are one of the chief preoccupations of the chemical engineer today. The valuable work done by Kelley at the U.S. Bureau of Mines has helped considerably in the

inorganic field and our Chemical Research Laboratory is making a start on the organic compounds which occur in gasworks liquors. The subject is one that does not pay an immediate dividend and the extensive finance required is difficult to justify to those who cannot or will not take a long-term view. It is however a very urgent need.

An example is given of the isentropic expansion of ethylene from 50 down to 1 atmosphere without indicating the restrictions which apply in practice if an attempt is made to carry it out through any type of orifice or nozzle.

The book is intended for third- or fourth-year chemical engineers, but is one which can be worked through with considerable profit by all chemists.

Part III deals with the chemical engineering problem of designing a reactor from first principles of physical chemistry. The authors start with the activated complex theory developed by Eyring and his co-workers. Since the theory at its present stage may give results 100 times or more from the experimental ones, they use it for the extension of fragmentary data rather than for the prediction of rates. They consider, for example, the pyrolytic dehydrogenation of benzene to give diphenyl with triphenyl as a by-product, and the pyrolysis of propane at 725° C. to give propylene, hydrogen, ethylene, methane and butane.

The question of catalysts and the effects of fouling are illustrated, for example, by the conversion of benzene into toluene over a silica-alumina catalyst and the hydrogenation of *iso*-octenes.

Two chapters deal with mass and heat transfer in catalytic beds and the design of catalytic reactors. It is strange how American authors show a predilection for the Fanning equation, which is merely a definition of what is meant by the symbol "*f*" and was much better handled by Stanton in his researches at the National Physical Laboratory. The work of Carman on the flow of fluids through packed solids should have been noted.

Under uncatalysed heterogeneous reactions the question of the nitration of benzene has been considered.

The book is a brave attempt to tackle the problem of complex organic reactions and the authors have not been frightened by the difficulty of the subject. The book is a milestone in the study of reaction kinetics and should prove useful in both the theoretical and practical fields, especially for the insight which it gives on the question of yields and by-products.

M. B. DONALD.

Synthetic Petroleum from the Synthine Process. By B. H. WEIL, M.S. in Ch.E., and J. C. LANE, B.S. in Ch.E. [Pp. xii + 303, with 20 figures.] (Brooklyn: Remsen Press Division, Chemical Publishing Co., Inc., 1948. $6.75.)

THIS is the first systematic attempt to develop in book form the vastly important subject of the production of liquid hydrocarbons by the now numerous variants of the Fischer-Tropsch process, operating from carbon monoxide and hydrogen.

The authors have brought together a very considerable mass of relevant data under the following headings: production and purification of the synthesis gas from coal—including underground gasification—and from natural gas; catalytic synthesis, and catalysts; products and by-products; basic economics and current developments.

Naturally full weight is given to war-time developments in Germany,

where between 1936 and 1940 there was a tenfold increase in production. In fact, the interest in the process is so great in the United States that the Bureau of Mines is responsible for a $30,000,000 programme, apart from the part played by individual coal and oil companies.

The various chapters of the monograph are exceedingly well documented. Furthermore, there is a series of useful appendices dealing with synthine patents that already number over 2000; with Government-released synthine reports amounting to 157 distinct cases; with a supplementary bibliography of current references comprising production, purification, catalysis, by-products, derivatives and the situation as it is to-day.

Two good indexes are provided. There is not the slightest doubt but that this compact summary will be of the greatest value to all executives, staffs and students of the oil and coal industries. A. E. D.

Flame-proofing Textile Fabrics. Prepared by a staff of specialists under the editorship of ROBERT W. LITTLE. [Pp. xx + 410, with 81 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. 40*s*. 6*d*. net.)

THIS addition to the American Chemical Society's series of monographs is not a straightforward account of the "fireproofing" of textiles, but a most valuable record of research work carried out during and since the war under the auspices of the United States Quartermaster-General. The rather baffling problem of rendering cellulosic textile fabrics flameproof, without serious loss of their strength and comfort in wear, has hitherto been tackled because of disasters and loss of life. The flannelette hazard, to which the Salvation Army gave prominence in this country, led to researches by Prof. W. H. Perkin—which have not received adequate recognition in the present book. In recent years there have also been disastrous fires in American places of entertainment and among boys playing in Red Indian outfits. Some of these accidents occurred too near Washington to be overlooked by Government, and there was a danger of hasty and restrictive legislation. The need for the protection of soldiers from incendiary bombs and flame-throwing, and of airmen from petrol fires, has intensified the problem enormously, and one of the minor gains of the war is the present knowledge, leading to the wide adoption of the urea-phosphoric acid process, so well summarised by the present contributors.

The book is divided into three main sections—theoretical, practical and commercial. The first section includes a very useful survey of the chemistry of cellulose, with special reference to the theory and products of molecular degradation, pyrolysis and combustion. Section II discusses methods for evaluating flameproofing treatments and testing related properties of textiles. It also describes the various flameproofing agents, under those having but a temporary effect, those which are removed by washing, and the most valuable group which afford permanent protection. Section III is a record of military demands, many of which went beyond normal civilian requirements, but the last chapter discusses the desirability of implementing some of the results in normal trade conditions.

There is some duplication of information, inevitable in a joint effort by so many writers, and no reference is made to non-cellulosic textiles. The book can be highly recommended as an account of one of the major war-time advances in textile processing. J. C. WITHERS.

The Dehydration of Food. By T. N. MORRIS, M.A. [Pp. viii + 172, with 30 figures.] (London: Chapman & Hall, Ltd., 1947. 15s. net.)

DR. A. J. M. SMITH in a lecture "Old and New Problems in the Carriage of Food Overseas" to the Food Group of the Society of Chemical Industry in 1935 pointed out that, in our annual imports of food, three million tons consisted of water. This fact defined the most important problem for the Ministry of Food during the war. One million tons of water were at once left overseas by the simple expedient of cutting out practically all imports of fruit. Much later the strain on our shipping was further relieved to the extent of some 300,000 tons by replacing shell eggs by dried egg. The war did not go on long enough for the other imported foodstuffs, notably meat, to be replaced by the corresponding dehydrated products. The Services, however, although perhaps not always very appreciative, benefited considerably from the research leading to the production of palatable, nutritious dehydrated foods.

To those of us who were in a position to judge, the Low Temperature Research Station at Cambridge unquestionably led the world in research on dehydration. Mr. T. N. Morris is a member of the staff of the Station, so that with his own scientific and literary abilities it would have been difficult for him to write anything other than this first-class and authoritative book.

Mr. Morris first distinguishes between "drying" naturally in the open air, usually in the sun, and "dehydration" in specially built chambers under controlled conditions of temperature, humidity and air speed, thus settling in simple fashion a controversy which at one time caused much concern in many lay minds. He then shows by means of tables the great saving in shipping and storage space when the usual foods are replaced by their dehydrated prototypes. The book then proceeds to give an account of the principles and methods of dehydration and the types of driers (or should it be dehydrators ?) in commercial practice. This is followed by detailed considerations of the points to be observed in the proper dehydration of vegetables, meat, fish, egg pulp, etc., as well as their storage. The difficult problem of compression in order to save additional storage space is also dealt with in some detail. Finally, methods are described for assessing the quality of different dehydrated foods, whilst two short chapters touch on bacteriological quality and the control of insect infestation.

It is almost gratuitous to criticise this book in any way but, for the sake of completeness at least, dehydrated bakers' yeast and bananas might have been included. Again—although this undoubtedly is a slip—no mention is made in the Preface of the part played by the Scientific Adviser to the Ministry of Food in defining and stimulating much of the earlier research. These, however, are small points in comparison with the excellence of the book as a whole. Its value can perhaps be measured from the fact that it is largely a digest of scores of published scientific papers; it is in fact food technology at the highest scientific level.

T. MORAN.

GEOLOGY

Vorräte und Verteilung der mineralischen Rohstoffe. By DR. PHIL. F. MACHATSCHKI. [Pp. viii + 191, with 6 figures.] (Vienna: Springer-Verlag, 1948. Swiss Frs. 16.–.)

THE aim of this small book is to present the facts of the distribution of mineral raw materials for the benefit not only of students but also of the

general public. The foreword recognises the importance of the subject for society in peace and war. An introductory chapter deals with the general supply position in regard to mineral deposits, classifies them, and illustrates their chemical composition with simple and effective circular diagrams, which emphasise particularly the little-known fact that oxygen forms nearly 50 per cent. by weight of the earth's crust. The second chapter treats of the occurrence and origin of mineral raw materials, and deals in a simple way with the geological aspects of the subject. The third chapter, which occupies most of the book, takes the more important economic minerals in turn, describes their chemistry, geology, origin and distribution within the principal political units of the world. The book concludes with a very comprehensive index. For those who read German this book provides a valuable summary of an exceedingly topical subject. That it is up to date is shown by the fact that four pages are devoted to radium and uranium.

G. W. T.

L'Érosion du Sol. By RAYMOND FURON. [Pp. 218, with 16 plates and 24 figures.] (Paris: Payot, 1947. Frs. 360.–.)

BROADLY speaking, human life on this earth is differentiated from wild animal life by the fact that man does not find his food all ready to hand in the produce of nature, but must needs cultivate the soil and work assiduously to develop the plant and animal life that is available. This involves a tremendous dilemma, since disturbing the balance of nature, which is essential for the continuance of the human race, exposes the soil to erosion in many different ways.

There are large areas in every continent which have been deforested or deprived of whatever might have been the native vegetation, and after being successfully cultivated for some time have become derelict because the soil is no longer protected by the original natural flora. The deserts of North Africa are but one example.

In the main section of this book Prof. Furon systematically surveys the erosion that has taken place in Europe, Asia, Africa, America and Australia and gives a useful and interesting historical account of all that has happened since the balance of nature was first disturbed. This section of the book is well illustrated with maps and is a good and concise survey of the dereliction of soil and the circumstances that have led to it.

This account of the present state of erosion is preceded by two chapters, one dealing with the general science and classification of soils and the second with the main causes of erosion. The account of the formation, constitution and classifications of soils in Chapter I is very elementary, but should be useful in giving a general account to those unacquainted with the subject of pedology. In the outline of the genetic classification of soils there is no reference to recent work or anything later than the classification proposed by Glinka. Indeed, it is noticeable throughout the book that, although published as recently as 1947, there are not many references to very recent literature.

Chapter II gives a simple account of laterisation, of the action of rain and wind, of disturbance of vegetation by animals and by the intrusion of human effort.

It is inevitable that in the main section of the book, describing the present

state of erosion, reference should be made to the methods of mitigating the problem, and this is particularly so in the part dealing with Japan, where systematic methods of controlling erosion have been in compulsory operation for a long time. The third part of the book, however, deals more particularly with the search for the remedy. There is a simple account of terracing and contour ploughing and the use of wind breaks and of irrigation, and the author calls for a more united and systematic survey of the whole problem.

N. M. C.

BOTANY

Drawings of British Plants. By STELLA ROSS-CRAIG, F.L.S. Part I: **Ranunculaceæ** [44 plates]; Part II: **Berberidaceæ, Nymphaeaceæ, Papaveraceæ, Fumariaceæ** [22 plates], with a Foreword by SIR EDWARD SALISBURY, C.B.E., F.R.S. (London: G. Bell & Sons, Ltd., 1948. Pt. I, 6*s.* net; Pt. II, 4*s.* 6*d.* net.)

AT different times a great many attempts have been made to illustrate the range of form and structure observable in British plants. These date back nearly four hundred years, but there has, as yet, been no complete series of drawings of native flowering plants which gives botanical details as well as the appearance of the whole plant.

In this series of drawings Miss Ross-Craig hopes to illustrate all the clearly defined species native in the British Isles, excluding, at least for the time being, critical species and microspecies whose status is still doubtful, and certain plants which are found only in the Channel Islands and nowhere else in the British Isles. Casuals and escapes from cultivation are omitted unless they are so frequent as to have become established over a wide area. The necessity of keeping the number of plates within reasonable bounds has prevented the inclusion of forms and varieties.

A work of this type, which must ultimately include at least 1500 plates, must take some years to bring to completion, and it is therefore to be published in parts, each complete in itself, consisting of one large family or of a group of smaller related families. The two parts thus already published exemplify the method of publication.

This new venture is a courageous attempt to meet what has long been a need for those wishing to identify British native plants and to students and others who are concerned with the details of their morphology. The complicated terminology which is necessary to describe the great variety of form found among plants often fails in its purpose by being incomprehensible to those who are supposed to have mastered it. The ideal has always been to have drawings which would show the essential features of form and for this purpose accurate drawings are necessary. At the same time, the representation of the plants as a whole requires an artistic capacity which has very often been beyond the powers of those capable of the requisite botanical accuracy. It may be said of the present venture that in these first two volumes Miss Ross-Craig has set a high standard in both respects and most botanists will wait hopefully for the issue of further numbers of this series.

W. H. P.

Botany of the Living Plant. By F. O. BOWER, Sc.D., LL.D., F.R.S. Fourth edition, revised and edited by C. W. WARDLAW, Ph.D., D.Sc., F.R.S.E. [Pp. xii + 699, with frontispiece and 507 figures.] (London: Macmillan & Co., Ltd., 1947. 36s. net.)

THE publication of another edition of a well-known book is not normally an occasion for a review, but special notice may well be given to the appearance of a fourth edition of Bower's *Botany of the Living Plant*—a work which has not been obtainable for some years, but which has still maintained itself as a favourite textbook. The present edition has been revised again with the collaboration of Prof. C. W. Wardlaw and is completely up-to-date, but the attraction of this work is that it was envisaged as a whole in the first place, and no other English botanical work gives so good an integration of the relations between form, structure and function. This foundation is fundamental to the study of plant form. It is here presented by a great teacher and investigator and will not easily be replaced. In its field, this is the best of English textbooks and one which all botanists will be glad to see re-issued.

W. H. P.

Soilless Growth of Plants. By CARLETON ELLIS and M. W. SWANEY. Second edition, revised and enlarged by TOM EASTWOOD. [Pp. x + 277, with 102 figures, including 3 coloured plates.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. 28s. 6d. net.)

IN the first edition Ellis and Swaney produced a non-technical volume largely intended for the general reader. They forecast the probability that soilless culture would make rapid strides and become of considerable commercial, as well as scientific, value. In America, at least, this has proved true, and Eastwood has summed up the present position in a revised and enlarged second edition. Unfortunately, however, the revision has been so drastic that little of the original can be traced even in the titles of the chapters, and practically a new volume has resulted; no acknowledgment of this has been made in the foreword or elsewhere, which seems hardly fair to the original authors.

During the last ten years many firms and government-sponsored schemes, especially in the tropics, have developed various methods of sub-irrigation and gravel cultures with various degrees of success, both from the commercial point of view and as a means of providing vegetable crops in areas where drought and excessive heat renders it difficult to grow such types of food under ordinary soil conditions. Eastwood describes the very varied equipment, methods of manipulation, and the weak as well as the strong points of the outfits.

Occasionally preliminary success has later been followed by comparative failure, and suggestions are made as to the possible causes of this, two chapters being devoted to the technical control of the nutrient solution and of the plant cultures. Many practical working details are given, with information about obtaining the necessary chemicals, the best methods of making up the culture solutions, and the analytical tests required during growth. A chapter on "common detriments" helps to dispel the general idea that plants grown in soilless culture are immune from pests and diseases, suggesting precautionary methods for reducing possible losses from these causes.

As time goes on, it is likely that considerable advances and modifications will occur in hydroponics, but meanwhile this revision provides a most useful work of reference not only for the commercial grower, but for scientific workers in the same field, and for amateurs experimenting with the methods on a small scale or in the home.

W. E. B.

BIOLOGY

Animal Colour Changes and their Neurohumours. By G. H. Parker. [Pp. x + 377, with 126 figures.] (Cambridge: at the University Press, 1948. 30*s*. net.)

This book deals only with those colour changes that are produced by the dispersal or concentration of pigments in special cells, the chromatophores; there is no reference to phenomena such as the plumage changes of birds. However, the chromatophores have a right to this exclusive treatment, for they are true effector cells, third in importance to muscles and glands. Other pigmentary colour changes come as a result of pigment deposition or change in cells whose primary function is other than the control of the animal's colour.

To those who are already interested in the subject, this book is obviously important; it is the first large-scale review published since 1914, and its author is one of the best-known students of the subject: 74 publications are listed under his name in the bibliography, which covers the period 1914–43. (There is also a useful short-list of works of historical importance, and of general reviews published before the earlier date.) Zoologists will now be able to find their way more readily through the mass of research work in this subject which has, unfortunately, become more complicated with the increase in our knowledge. This complexity is partly the consequence of the great uncertainties that still remain in the analysis of the mechanism of chromatophore action. For example, if one seeks the secret of that famous animal the chamæleon—which can change by fine gradations from orange to yellow, to green, then to dark olive, or blue grey; or perhaps through flesh-colour to red-brown and grey—much is still mysterious. In the first place the chamæleon has four kinds of chromatophores, and for only one of these, the melanophores, has the mechanism of colour-change been studied. And even for the melanophores there is still uncertainty; there is some nervous control and some through hormones, both via the eye; and there is some control through the direct effect of light on the chromatophore.

This association of two types of control, nervous and hormonal, recurs throughout the animal kingdom. There may be a direct nervous control, such that stimulation of the appropriate nerve will cause a colour change; or there may be instead, or in addition, a control through substances of the kind of hormones. This is the basis of Parker's attempt at a synthesis, anticipated in the word "neurohumours" in his title. It has often been suggested that all nerve fibres stimulate the effector cell with which they are connected through the mediation of substances similar to hormones secreted in minute quantities at the point of contact of fibre and effector. Although he does not discuss this broader physiological question, Parker uses a view like this to show that the duality of the chromatophore control is deceptive; all is due to humours because nerves work through humours anyway. Most readers will not be impressed by this side of the book; much

of the argument is insecurely founded on verbal jugglery, and a grand simplifying analysis does not emerge.

The general reader, if he glances at the book, will be amazed by the absence of coloured plates. And of some of the half-tone plates the biologist will suspect that they were sent by radio from the American author to the Cambridge publisher. But no criticism can detract from the importance of this book. The legends to the two halves of Fig. 11 are reversed.

W. H.

Life: Its Nature and Origin. By JEROME ALEXANDER. [Pp. x + 291, with 31 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. 30*s*. net.)

FEW men would have the courage to attempt the survey of an all-embracing subject such as this and fewer still to encompass it within a book of 300 pages. One would expect such a work to be either superficial, while ranging far and wide over the biological scene, or to be developed comprehensively along the author's own particular line at the expense of the remainder of the field. Mr. Alexander's book, however, comes into neither category. He shows a clear grasp of many subjects outside his own main interests of colloid science and biocatalysis, for he ranges from atomic physics and molecular structure through catalysis, chemotherapy, immunology, genetics and embryology to his own philosophy of science and religion. While some points, in particular the practical aspects of atomic bombs, may seem incongruous, the book is, thanks to the author's clear and imaginative style, far more readable than would be suggested by its assorted contents. The reader's interest is stimulated continually by the author's sense of humour; subtly expressed by an unusual turn of phrase, but never used other than to make a point clear. The historical aspects of the colloidal state and of catalysis are made particularly interesting by quotations from Graham and Berzelius—in fact, it is in these two concepts, in particular catalysis, that Mr. Alexander sees the whole basis of life.

The first chapter "How did Life Originate?" recounts the standard views and can be no more than speculative. The discussion of the rapidly dissolving boundary between the living and the non-living, with the viruses occupying an embarrassing intermediate position, is perhaps unjustly short. Reference to the papers of Pirie could with advantage have been included. The chapter on biocatalysis is perhaps the most stimulating part of the book. The chemical viewpoint that catalysts are merely accelerators has overshadowed the original opinions of Berzelius, who regarded them as initiators of reactions—a view now more acceptable to biochemists. Justifiable emphasis is laid upon the concept of catalytic surfaces, *i.e.* to a fixed spatial arrangement of catalysts built into the cell structure. Mr. Alexander takes up this point again in developing his theories of antibody formation, although the purely speculative nature of these views is not sufficiently emphasised. Nevertheless, these speculations are of interest, as is also their extension to genetics and evolution, which are regarded basically as the inheritance of existing and potential biocatalysts. The section on catalysis would have been of greater value had less space been devoted to industrial catalysis and more to the biochemistry of enzyme action, *e.g.* hydrogen activation, oxidoreduction systems, enzyme-substrate interaction.

Most "introductory" books on genetics are esoteric to a degree which

discourages those approaching the subject, but Mr. Alexander's chapter on this subject is a refreshing exception, giving a clear picture of chromosomal behaviour, although the chromosome maps that he includes require far more discussion than can be given in the allotted space. This part could be improved by a more careful definition of some of the terms and also by a discussion of the chances against Mendel's experiments giving the expected results. The chapter on embryology, based mainly upon the works of Needham, contains a timely warning against the mesmeric influence of technical expressions "which for the most part are merely the terminology of enlightened ignorance."

Some errors and obscurities are inevitable in a book with this breadth of outlook. "Reuse" (p. 102) looks odd, as does "The Origin of the Species" (p. 250), while "all atoms of every element are precisely alike" (p. v) and "Dextrose. Formed on protein breakdown" (p. 225) are both misleading. The chemical structures of thiamine and biotin (p. 76) are known and, in fact, are given elsewhere in the book. Although much space is allotted to diagrams of phthalocyanin and nickel phthalocyanin, it is not apparent what bearing this has on nickel catalysis. References to "*Sir* E. A. Adrian" require correction.

The gap between the popular exposition and the scientific monograph is too great to be bridged except on rare occasions. The appearance of Mr. Alexander's book is such an occasion. Its chief value will be to enlarge the outlook of the scientist who feels the urge to look over the edge of his own rut and to see how his neighbours are progressing. It can be confidently recommended as a mine of information and ideas to the serious biologist in his not-so-serious moments.

E. F. H.

Submicroscopic Morphology of Protoplasm and its Derivatives. By A. Frey-Wyssling. Translated by J. J. Hermans and M. Hollander. [Pp. viii + 255, with 161 figures.] (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. 32*s.* 6*d.* net.)

Prof. Frey-Wyssling has devoted most of his scientific life to adventures in many of the fields explored in this book, and his *Submikroskopische Morphologie des Protoplasmas und seiner Derivate*, published in 1938, is already well known. The present monograph is effectively a new edition, brought up to date and translated into English. The translation obviously follows too closely the German text to be described as idiomatic English, and there is a fair number of misspellings and misprints besides; but, barring such phrases as "The necessary blindage of the scattered light" (p. 83) and such descriptions as "Fig. 86 shows molecules of hemocyanin clothed with evaporised gold cast down in oblique direction" (p. 85), the story runs along easily enough and cannot fail to increase still further the prevailing interest in this fascinating subject.

The treatment falls into three main sections: the first provides the background of fundamental principles and methods, the second summarises what is known about the submicroscopic structure of cytoplasm, nucleoplasts, and erythrocytes, while the last is concerned with the specialised derivatives of protoplasm—cell walls, starch grains, nerve, muscle, collagenous and keratinous tissues, and the like. Prof. Frey-Wyssling writes predominantly from

his own viewpoint (and the present reviewer, for one, could not subscribe to much of what he says about the proteins at least), but that is not unnatural or blameworthy on topics so comparatively young and controversial.

W. T. A.

Genetics. By H. KALMUS, Sc.D. and M.D. Prague, in collaboration with LETTICE M. CRUMP, M.Sc. [Pp. viii + 171, with 24 figures.] (London: Penguin Books, Ltd, 1948. 1*s.* 6*d.* net.)

No difficulty whatsoever is encountered when the geneticist demonstrates to himself the importance of his subject in application in the fields of animal and plant breeding, of medicine, even of politics, yet when the geneticist attempts to persuade the breeder, the doctor and the politician that the possession of an understanding of genetical theory is an essential part of their equipment, difficulties at once arise. Whilst the technical terms of many other branches of science have long since claimed their places in the everyday vocabulary of the "practical" man, those of genetics are very seldom included, and, whereas an adequate understanding of the elements of nutritional or sex physiology can be offered and accepted with relative ease, it has to be acknowledged that the majority of attempts to equip the practical man with an equivalent appreciation of the principles of genetics have ended in complete failure. That this is so is not only puzzling but most regrettable, at least from the point of view of the geneticist, for to him it would seem most necessary that such an understanding should be possessed by all who are concerned in policies and activities which, directly or indirectly, touch upon the manipulation of the characterisation of man, animal or plant. Indeed, today it can be maintained that without a sufficient acquaintance with genetics no one can hope to understand the biological bases of the political theory that prevails in the U.S.S.R.

One reason why genetics is not understood is that this science has grown with phenomenal speed since the beginning of the century to claim its present predominant place amongst the biological sciences. So rapid has been this growth that genetics is not completely understood even by professionals in the other branches of biology, who commonly have failed to keep abreast of its progress. Another reason undoubtedly is that for an understanding of genetics a certain knowledge of other sciences is demanded, a knowledge of zoology, of botany, of anthropology or of clinical medicine, together with some insight into statistics. Furthermore, whilst the achievement of an understanding of genetical principle is not beyond the powers of the average individual, it does demand a certain intellectual effort. But why an appreciation of such simple genetical terms as "heterozygous," "meiosis," "linkage" or "mutation" should prove to be so difficult to those who can speak so easily and with apparent understanding of the technicalities of nutritional science or of the intricacies of aeronautics remains a mystery.

To the geneticist engaged in the teaching of his subject this book would seem to be a remarkable presentation of the subject in the simplest possible style, yet with complete accuracy and no little charm. To him it would appear impossible that anyone who wished to secure an adequate understanding of genetics could fail to do so, having not read but studied this book. It is quite amongst the best of its kind and is completely up to date. Since it has a glossary, there can be no excuse for any kind of misunderstanding on the part of the reader. It is to be strongly recommended and would

form an admirable textbook for those school forms where biology is taught, as well as an admirable introduction to general genetics for medical students or students of agricultural or veterinary science, who later are to receive instruction in the special genetics of man, plant and animal respectively. What is not least remarkable about the production is that such a manual dealing in such a way with so important a science should be available at the price at which it is offered.

F. A. E. C.

Hallmarks of Mankind. By FREDERIC WOOD JONES, D.Sc., M.B., B.S., F.R.S., F.R.C.S. [Pp. vi + 86, with 23 figures.] (London: Baillière, Tindall & Cox, 1948. 10*s.* 6*d.* net.)

IN this short book, written with his usual vigour and skill and illustrated with his own pleasing pen-drawings, Prof. Wood Jones reiterates some of the views concerning human evolutionary relationships which he has expounded in more detail in other works. In the first part he enumerates several propositions about the Primates, and proceeds to demolish them. He points out that parallel evolution (the independent appearance of closely similar features in two or more lines which have already diverged from a common ancestor) has invalidated some of the classic evolutionary series which seemed adequate at the end of last century; this we know from more abundant fossil material. Applying this consideration to Primates, he argues that the lemurs must have diverged from the ancestor of other Primates at a very early stage of mammalian evolution, and might legitimately be removed from the order altogether. He regards Tarsius as a genuine member of the Anthropoidea, a representative, in spite of its various specialisations, of the primitive ancestral form of this group, and for this reason more closely related to the ancestors of Man than are the Old World monkeys or the anthropoid apes. For Prof. Wood Jones has always emphasised the basic primitiveness of human structure and has combated the view that man evolved relatively recently from any form that could rightly be called an anthropoid ape. Since fossil tarsoids already show evidence of specialisation at the beginning of the Tertiary, this would imply the divergence of Man at a very remote period. He emphasises the various features of the living and Miocene apes (the latter known almost entirely from jaws) which mark them as divergent offshoots modified for a peculiar mode of arboreal progression, and he denies the view that man acquired the fundamental property of upright progression as a result of an arboreal apprenticeship of this type. It is surprising that he does not discuss the bearing which the recent discoveries of the Australopithecine fossils in South Africa have on these contentions, but dismisses them as true apes, albeit more primitive and therefore more closely resembling man than any previously known. One would have thought that the evidence, both geological and anatomical, that these forms were not arm-swinging tree dwellers, and could walk upright, and even possibly use their hands for wielding crude weapons, would prove most welcome to him. Moreover, one authority at least, who has examined the remains, reports human features too numerous and detailed to be plausibly interpreted as parallelisms, and considers that the Australopithecinæ show no close affinity with the recent anthropoids. It is true that the structure of the premaxillary bone, a feature on which Prof. Wood Jones focusses great attention, as a unique peculiarity of man, is not yet fully elucidated in these fossils. One wonders, however,

when it is argued that the burying of the facial part of this bone beneath the maxilla at an early stage of embryonic development denotes an early divergence of man in evolution, just how useful the time-scale of ontogeny is when it comes to measuring the time-scale of phylogeny.

In the second part of the book, Prof. Wood Jones includes some discussion of general evolutionary mechanism. It is odd that, in citing the concurrent elongation of the giraffe's neck and legs as an evolutionary process, he finds it inconsistent with an explanation along Darwinian lines, and prefers to fall back on the vaguer principle of "the satisfaction of a need." A critical and humble neo-Darwinian would no doubt admit that no direct attack on this and many other examples of major evolutionary change is possible, and that we really do not know how giraffes came to be as they are; he might, however, claim at least a methodological soundness in basing any speculation on experimental findings, even though these are derived from genetical work of restricted scope, and on less spectacular examples of the evolutionary process.

N. A. BARNICOT.

PHYSIOLOGY AND BIOCHEMISTRY

Annual Review of Physiology, Vol. X, 1948. VICTOR E. HALL, Editor. JEFFERSON M. CRIMSON and ARTHUR C. GIESE, Associate Editors. [Pp. xii + 552.] (Stanford, California: Annual Reviews, Inc., and American Physiological Society, 1948. $6.00.)

THE *Annual Review of Physiology* now enjoys an established place as an indispensable item in the library not only of the physiologist but also of those engaged in the related medical sciences; and this present volume, the tenth in the series, well maintains the high standard set in previous issues. In twenty-three highly documented articles on topics of major interest in the field of general physiology, the reviewers have for the most part dealt well with the unenviable task of presenting in readable form within a somewhat restricted compass the chief developments during the past year or so in their speciality and of relating these to the previous state of knowledge, although a few of the contributions are marred by a tendency to include trivial items of information for the sake of completeness. Although, as the editors remark in the Preface, they are keenly aware of their responsibility for achieving a truly international scope in authorship and coverage, the bulk of the articles are of American origin. Vass and McMichael in this country review the Digestive System and Heart, while Lee (Australia) and Eccles (New Zealand) write on Heat and Cold, and Conduction and Synaptic Transmission in the Nervous System, respectively.

Among the many items of topical interest in this volume are accounts of peptic ulcer, folic acid, the hæmatological effects of the nitrogen mustards, urethane and stilbamidine, cardiac output and intracardiac pressures, the metabolic aspects of shock, antithyroid drugs, psychosomatic relationships in physiological states, and hypertension. To those who, like the reviewer, find particular pleasure in reading of recent advances in some field of interest quite unrelated to their own, one may recommend Beadle's fascinating review on the Physiological Aspects of Genetics, with its accounts of the mechanism of gene action, bacterial genetics, genetics of viruses and chemically induced mutations.

R. A. GREGORY.

Feeding the Human Family. By F. Le Gros Clark, M.A. Sigma Introduction to Science 11. [Pp. x + 125, with 8 plates and 14 figures.] (London: Sigma Books, Ltd., 1948. 7*s*. 6*d*. net.)

One might well ask about Mr. Le Gros Clark's book in the "Sigma Introductions to Science" series, *que diable allait-il faire dans cette galère?* For if ever a book did not merit the description of this particular series, it is this one. True, in the course of it the author is bound from time to time to give some elementary instructions about the scientific bases of modern nutritional theory or practice, but that is incidental—almost accidental.

No, this book is a sociological essay on a world-wide theme, by a recognised British expert; it demonstrates clearly that if he is not recognised also as an international expert, so much the worse for international affairs. It embodies the results of many years' study and thought and gives conclusions that are as incontrovertible to the scientist as they must be alarming to the statesman, if he understands them.

The world holds too many people for its food supply. The former are increasing in numbers more rapidly than the latter is increasing in volume. Potentially there is little hope of reducing the rate of population increase, but there are many indications that technical knowledge to-day, if it could be applied without fear or favour—that is, without considerations of finance, politics and nationalist restrictions—could bridge the gap and raise the world's nutritional status in the course of a few years.

Typical both of the author's views and of his method of presenting them are, for instance, "The fact is that this problem of how to dispose of farm surpluses without burning them is the core of the whole matter. If we cannot solve it, we might as well shut up shop" (p. 71) and "Especially will it be necessary for governments to relinquish their traditional belief that the peasant or the primitive cultivator, as we know them, should be preserved as a heritage or survival, as a stabilizing factor on the community or as a source of man-power in case of war. The peasant working for his own subsistence has become an anomaly in our modern civilization; and he must be replaced by the farmer producing in part for a food market, in which he himself has a share" (p. 84). The author is continually bringing the reader back from a consideration of specific examples—it may be a comparison between pre-war Bulgarian and United States diets, or meat consumption in Mexico City and rice consumption in India, or stock-breeding by the native population in South Africa—to some clear-cut general statement that offers, or should offer, a basis for developing plans and initiating action.

It is impossible to discuss these plans and action here, and indeed it would be in part to frustrate Mr. Le Gros Clark's intentions for a reviewer to do so. The important thing is for the largest possible number of individuals to read the book itself, to follow its argument and to consider its conclusions. Moreover, that is why it seems fair to criticise the inclusion of this book in a series of "Introductions." Though this may encourage the non-scientist to read it—much to his benefit, for he will meet in it many shrewd observations on scientific and technical matters—it must on no account be an excuse for the expert to brush it aside, saying "Oh, I know all this." He will find a brilliant and often disconcerting new light thrown by the author on much that has been taken for granted. In these pages Mr. Le Gros Clark has indeed stated with astonishing clarity just what it is that *homo sapiens* has to decide if his species is to have even a sporting chance of averting the world

catastrophe that must follow further deterioration in man's nutrition. Anyone who fails to study Mr. Le Gros Clark's arguments and conclusions may be generically *homo*—but can he be justly described as *sapiens* ?

A. L. BACHARACH.

PHILOSOPHY AND HISTORY OF SCIENCE

The Philosophy of a Scientist. By R. G. GORDON, M.D., D.Sc., F.R.C.P.Ed. [Pp. 206.] (London: Hutchinson's Scientific and Technical Publications, 1948. 16*s*. net.)

THIS is a scholarly and thought-provoking book, enriched by many quotations from poets and other sources. But it was written during World War II and consequently is somewhat outdated by the vexatious delays in publication to which British writers are subjected nowadays during rapidly changing world conditions.

Various passages might well have been corrected during this period. For instance, belief in the significance of the soul goes back beyond the Ancient Greeks to the Egyptians and others (p. 9); and the Golden Age of Greek thought was not 5000 years ago (p. 51) but has about half that antiquity, as stated correctly later (p. 95). The author's thesis of human "progress" (p. 89) is linked with nineteenth-century or earlier beliefs which many modern philosophers and practical men have discarded; and in this atomic bomb era the assumption of an indefinitely long innings for *Homo sapiens* seems unwarranted. There are too many assumptions behind the hope that the human race will unaided strive increasingly after goodness, truth and beauty and so achieve harmony, universal happiness and what the author, following S. Alexander, calls "Deity." That "man" has increasing power to mould and control his material environment is undeniable; but in practice this tremendous authority is in the hands of the leaders of various human groups with utterly contrasting ideals. The inherent difficulties are indicated by the long list of "musts" needed for virtue on even a national scale (p. 176).

Dr. Gordon criticises the crude materialism found in some scientific writing; but many readers will regard his own outlook as but a refined variant of the same theme; and to this reviewer the use of "GOD" for something secondary, human, or non-existent is unjustified. Leading modern physicists have upheld the theistic beliefs of their famous predecessors; and the "reverence" which J. S. Huxley and our author find inherent in human nature surely supports something stronger than a "humble agnosticism" (Chap. XI). The "zest" of reformers like Shaftesbury was based on Evangelical Christianity; and the relations of C.M.S. and Methodist missionaries are very cordial (*cf*. p. 172).

It is pleasant to find recognition given to co-operation among animals as described by Kropotkin (pp. 91 *seq.*); but nothing is said of the amazing co-operation found among plants and between them and the animal kingdom.

Finally, the volume is pleasantly produced but its cost very high.

L. RICHMOND WHEELER.

Sun, Stand Thou Still; The Life and Work of Copernicus the Astronomer. By ANGUS ARMITAGE, M.Sc., F.R.A.S. Life of Science Library. [Pp. x + 210, with 7 plates and 16 figures.] (London: Sigma Books, Ltd., 1947. 12*s*. 6*d*. net.)

THE realisation that the earth is not a fixed object at the centre of a revolving universe has come only gradually to mankind. The recognition that the globe upon which we live rotates daily on its axis, that it is but one of a number of planets travelling in orbits round the sun, that when we look up into the starry sky we see other suns and other systems at vast distances from us, these steps in our appreciation of the structure of the universe constitute one of the great revolutions in human thought. The evidence has accumulated gradually, and at every step the revelations of the astronomers have been opposed by superstition, ignorance and incredulity.

One of the vital links in the chain was that forged by Nicholas Copernicus, for it was he who satisfied himself that the motions of the heavenly bodies were not to be explained except on the theory of a sun-centred universe. By the publication in 1543, the last year of his life, of his great work *The Revolutions of the Heavenly Spheres*, he convinced the astronomers who followed him of the correctness of his conclusions, and thus overthrew the older Ptolemaic system which placed the earth at the centre.

The author sets out briefly but with admirable clarity, in the three sections of his book, the progress of astronomy before Copernicus, the story of the man and his work and the subsequent history and triumph of his theory. It is interesting to learn that to his contemporaries Copernicus was better known for his other activities than for his astronomy. Although he held a clerical office as canon of Frauenburg, he was in fact an administrator and man of affairs. Owing his lengthy education at Cracow and afterwards in Italy, and his subsequent progress, to the support and patronage of his kinsman, Lucas Waczenrode, Bishop of Ermland, he became a man of great learning and varied accomplishments—scholar, physician and economist as well as astronomer.

But it was in astronomy that he made his greatest contribution to knowledge, as one or two of his closest friends realised. His observatory was a tower in the outer wall of Frauenburg Cathedral, where he worked with crude instruments, for it was before the days of the telescope. He was not himself a great observer, depending much on the observations of others, and greatly hampered by their inaccuracy. By the use he made of the data he painstakingly accumulated, however, he reached his great generalisation, which, as Dr. Armitage points out, was in the nature of a choice rather than a discovery of something new. The book is an excellent introduction to the work of one of the founders of modern knowledge and to the history of early astronomy; and it deserves to be widely read.

T. M.

The Naming of the Telescope. By EDWARD ROSEN. Foreword by HARLOW SHAPLEY. [Pp. xvi + 110, with frontispiece and 15 figures, including 4 plates.] (New York: Henry Schuman, Inc., 1947. $2.50.)

IN this little book, whilst never departing from the theme expressed in his title, Dr. Rosen manages to convey an entertaining picture of the scientific activities of those philosophers of Renaissance Italy from whom our present men of science are in the direct line of intellectual descent. The story of the

invention of the telescope has been told many times, but in his search for the originator of the name of the instrument Dr. Rosen is covering relatively untrodden ground. The search makes enough of a detective story to render it unfair to the author to reveal his solution: suffice it to say that it is not arrived at without a good deal of painstaking research, in which the various ascriptions to Cesi, Demisiani and others are critically examined and various clues are pursued into fascinating sidelines. The triumph of the new word over the earlier *organum*, *instrumentum* and *perspicillum* was sudden and complete, and in describing it Dr. Rosen contrives to breathe life into what might seem at first glance a trivial investigation.

The book will be of less interest to users of telescopes than to bibliophiles and students of the history of science. The latter will be intrigued by a reference in Dr. Shapley's foreword to an occasion (the banquet given on April 14, 1611, by Cesi in Galileo's honour) on which Galileo "trifled with his customers, as astronomers occasionally do, by showing them upside-down views of terrestrial objects." The relevant dates do not exclude the possibility that Galileo had heard of Kepler's extra-focal use of a convex eye-lens, and in any case Galileo might himself have independently discovered the advantages of such a system; but if the telescope were of this type it is surprising that contemporary accounts of the banquet do not, in describing the incident, mention the inversion of the image. This would surely have been, then as now, a striking feature to those using the telescope for the first time. And indeed Sirturi (writing, it is true, seven years later) describes how Galileo satisfied his fellow-guests' curiosity by taking out the lens and the *concave* eyepiece.

A. H.

The Growth of Physical Science. By Sir James Jeans, O.M. [Pp. x + 364, with 14 plates and 39 figures.] (Cambridge: at the University Press, 1947. 12*s.* 6*d.* net.)

It is fitting that the last book written by the late Sir James Jeans should be a history of the mathematical and physical sciences to which he himself contributed so much. Our only regret is that this venture into the domain of the history of science did not produce a result worthy of unstinted praise.

Sir James says, in his short preface, that his aim was not to add to the number of detailed histories of science, but rather to try to describe the main lines of advance of physical science, including astronomy and mathematics, in language which could be understood by the general reader with little or no scientific knowledge. As far as tracing the main course of development is concerned, he succeeded admirably: the reader is carried along from the beginnings in Babylonia and Egypt, through Greece and Alexandria, Islam and the Dark Ages, to the birth of modern science, and so through the following three centuries of achievement, down to the present day. The reader for whom the book is intended will find little here that is beyond his understanding: difficulties are smoothed away with a few well-chosen words of explanation or an apt comparison. Even the last chapter, on the new era in physics, is easy reading.

But a history, whether of science or any other branch of human activity, must not only present a true picture of the development of the subject as a whole, but must also be accurate in detail. It does not matter that the book in question is written for the general reader and not for the more serious

student. The history of science has perhaps suffered more from lack of accuracy in detail than that of most other subjects, possibly because in the past it has been written mainly by scientists whose preoccupation with their particular sciences has left them little opportunity for historical research. There is, however, at the present time, a growing literature in the history of science embodying the results of research based entirely on original sources, so that many time-honoured errors and misconceptions are tending to disappear. This makes it difficult to understand how the present book has come to contain so many errors, all the more to be deplored when so distinguished a name as that of Jeans adds weight to the statements.

A few only of the more obvious inaccuracies can be mentioned here. The diagram on p. 142 departs quite pointlessly from that given by Stevin, an endless chain being substituted for Stevin's spheres connected by a cord. Gilbert did not introduce the word "electricity" (p. 152), the earliest known use of which occurs in a work by Thomas Browne published in 1646; all Gilbert did was to coin the word *electrica* to denote substances showing the amber phenomenon; again, the statement on p. 286 that Faraday "adopted the same conception of lines of force as Gilbert had used in his *De Magnete*" credits Gilbert with more than his due. Du Fay called his two electric fluids "vitreous" and "resinous," not "positive" and "negative" (p. 277); Franklin first applied the terms positive and negative to electricity. Galvani's discovery was made in 1780, not in 1793 (p. 279); and with an electric machine, not a Leyden jar. Nicholson and Carlisle's discovery was not made in the manner stated (p. 281). The paragraph on Ohm (p. 280) is very misleading. Other statements are at least open to serious question, as, for instance, that the mariner's compass was invented by the Chinese and introduced into Europe by Mohammedan sailors (p. 276).

The uncritical reader will, however, derive much pleasure and no little profit from a perusal of the book; the more critical may add to both pleasure and profit by correcting the errors.

N. H. de V. H.

Lord Kelvin. By A. P. Young, O.B.E., M.I.E.E. [Pp. vi + 41, with plates and 1 figure.]

James Clerk Maxwell, F.R.S., 1831–1879. By R. L. Smith-Rose, D.Sc., Ph.D., M.I.E.E. [Pp. vi + 34, with 7 plates and 1 figure.]

Science in Britain Series. (London, New York, Toronto: Longmans, Green & Co., for the British Council, 1948. 1*s*. 6*d*. net each.)

No survey of British science would be complete without some account of that part of it which is identified with the names of James Clerk Maxwell and William Thomson, Lord Kelvin; and these two brochures are thus very welcome additions to the admirable series on Science in Britain, produced by the British Council. The Council has from time to time been the object of criticism, some of it spiteful and much of it ill-informed, and it is therefore pleasant to be able to speak in appreciation of that aspect of its work with which we are here concerned. Those who have had the opportunity of seeing the books in this series, and others of a comparable nature, in use in overseas countries, have no doubt that the Council is performing one of its most valuable services to our country in thus disseminating a knowledge of British cultural activities.

Maxwell and Kelvin—two of the giants of physical science. Maxwell's great achievement, the electro-magnetic theory of light, is well known; how he clothed in mathematical form the earlier, non-mathematical reasoning of Faraday on the propagation of magnetic disturbances, and reached his conclusion that the velocity of the impulses is identical with that of light. The subsequent experimental verification of Maxwell's predictions by Hertz, and the immense practical consequences which have flowed from the full development and application of the theory of wave propagation, are now equally well known. Dr. Smith Rose describes this aspect of Maxwell's work, and also emphasises, what is perhaps less popularly appreciated, the fundamental character of his contributions to molecular physics and the kinetic theory of gases. It is an excellent account of the short and brilliant life—for Maxwell died at 48—of the greatest mathematical physicist of the nineteenth century.

Kelvin's genius was of a different character. In his long and fruitful life he not only made important theoretical advances, particularly in the field of thermodynamics, but as an engineer applied his wide knowledge of physical principles to invention. His work on the fundamental electrical units and on the perfection of precise measuring instruments has been of outstanding importance. His contributions to the art of navigation include the improvement of the mariner's compass, and he was a pioneer in the laying of the Atlantic cable. Mr. Young describes with much sympathy and understanding the life of this remarkable figure of the Victorian age, whose prejudices and limitations serve only to throw into relief the immensity of his achievement.

T. M.

The Early Work of Willard Gibbs in Applied Mechanics. Assembled by L. P. WHEELER, E. O. WATERS and S. W. DUDLEY. [Pp. viii + 78, with 3 plates and 35 figures.] (New York: Henry Schuman, Inc., 1947. $3.00.)

To mark the occasion of the centennial anniversary in 1947 of the Sheffield Scientific School, Yale University, some early work of Willard Gibbs, its most distinguished son, has been brought together and published for the first time by the Historical Library of Yale University. The work includes a paper entitled "On the Form of the Teeth of Wheels in Spur Gearing," which is stated to be the dissertation submitted by Gibbs for the degree of Ph.D. in 1883. There is no direct evidence, however, that this is so, though circumstantial evidence points to its probability. The other two items consist of notes of a railway brake invention, which Gibbs patented, and a description of a conical-pendulum governor for steam engines.

It is perhaps natural that the enthusiastic compilers of this little volume of Gibbsiana should believe that they have found many indications of genius in these early efforts of Gibbs, but to an unbiassed reader it is difficult to conclude that the dissertation is anything but mediocre. The publication of this youthful work is of some biographical interest, but it does not, of course, influence the pre-eminent position which Gibbs occupies as a mathematical physicist. One cannot help feeling, after reading this book, that, if Gibbs had not had the opportunity, soon after taking his degree, of visiting Europe, and discovering new horizons of science, his genius would never have blossomed as it did.

A. T. P.

The Alchemist in Life, Literature and Art. By John Read, Ph.D., M.A., Sc.D., F.R.S. [Pp. xii + 100, with coloured frontispiece, 29 other plates and 6 text illustrations.] (London: Thomas Nelson & Sons, Ltd., 1947. 10*s.* 6*d.* net.)

Here Prof. Read gives an account of his studies of the alchemist as a figure in bygone times, in literature and in art. He explains how alchemy arose and to what it led, its curious symbolism and language, its theories, what it drew from mythology and religion, astrology and magic, mysticism and music, and the part that all these strange factors have played in the "Great Work." The alchemist in literature is depicted from a study of the writings of Trismosin, Chaucer's *Canon's Yeoman's Tale*, Ben Jonson's *Alchemist* and the diaries of Simon Forman. In art, the alchemist has found many delineators and Prof. Read presents a well-illustrated study of their work from the paintings of Brueghel, Stradanus, Teniers, Steen, Wijck, van Ostade and others.

All interested in the history of science will find much to attract them in this book. A glossary of alchemical terms is provided and readers will find no difficulty in following the text.

D. McKie.

Humour and Humanism in Chemistry. By John Read, Ph.D., M.A., Sc.D., F.R.S. [Pp. xxiii + 388, with coloured frontispiece, 59 other plates and 30 text illustrations.] (London: G. Bell & Sons, Ltd., 1947. 21*s.* net.)

This book is a sequel to the author's well-known *Prelude to Chemistry* and seeks to complete the broad humanistic account of the development of alchemy and chemistry presented in the earlier work. In the first five chapters much is still said of alchemy, with interesting studies on John Damian, alchemist to James IV of Scotland and unsuccessful airman, on Alexander Seton, the Cosmopolite, and his wanderings in Europe until the final tragedy of his extraordinary life, on Michael Sendivogius and on the "gold-making" of van Helmont, Elias and Helvetius. The next four chapters deal with the transition from alchemy to chemistry, and here the author describes the work of Beguin, Davidson, Glauber, Le Febure, Glaser and Lemery and gives an interesting chapter on Boerhaave, followed by another on Black and a further one on the early history of the gases. Then the reader is told of daily life in the modern chemical laboratory from the days of Liebig on to the author's own time as a student in Zürich under Werner and later in Cambridge under Pope. The concluding chapters deal with work on Australian plant chemistry and with the dramatic element in chemistry.

The book justifies the author's contention that "it is not too much to claim that the study of chemistry, if approached befittingly, may reasonably take rank beside the so-called humanities as a broadly educative, cultural, and humanising influence; and that the specialised outlook which is becoming increasingly bound up with the trend of scientific research may be alleviated by the cultivation of an interest in the broad humanistic aspects of science." The text is splendidly illustrated and readers will find in it both profit and entertainment.

D. McKie.

The Royal Institution. By THOMAS MARTIN. Second edition. Science in Britain Series. [Pp. vi + 54, with 12 plates.] (London: Longmans, Green & Co., for the British Council, 1948. 1*s.* 6*d.* net.)

THE history of institutions is to be read in the lives of the men who have served them in office. So says the author of this book, and how true it is in relation to the Royal Institution. Davy, Young, Faraday, Tyndall, Dewar, Bragg—what would the Royal Institution have been without them? And we must not forget Count Rumford, to whom its foundation was due. After describing the origin of the Royal Institution, the author proceeds to relate its history in terms of those who by their work have made it into a place for which generations of scientific workers have held a special affection and to which overseas scientists inevitably come when visiting this country.

No one could have written a better history than the secretary of the Royal Institution. His knowledge and his facile pen have given us a fascinating story, which makes one unwilling to put down the book until one has finished reading it. Davy, Faraday and the rest all stand out from its pages as living people, and the story of their discoveries is set out in such simple language that those with only a smattering of science can enjoy it. The reviewer doubts whether better value for 1*s.* 6*d.* could be found in any other book.

F. P. D.

Science and its Background. H. D. ANTHONY, M.A., B.Sc., Ph.D., F.R.A.S. [Pp. x + 304, illustrated.] (London: Macmillan & Co., Ltd., 1948. 10*s.* 6*d.* net.)

ONE can only suppose that the author had an immature and ignorant class of reader in view when he wrote this book. In no other way can one account for the almost total exclusion of philosophical considerations which are at the core of the history of scientific thought. Or is Science merely a series of haphazard discoveries and inventions? Surely one must see in Science (spelt with a capital) nothing less than a great philosophical movement, the implications of which in thought and education need urgently to be explicated by books carrying just such titles as this one. It is consequently very disappointing that Dr. Anthony should seek to simplify this great subject and present it in the form appropriate to a school textbook for fifth formers. For assuredly it is not a subject for schoolboys.

Having rejected philosophy, it is not so surprising that Aristotle is treated merely as a classifier of facts, that Pythagoras and Plato are virtually omitted, that there are but three sentences on the Aristotelian philosophy in Scholastic thought and one sentence on Albertus and Aquinas together. One could go on to illustrate the extraordinary delusion that you can write the history of Science in this way. It is no reward to have one and a half pages on the history of Greece and Persia, the same on the rise of the Roman Empire, two pages on Alcuin of York, four pages on Columbus, eight on da Vinci—when Tycho Brahe gets barely a page and Descartes is never mentioned. Even as a summary of great scientific achievement the work is a failure. A lot of praise, for instance, is given to Newton and the *Principia*, including the usual couplet, but the contents of the *Principia*, like those of Galileo's works, is scarcely indicated. As for the overthrow of the Cartesian system and all the struggle and failure which lies behind the well-known record—there is nothing of all this.

The latter part of the book, dealing with the work of the nineteenth century onwards, is a mere digest of selected topics. Einstein is mentioned, but not Planck, and there is a good deal about the indomitable spirit of man. Vastly too many subjects are touched on in this disappointing book.

A. E. BELL.

MISCELLANEOUS

Bibliography of the Technical Literature on Silk. By F. O. HOWITT, Ph.D., F.R.I.C., F.T.I. [Pp. xxiv + 248.] (London: Hutchinson's Scientific and Technical Publications, 1947. 21*s*. net.)

THIS is an age of new fibres, but there is an enduring magic about natural silk, with its old, old romantic story, to quicken the imagination and give charm to the most routine of technical processes. It speaks of "the gorgeous East" whence, like chess, it came thousands of years ago. And like chess it still holds out its problems, and indeed problems of a higher fascination than ever; for even now, after all these years of delightful craftsmanship and artistic attainment—even now it leads the way into a new and adventurous world of protein studies. The title of Dr. Howitt's book may perhaps read a little dull, but behind it all lies a thing of beauty and wonder.

The idea, of course, is only to provide critical guidance to the numerous pathways into the silk world. The book is not a great tome, neither is it a mere list of over two thousand references: in addition to an Introduction and Glossary of Silk Terms, and Patent, Subject and Author Indexes, fifteen chapters are devoted to reviewing the subject-matter of the references; and lest the prospective reader should take the title too literally and assume that only or chiefly technological matters are considered, let it be stressed that the longest chapter of all (about 70 pages) deals with the properties of fibroin as a protein. The whole is the outcome of Dr. Howitt's resolve, on his appointment as Head of the Silk Section of the British Cotton Research Association, to collect and classify for the first time all the published data on silk that are scattered so widely and incoherently over the journals. The record brings us to the end of 1944, and for this well-planned labour scientists and technologists alike will feel extremely grateful to all at the Shirley Institute who have shared in it.

We have noticed a curious misprint in the section on byssus silk (p. 2) and again in the Subject Index, where the shell-fish, *Pinna nobilis*, is described as *Prima nobilis*.

W. T. ASTBURY.

Scientists Starred 1903–1943 in "American Men of Science." By STEPHEN SARGENT VISHER. [Pp. xxiv + 556, with 117 figures.] (Baltimore: The John Hopkins Press; London: Oxford University Press, 1947. 25*s*. net.)

IN 1903 J. McKeen Cattell inaugurated a system of listing the outstanding American scientists, and 1000 of these were starred in the first directory of "American Men of Science." The number was increased in successive editions, and the present volume is concerned with the particulars of 2607 scientists who had been so starred up to 1943. Various sources have been used to glean information concerning the starred scientists, including a questionnaire sent out by Cattell to the leading scientists of 1903 and a further

questionnaire sent to 1440 living starred scientists in 1946. The result is a monumental volume packed with information, the main objects being "(1) to find out where these leaders in American Science were born, educated and employed; (2) to discover what conditions correlate with their production; (3) to ascertain what can be learned from them or otherwise as to possible methods of increasing the number of such outstanding workers and of increasing scientific achievement; (4) to provide a supplementary recognition of the contribution made by these scientists and thus to encourage those who are living and honour those who have died."

The work is statistical in the sense of being a numerical compilation, with the data classified in every conceivable way, but it is also encyclopædic and biographical, with the names freely quoted. The conclusions can only be noted briefly. Notable persons are found to be a product of both environment and heredity, the most important environmental influences being those of home and community. It is asserted that, in order to increase the number of creatively productive people, there must be an increase in the number of children born and reared in families potentially able to contribute superior qualities both genetically and culturally. Physical or geographic environments have been studied, and the notable scientists and their parents were found exceptionally mobile, and alert to better their opportunities. Teachers are important, young and vigorous teachers (and university presidents) being distinctly advantageous. High ratings were found for curiosity, perseverance and enthusiasm, and low ratings for personal attractiveness and a good memory. But such a bald summary fails to do justice to the immense work put into the compilation.

J. Wishart.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Introduction to the Algebraic Geometry of a Plane. By J. W. Archbold, M.A., Lecturer in Mathematics, University College, London. London: Edward Arnold & Co., 1948. (Pp. xiv + 300, with 108 figures.) 25*s*. net.

A.C. Network Analysis by Symbolic Algebra. By W. H. Miller, A.M.I.E.E. Manchester: Classifax Publications, 1948. (Pp. 41, with 37 figures.) 4*s*. net.

Weather Forecasting. By Instructor Commander S. W. C. Pack, Royal Navy, M.Sc., A.C.G.I., D.I.C., M.I.E.E. London, New York, Toronto: Longmans, Green & Co., 1948. (Pp. 192, with 131 figures, including 8 plates.) 25*s*. net.

General Physics. By A. E. E. McKenzie, M.A., Trinity College, Cambridge. Cambridge: at the University Press, 1948. (Pp. viii + 499, with 369 figures.) 8*s*. 6*d*. net.

The Elements of Physics. By Alpheus W. Smith, Professor of Physics, Emeritus, The Ohio State University. Fifth edition. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. xvi + 745, with 4 plates, 917 figures and 22 tables.) 30*s*. net.

Introduction to Physics. By Harley Howe, Professor of Physics, Cornell University. Second edition. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. xii + 599, with 505 figures and 31 tables.) 27*s*. net.

Higher Physics. By E. Nightingale, M.Sc.(Vict.), A.R.C.O., Senior Science Master at St. Albans School. London: G. Bell & Sons, Ltd., 1948. (Pp. xvi + 808, with 746 figures.) 27*s*. 6*d*. net. Also in separate parts: Pt. I: Mechanics and Properties of Matter, 7*s*. 6*d*. net; Pt. II: Heat, 7*s*. 6*d*. net; Pt. III: Light and Sound, 10*s*. net; Pt. IV: Electricity, 12*s*. 6*d*. net.

Physics Revision Course for Higher School and Intermediate Examinations. By V. H. L. Searle, M.Sc., Lecturer in Physics, University College of the South-West, Exeter. London: Edward Arnold & Co., 1948. (Pp. viii + 120, with 74 figures.) 6*s*. 6*d*. net.

Heat. By Archie G. Worthing and David Halliday, University of Pittsburgh. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xii + 522, with 245 figures and 40 tables.) 36*s*. net.

The Interpretation of Spectra. By William Mayo Venable, Consulting Engineer, Blaw-Knox Co., Pittsburgh, Pa. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. (Pp. vi + 200, with 12 figures and 73 tables.) 36*s*. net.

Multiple-Beam Interferometry of Surfaces and Films. By S. Tolansky, Professor of Physics, Royal Holloway College, University of London. Monographs on the Physics and Chemistry of Materials. Oxford: at the Clarendon Press, 1948. (Pp. viii + 187, with 113 figures.) 18*s*. net.

Vision and the Eye. By M. H. Pirenne, Dr. Sc. (Liège), Ph.D. (Cantab.), Imperial Chemical Industries Research Fellow of the University of London. Frontiers of Science Series. London: The Pilot Press, Ltd., 1948. (Pp. xx + 188, with 4 plates and 99 figures.) 12*s*. 6*d*. net.

Wave Mechanics and its Applications. By N. F. Mott, Melville Wills Professor of Theoretical Physics in the University of Bristol, and I. N. Sneddon, Lecturer in Natural Philosophy in the University of Glasgow. Oxford: at the Clarendon Press, 1948. (Pp. xii + 393, with 68 figures and 11 tables.) 30*s*. net.

Microwave Transmission Circuits. Edited by George L. Ragan, General Electric Research Laboratory. Office of Scientific Research and Development, National Defense Research Committee. Massachusetts Institute of Technology Radiation Laboratory Series, Vol. 9. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. xvi + 725, with 624 figures and 45 tables.) 51*s*. net.

Electromagnetic Waves and Light. First Part. By Charles F. Meyer, Associate Professor of Physics, University of Michigan. Distributed by Ulrich's Book Store, 549 East University Avenue, Ann Arbor, Michigan, 1948. (Pp. xii + 83, with 29 figures and 5 tables.) $1.35.

Radar Scanners and Radomes. Edited by W. M. Cady, Head, Physics Division, U.S. Naval Ordnance Test Station, Pasadena Annex, M. B. Karelitz, Assistant Director of Research, General Precision Laboratory, Inc., Louis A. Turner, Head, Department of Physics, State University of Iowa. Massachusetts Institute of Technology, Radiation Laboratory Series, Vol. 26. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. xvi + 491, with 232 figures and 49 tables.) 42*s*. net.

Problems in Electrical Engineering, with Answers. By S. Parker Smith, C.B.E., D.Sc., M.I.E.E., Assoc.M.Inst.C.E., Professor of Electrical Engineering, Royal Technical College, Glasgow. Fifth edition. London: Constable & Co., Ltd., 1948. (Pp. xviii + 378.) 10*s*. net.

Fundamentals of Vibration Study. By R. G. Manley, M.Sc., Research Department, Vickers-Armstrong Ltd. Second edition. London: Chapman & Hall, Ltd., 1948. (Pp. xvi + 156, with 53 figures and 12 tables.) 15*s*. net.

An Introduction to Photo-elastic Analysis. By Arnold W. Hendry, B.Sc. (Eng.), Ph.D., A.M.Inst.C.E., A.M.I.Struct.E., Lecturer in Engineering, University of Aberdeen. Blackie's "Technique" Series. London and Glasgow: Blackie & Son, Ltd., 1948. (Pp. viii + 152, with 77 figures, including 8 plates.) 7*s*. 6*d*. net.

Smith's General Chemistry for Colleges. By William F. Ehret, Professor of Chemistry, New York University. Sixth edition. U.S.A.: D. Appleton-Century Co., Inc.; London: G. Bell & Sons, Ltd., 1946. (Pp. xiv + 677, with 264 figures.) 20*s*. net.

Rhenium. By J. G. F. Druce, M.A., M.Sc.(Lond.), R.Nat.Dr.(Prague), F.R.I.C., F.C.S. Cambridge: at the University Press, 1948. (Pp. viii + 92.) 10*s*. 6*d*. net.

The Modern Metallurgy of Alloys. By R. H. Harrington, Research Laboratory, General Electric Company. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xii + 209, with 49 figures and 30 tables.) 21*s*. net.

Introduction to Chemical Thermodynamics. By Luke E. Steiner, Professor of Chemistry, Oberlin College. Second edition. International Chemical Series. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. xiv + 510, with 42 figures and 59 tables.) 36*s*. net.

Distillation and Rectification. By Dr. Emil Kirschbaum, Professor, Karlsruhe Institute of Technology. Translated by M. Wulfinghoff. Brooklyn: Chemical Publishing Co., Inc., 1948. (Pp. xiv + 426, with 236 figures and 5 graph-tables in pocket.) $10.00.

Dipole Moments. By R. J. W. Le Fèvre, D.Sc., Ph.D., F.R.I.C., F.A.C.I., Professor of Chemistry, University of Sydney. Second edition. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1948. (Pp. vi + 117, with 23 figures.) 5*s*. net.

Spectroscopy and Combustion Theory. By A. G. Gaydon, D.Sc., Warren Research Fellow of the Royal Society. Second edition. London: Chapman & Hall, Ltd., 1948. (Pp. xii + 242, with 4 plates, 13 figures and 7 tables.) 25*s*. net.

The Systematic Identification of Organic Compounds. By Ralph L. Shriner and Reynold C. Fuson. Third edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. x + 370.) 13*s*. 6*d*. net.

Fourier Technique in X-ray Organic Structure Analysis. By A. D. Booth, Ph.D., Fellow and Lecturer at Birkbeck College, London. Cambridge: at the University Press, 1948. (Pp. viii + 106, with 48 figures, including 5 plates.) 12*s*. 6*d*. net.

The Chemistry of High Polymers. By C. E. H. Bawn, Ph.D., University of Bristol. London: Butterworths Scientific Publications, Ltd., 1948. (Pp. x + 249, with 1 plate, 72 figures and 31 tables.) 17*s*. 6*d*. net.

Advances in Protein Chemistry. Vol. IV. Edited by M. L. Anson, Continental Foods, Inc., Hoboken, N.J., and John T. Edsall, Harvard Medical School, Boston, Mass. New York: Academic Press, Inc., 1948. (Pp. x + 575, with 111 figures and 39 tables.) $8.50.

Qualitative and Volumetric Analysis. By J. C. Giblin, B.Sc., A.R.I.C., Member of the Society of Public Analysts and other Analytical Chemists, Senior Chemistry Master, Royal Grammar School, Worcester. London, New York, Toronto: Longmans, Green & Co., 1948. (Pp. x + 171.) 6*s*.

Organic Reagents used in Gravimetric and Volumetric Analysis. By John F. Flagg, Ph.D., Research Laboratory, General Electric Company, Schenectady, New York. Chemical Analysis Series, Vol. IV. New York and London: Interscience Publishers, Inc., 1948. (Pp. xiv + 300, with 11 figures and 31 tables.) 36*s*. net.

Reagents for Qualitative Inorganic Analysis. Edited by P. E. Wenger and R. Duckert, Geneva. Second Report of the International Committee on New Analytical Reactions and Reagents of the International Union of Chemistry. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. (Pp. xxii + 379, with 57 microphotographs.) 39*s*. 6*d*. net.

The Chemical Warfare Service in World War II. A Report of Accomplishments. By the Chemical Corps Association, Washington, D.C. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. (Pp. 222, with 3 portraits and 58 figures.) 24*s*. net.

Petroleum Production. Vol. IV: Condensate Production and Cycling. By Park J. Jones, Consultant, Houston, Texas. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. (Pp. vi + 238, with 146 figures and 54 tables.) 30*s*. net.

Sequence in Layered Rocks. By Robert R. Shrock, Associate Professor of Geology, Massachusetts Institute of Technology. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. xiv + 507, with 397 figures.) 45*s.* net.

Road Aggregates: Their Uses and Testing. By Bernard H. Knight, D.Sc., Ph.D., M.Inst.C.E., F.S.I., M.I.Struct.E., A.M.T.P.I., Professor of Highway Engineering, University of the Witwatersrand, and Rena G. Knight, M.A., M.Sc., D.I.C. Second edition. The Roadmakers' Library, Vol. 3. London: Edward Arnold & Co., 1948. (Pp. xii + 259, with 95 figures and 28 tables.) 25*s.* net.

Soil Mechanics in Engineering Practice. By Karl Terzaghi, Professor of the Practice of Civil Engineering, Harvard University, and Ralph B. Peck, Research Professor of Soil Mechanics, University of Illinois. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xviii + 566, with 218 figures and 28 tables.) 33*s.* net.

Fundamentals of Soil Mechanics. By Donald W. Taylor, Associate Professor of Soil Mechanics, Massachusetts Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xii + 700, with 239 figures and 13 tables.) 36*s.* net.

The Effect of the Soil Foundation on the Road Surface. By D. J. MacLean, B.Sc., A.Inst.P. Department of Scientific and Industrial Research, Road Research Laboratory, Road Research Technical Paper No. 11. London: H.M. Stationery Office, 1948. (Pp. iv + 20, with 16 plates and 7 figures.) 1*s.* 3*d.* net.

Growth of Plants. Twenty Years' Research at Boyce Thompson Institute. By William Crocker, Managing Director, Boyce Thompson Institute for Plant Research, Inc., Yonkers, N.Y. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. (Pp. vi + 459, with 171 figures, including 7 coloured plates, and 48 tables.) 60*s.* net.

The Potato. By W. G. Burton, University of Cambridge and Department of Scientific and Industrial Research, Low Temperature Research Station. London: Chapman & Hall, Ltd., 1948. (Pp. xiv + 319, with 25 figures, including 15 plates, and 59 tables.) 25*s.* net.

Textbook of General Zoology. By Winterton C. Curtis, Professor Emeritus of Zoology, and Mary J. Guthrie, Professor of Zoology, University of Missouri. Fourth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. (Pp. xx + 794, with 524 figures.) 27*s.* net.

Animal Breeding. By Laurence M. Winters, Professor of Animal Husbandry in Charge of Research in Animal Breeding, University of Minnesota. Fourth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. viii + 404, with 155 figures and 58 tables.) 27*s.* net.

Animals Alive. By Austin H. Clark, F.R.G.S., Smithsonian Institution, United States National Museum. New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1948. (Pp. viii + 472, with 26 pages of figures.) 22*s.* net.

The Ways of Fishes. By Leonard P. Schultz, Ph.D., Curator of Fishes, Smithsonian Institution, United States National Museum, with Edith M. Stern. New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1948. (Pp. xii + 264, with 80 figures.) 22*s.* net.

Conseil Permanent International pour l'Exploration de la Mer, Annales Biologiques, Vol. III, 1946. Edited by H. Blegvad with the assistance of AA. J. C. Jensen. Copenhagen: Andr. Fred. Høst & Fils, 1948. (Pp. 115, with numerous figures and tables.) Kr. 12.–.

Rational Fishing of the Cod of the North Sea. Being the Buckland Lectures for 1939. By Michael Graham. London: Edward Arnold & Co., 1948. (Pp. 111, with frontispiece and 9 figures.) *5s.* net.

On the Periodicity of Salmon Reproduction in the Northern Baltic Area and its Causes. By T. H. Järvi. Conseil Permanent International pour l'Exploration de la Mer, Rapports et Procès-Verbaux des Réunions, Vol. CXIX. Copenhagen: Andr. Fred. Høst & Fils, 1948. (Pp. 131, with 34 figures and 32 tables.) Kr. 15.–.

Human Embryology and Morphology. By Sir Arthur Keith. Sixth edition. London: Edward Arnold & Co., 1948. (Pp. xii + 690, with 578 figures.) 40*s.* net.

A Laboratory Manual of Comparative Vertebrate Embryology. By Allyn J. Waterman, Professor of Biology, Williams College. New York: Henry Holt & Co.; London: Constable & Co., Ltd., 1948. (Pp. viii + 248, with 57 plates and 19 tables.) 21*s.* net.

Introduction to Genetics and Cytogenetics. By Herbert Parkes Riley, Head, Department of Botany, University of Kentucky. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xii + 596, with 154 figures and 26 tables.) 30*s.* net.

Human Ancestry from a Genetical Point of View. By R. Ruggles Gates, F.R.S., Emeritus Professor of Botany in the University of London, Research Fellow in Biology, Harvard University. Cambridge, Mass.: Harvard University Press; London: Oxford University Press, 1948. (Pp. xvi + 422, with 27 plates, 9 figures and 23 tables.) 42*s.* net.

Advances in Genetics. Vol. II. Edited by M. Demerec, Carnegie Institution, Cold Spring Harbor, N.Y. New York: Academic Press, Inc., 1948. (Pp. viii + 373, with 66 figures and 24 tables.) $7.50.

Feeding the Human Family. Science Plans for the World Larder. By F. Le Gros Clark, M.A. Sigma Introduction to Science, 11. London: Sigma Books, Ltd., 1948. (Pp. x + 125, with 8 plates, 14 figures and 16 tables.) 7*s.* 6*d.* net.

Practical Methods in Biochemistry. By Frederick C. Koch, Professor Emeritus of Biochemistry, and Martin E. Hanke, Associate Professor of Biochemistry, University of Chicago. Fifth edition. Baltimore: The Williams & Wilkins Co.; London: Baillière, Tindall & Cox, 1948. (Pp. x + 419, with 22 figures and 51 tables.) 16*s.* 6*d.* net.

The Biochemical Reactions of Chemical Warfare Agents. A Symposium held at the London School of Hygiene and Tropical Medicine on 13 December 1947. Organised and edited by R. T. Williams. Biochemical Society Symposia, No. 2. Cambridge: at the University Press, 1948. (Pp. vi + 73, with 9 figures and 16 tables.) 5*s.* net.

Radioactive Indicators. Their Application in Biochemistry, Animal Physiology, and Pathology. By George Hevesy. New York and London: Interscience Publishers, Inc., 1948. (Pp. xviii + 556, with 97 figures, 208 tables and chart in pocket.) 60*s.* net.

Reflections on the Philosophy of Sir Arthur Eddington. By A. D. Ritchie. The first Arthur Stanley Eddington Memorial Lecture, 4 November 1947. Cambridge: at the University Press, 1948. (Pp. x + 38.) 2*s.* net.

Darwin: Before and After. By Robert E. D. Clark, M.A., Ph.D. The Second Thoughts Library, No. 1. London: The Paternoster Press, 1948. (Pp. 192.) 6*s*. net.

Fontes Historiæ Botanicæ Rossicæ. By Vladimir C. Asmous, B.A., Harvard College Library. *Chronica Botanica*, Vol. 11, No. 2, pp. 87–118, with 11 illustrations, including 4 plates. Waltham, Mass.: The Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1947. $1.25.

The Royal Institution. By Thomas Martin. Second edition. Science in Britain Series. London: Longmans, Green & Co., for the British Council, 1948. (Pp. vi + 54, with 12 plates.) 1*s*. 6*d*. net.

The Merchant Ship. Design Past and Present. By G. S. Baker, O.B.E., D.Sc., late Superintendent Ship Division, National Physical Laboratory. London: Sigma Books, Ltd., 1948. (Pp. 159, with 16 plates and 17 figures.) 12*s*. 6*d*. net.

Report of the Royal Society Empire Scientific Conference, June–July 1946. Vols. I and II. London: The Royal Society, 1948. (Vol. I: pp. 828; Vol. II: pp. 707—both illustrated.) £2 2*s*. net the set.

The Presentation of Technical Information. By Reginald O. Kapp, B.Sc. (Eng.), M.I.E.E., Pender Professor of Electrical Engineering, University College, London. Based on four public lectures given at University College, London. London: Constable & Co., Ltd., 1948. (Pp. xii + 148.) 6*s*. net.

The Genius of Industrial Research. By D. H. Killeffer. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. (Pp. x + 263, with 7 figures.) 27*s*. net.

The Truth about the Stork. By Edward F. Griffith, M.R.C.S., L.R.C.P. London: H. K. Lewis & Co., Ltd., 1948. (Pp. 137, with 59 figures.) 6*s*. net.

New Fields of Psychiatry. By David M. Levy, M.D. New York: W. W. Norton & Co., Inc.; London: Chapman & Hall, Ltd., 1947. (Pp. 172.) 12*s*. 6*d*. net.

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SCIENCE PROGRESS

THE SOLVATION OF COLLOIDS *

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The interaction of colloids with their environment, usually water, is found to vary over wide ranges, inconsiderable in the suspensions and dispersions, becoming increasingly important as we pass through the hydrous oxides to the truly hydrophilic starches and proteins. In non-aqueous systems the polymers may be taken as representative of this lyophilic class when dispersed in a good plasticiser or dispersion medium.

It is well known that the precipitates obtained on the interaction of an aqueous solution of an aluminium or ferric salt with ammonia are gelatinous, and more or less rapidly undergo a process of ageing, becoming more compact and retaining less water when placed in a filter funnel. When precipitated by ammonium carbonate, rather than by ammonia, the precipitates do not age so rapidly and are more readily peptised by acids.

Clearly, there must be differences in construction in both gels and sols, dependent on their mode of formation.

Three different proposals have been advanced to interpret their structure.

The earliest advanced by Pauli envisaged the formation of a hydrated adsorption complex which could undergo superficial ionisation. Thus, if aluminium hydroxide were prepared from aluminium chloride, the complex could be written

$$[(Al_2O_3\omega.H_2O)_x(AlOCl)_y(AlO)_z]^{z+} + zCl'$$

a z valent positive colloidal ion.

Weiser, in his classical work on the hydrous oxides, adopts a more physical concept in which a hydrated adsorption complex is

* Being a lecture given at the Royal Institution on Thursday, November 25, 1948.

likewise formed, but a number of ionic species are present

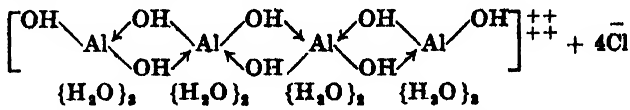
$$[(Al_2O_3.H_2O)_x(HCl)_y(H_2O)_z(\overset{+}{H})_a(\overset{+++}{Al})_b] + (a + 3b)Cl'$$

When we recollect that aluminium possesses a co-ordination number of six and is amphoteric, it is possible to regard these hydrous oxides as colloidal electrolytes. Thus, we can write aluminic acid, aluminium chloride and basic aluminium chloride as follows:

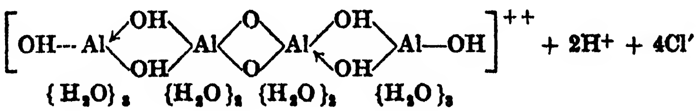
$$[Al(OH''')_6]\overset{+}{H}_3, \qquad [Al\{H_2\overset{+++}{O}\}_6]\overset{-}{Cl}_3, \qquad [\{H_2O\}_5\overset{++}{Al}(OH)]\overset{-}{Cl}_2$$

where ( ) represents a co-ordinate and { } a homopolar covalent link.

The monochloride $[\{H_2O\}_4Al(OH)_2]^+\overset{-}{Cl}$ can clearly condense to give a polymeric chain; thus the tetramer can be written



This process of condensation was termed " olation " by Bjerrum. Stiasny, who investigated these complexes in more detail, believed that further condensation could take place; thus the tetramer would go to



The hydroxyl groups held by co-ordinate linkage could be replaced by other anionic groups to yield polymers of different composition and greater stability.

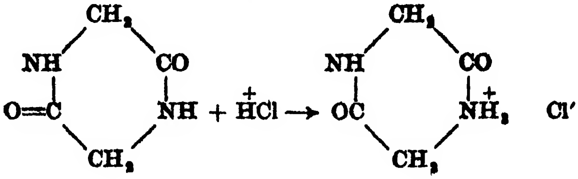
We can thus regard the " bound " water in these hydrous oxides as actually taking part in the chemical constitution of the polymer, serving both as a condensing agent and a homopolar covalent-bound constituent.

Solutions of the proteins and several other hydrophilic sols do not flocculate when brought to the isoelectric point, but it is well known that they can be flocculated in a variety of ways and are most susceptible at the isoelectric point.

Katz was the first to point out that, in the starches and proteins, surface charges produced by ionic adsorption or surface dissociation affected their stability not only by modification of their charge and electrokinetic potential, as in lyophobic suspensions, but also by affecting the amount of " bound " solvent, presumably by ionic polarisation.

This is well exemplified by the marked increase in solubility of

diketopiperazine in solutions of hydrogen chloride or the halides of lithium. The ionic form in solution may be written



Unequal ionic distribution between solute and water may thus affect the solution. We may recollect the peptising action of ions on the swelling temperature of starch solutions where the effectiveness of dispersion was found to be

$$\mathrm{CNS}' > \mathrm{I}' > \mathrm{Br}', \mathrm{NO}'_3 > \mathrm{Cl}' > \mathrm{HPO}''_4 > \mathrm{SO}''_4$$

which order is not only roughly their order of adsorption as determined by the electrocapillary curve, but actually their order of adsorption by starch, the SO''_4 ion not being adsorbed at all. These changes in solubility due to ionic adsorption are quite distinct from those due to ionic forces alone. Thus, the solubility of the globulins, *e.g.* hæmoglobin, is materially increased on the addition of any neutral salt. The change in solubility obeys the Brönsted-Bjerrum equation

$$\log_{10}\frac{\mathrm{S}}{\mathrm{S}_0} = -\log_{10} f \quad \frac{0{\cdot}5z^2\sqrt{c\nu^2}}{1 + 1{\cdot}5\sqrt{\Sigma c\nu^2}}$$

Where S is the solubility in presence of neutral salt of concentration c and mean valency ν and z is the mean valency of the protein.

We note that at the surface of a z valent colloidal particle of radius r the potential is $\frac{z}{r}$, the radial field of force $\frac{z}{r^2}$ and the divergence of the field $\frac{z}{r^3}$; the latter is responsible for the attraction of the water dipoles and must clearly be less for a colloid particle than for a smaller ion.

Katz extended this concept of ionic adsorption to all peptising and flocculating agents. According to this view, the degree of solvation can be affected by the introduction of hydrophilic or hydrophobic groups, and these can be introduced by forming covalent linkages with some unsaturated groups in the colloid particle.

He pointed out that the $>$ CO, and especially the $>$ CS linkage, were strongly adsorbed by starch, which provided a mechanism for

the introduction of polar and peptising groups, *e.g.* thio-urea, sodium thio-barbiturate, or non-polar groups, *e.g.* dimethyl thio-urea, or the well-known example of tannin.

The solvation and stability of the proteins can be modified not only by altering the degree of ionisation of the polar groups already present, or by the insertion of polar or non-polar groups by adsorption, but also by modifying the coulombic forces round the ions, *i.e.* by the addition of alcohols. Simple considerations indicate that the change in solubility should be related to the change in dielectric constant of the equation

$$\log \frac{S_1}{S_2} = K\left(\frac{D_2}{D_1}\right)^2$$

where S_1 and S_2 are the solubilities in media of dielectric constants D_1 and D_2 respectively.

Under suitable conditions progressive removal of the water of solvation may effect a phase separation in the form of liquid viscous drops, further dehydration leading to a flaking or granular flocculation. This intermediary viscous liquid form is termed a coacervate, and can readily be produced by adding sodium sulphate to a warm solution of gelatine, or by adding aliphatic alcohols to isoelectric albumen solution. De Jong's coacervates present many interesting biological analogies.

This concept of active hydrophilic groups forming part of the structure of macromolecules of the starches and proteins is naturally generally applicable to all polymeric systems where hydrophilic groups for particular solvents may be present. We are thus led to regard in solvent polymer interaction the relative magnitudes of the cohesive and adhesive forces, which may be designated as polymer-polymer and polymer-solvent interaction, as one of the important factors governing the solvation. There is another factor which is not quite so obvious, but equally important. The polymers in their solid state consist of partly orientated and partly interlocked macromolecules. The macromolecules themselves may be relatively rigid, as in cellulose, or relatively flexible as in indiarubber. It is evident that, in the assembly of matted molecules of the solid, parts of the system are free to move by thermal agitation if they are flexible. Introduction of a potential solvent into the solid by interaction may restrict these motions, and consequently even a good solvent will not penetrate unless there is a corresponding large heat effect. Furthermore, on dispersion of the matted structure in a good solvent the individual macromolecules are free to move and twist as they desire. This large change in entropy which we encounter in dealing

with polymers is a factor equally important to the thermal energy which we have considered.

We can evaluate these entropy changes quite readily from the free energy and heat changes involved. Thus, when a polymer has reacted with a limited amount of solvent to give a uniformly swollen polymer, the free energy change is given by the expression

$$\Delta F = RT \log \frac{P}{P_\alpha}$$

where P is the vapour pressure of the solvent above the swollen gel and P_α that above the solvent. Whilst the heat evolution can be evaluated from the term

$$\Delta H = \frac{RT_1T_2}{T_2 - T_1} \log \frac{P_{T_1}}{P_{T_2}}$$

where P_{T_1} and P_{T_2} are the vapour pressures at the respective temperatures T_1 and T_2.

The change in entropy is obtained from the expression

$$T\Delta S = \Delta H - \Delta F$$

If, for example, we examine the effect of water on various proteins, we find, in addition to the adsorption on the hydrophilic groups, that with some proteins there is a large positive entropy change, with others a large negative entropy change. Evidently, in the latter case the water must be firmly bound and the internal motions of the polymer scarcely affected by the presence of the water. Such is the case in wool. In the former case, evidently the polymer chains are being made more mobile, *i.e.* solution is taking place and the chains must be flexible. Albumen is a typical protein which reveals this effect.

These two important factors, the heat of mixing and the entropy changes due to flexibility, can, as shown by Flory, Huggins and other investigators, account for many of the interesting properties of gel formation swelling, phase separation and the like, which we come in contact with in polymer solvent interaction.

The quasi-thermodynamic expression governing these properties can be written

$$\log \frac{P}{P_\alpha} = \log (1 - V) + V(1 - X) + \mu V^2 \quad . \qquad . \quad (1)$$

where V is the volume fraction of the polymer, X the ratio mol volume solvent/mol volume polymer and μ the interaction.

If, as is usually the case, $1 - X \sim 1$ then

$$\log \frac{P}{P_a} = \log (1 - V) + V + \mu V^2$$

It has been found, to a first approximation, that the heat of mixing varies approximately as V^2, and we can write

$$\mu = f\Delta S + f\frac{\Delta H}{T} \quad . \quad . \quad . \quad . \quad (2)$$

If there be no heat of mixing, μ represents purely a change in the entropy. Small positive or negative values of μ indicate that the solvent is a good one for the particular polymer under consideration.

In the same way it can be shown that if the solvent merely swells the polymer, but does not permit it to go into solution, we have

$$\mu V^2 = - \log (1 - V) - V - \frac{\rho(1 - V)V^{1/3}}{MC} \quad . \quad . \quad (3)$$

where M is a parameter roughly equal to the molecular weight of the chain lengths lying between the links that hold the swollen polymer together.

Thus, the interaction constant μ is of prime importance in discussing the solvation of colloids, and it is pertinent to enquire how the value of μ may be determined. As we have already noted, it may be evaluated from the lowering of the vapour pressure, but the colligative properties of solutions are all connected together by a rigorous set of thermodynamic relationships. Hence, we might expect to obtain information from the osmotic pressures (π) of the solutions.

From (1) we obtain

$$- \Delta F = RT.XV + RT(\tfrac{1}{2} - \mu)V + \frac{RTV^3}{3} + \ldots$$

and since $\Delta F = \pi V$

$$\frac{\pi}{C} = \frac{RT}{M} + \frac{RT}{(1 - V)\rho}(\tfrac{1}{2} - \mu)C + \frac{RT}{3(1 - V)\rho}C^2$$

thus the value of μ can be determined from the slope of the customary $\frac{\pi}{C}$ C plots.

It is evident that if $\mu > \frac{1}{2}$ there will be a critical value of V which allows $P = P_a$, *i.e* the polymer will have a limited solubility and

the system will split up into two phases, a swollen gel and a dilute solution.

The entropy factor in solvation of these colloids can thus be the determining factor whether, on the progressive addition of a "solvent," the system continues as a one-phase system or breaks up at a certain point into two phases. It is quite possible that certain physicists have been led astray in postulating long range forces to account for the apparent swelling of certain proteins, *e.g.* virus, to a very large but limited extent. We see that the swollen virus water system may be in equilibrium with a very dilute virus solution, in which the virus molecules have many more degrees of freedom of movement than those in the swollen system, where certain movements, *e.g.* rotation, are hindered.

Many attempts have been made to obtain some idea of solvation of colloidal particles and polymeric molecules from considerations of viscosity. According to the simple Einstein equation, the viscosity of a suspension η should be dependent only on the volume of the disperse phase $\eta = \eta_0(1 + 2{\cdot}5\phi)$ and the viscosity of the dispersive medium.

If the dispersed particles are not spherical, then, since they are brought to some sort of order in the stream lines, and whether or no this order is disturbed by Brownian agitation, extra energy has to be expended, giving rise to an increase in viscosity. Many equations have been suggested to describe the effects due to such asymmetry in terms of the axial ratio f for a particle in the form of a hypothetical ellipsoid of revolution. One of the most satisfactory is

$$\eta = \eta_0(1 + 2{\cdot}5 + 0{\cdot}067f^2)\phi\frac{c}{100 - c} + 4{\cdot}4\phi^2\frac{C^2}{(100 - C)^2}$$

Again, if the particle possesses an electric charge, there will be an electro-viscous drag which again adds to the viscosity. The well-known Smoluchowski equation

$$\eta = \eta_0\{1 + 2{\cdot}5\phi\}\left\{1 + \frac{1}{\lambda\eta r^2}\left(\frac{D\zeta}{2\pi}\right)^2\right\}$$

where λ is the specific conductivity, ζ the electrokinetic potential, r the radius of the particle, and D the dielectric constant of the medium, has not been the subject of many experimental investigations. In actual fact, neither of these two modifications of the simple Einstein equation is sufficient to account for the observed viscous behaviour. We might note that in good solvents polymer-polymer contacts are very few. The molecules are thus more highly

dispersed and therefore the solutions should have a low viscosity, but if the polymers are flexible, then in good solvents the polymers will also be highly extended, and the asymmetry term in the viscosity curve becomes important and gives as a result a high viscosity concentration dependence. We note also that if the molecule be spiroform in its relative movement through the solvent, the liquid inside the spiral may either move as a whole with the spiral, or the spiral may move through the liquid, which then washes both inside and outside. The quantity of liquid which moves with a colloid polymeric molecule is thus dependent on its shape, the viscosity of the medium and the relative velocity of motion.

In the same way some of the polyion viscosity concentration curves exhibit a pronounced curvature suggesting that apart from the electro viscous effect due to the electrokinetic or ζ potential which we have referred to, there is a hydrodynamic unit involved carrying a large ionic atmosphere.

The addition of urea or guanidine to a solution of the protein myosin reduces the viscosity of the solution. Whilst we have seen from Katz's work that these substances are adsorbed by the protein and render it more hydrophilic, it is clear that the dispersing action may be so profound that the elongated molecules of the protein are actually broken down into smaller units. The primary extension of a coiled protein by the addition of such peptising agents causes, as we have seen, an increase in the viscosity of the solution.

Valuable information concerning the interaction of colloids with the surrounding solvent is to be gained from a study of what is termed "solubilising" actions. Soaps, detergents and salts of cholic acids (bile salts) belong to the class of colloidal electrolytes in that in solution the molecules are aggregated to form spherical micelles. In some high-melting-point forms these micelles are believed to acquire a plate or rod-like form. It has been found that the presence of such micellar forms readily increases the apparent solubility of a variety of substances. Closer investigation reveals the fact that the insoluble material may react with the surface molecules of the micelle to form complexes which are retained in the micelle. This process can be studied with great ease by examining the penetration of substances into monolayers thrown on to the surface of water in a Langmuir trough. It is found that those substances which are rendered more soluble by reaction with the surface molecules of the micelle penetrate into the monolayer. We can readily imagine that the complex must entail a reaction between the polar head of the film-forming material and the substance undergoing "solubilisation"; indeed, in most cases this appears to be

a hydrogen bond, but we must also observe that the hydrocarbon or non-polar portion of material undergoing solution dissolves in the hydrocarbon protein of the monolayer, thus increasing the stability of the monolayer. Such complexes may be very stable and quite recently those formed between proteins and fatty acids and other acid and basic detergents have excited a good deal of interest. In addition to this surface solubilisation, the micelle may dissolve in its interior some of the material undergoing solubilisation, *e.g.* azobenzene in a soap solution. It is clear, however, that a spherical micelle in which the interior consists only of the chains of the soap molecules from which it is formed cannot dissolve much hydrocarbon without affecting the spacing of the head groups which form the surface of the micelle. Nevertheless, such submicro emulsions are capable of being formed, as is revealed by the recent investigations of Schulman and Riley.

THE STRUCTURE OF LIQUIDS

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OF the three "states of aggregation" of matter the liquid state has undoubtedly the greatest significance for life on earth. For the vast majority of chemical reactions is tied up with this state because of the proximity and, at the same time, great mobility of the molecules, and for the same reason almost all biological processes take place in liquid media. This was indeed recognised more than 2500 years ago by one of the earliest scientific philosophers, Thales of Miletos, who taught that water was the essence of all existing things. It thus seems amazing that the study of the liquid state has been much neglected by modern physics in comparison with the study of gases and solids.

The usual concept of a liquid is based on its most conspicuous mechanical properties, namely the absence of rigidity combined with very small compressibility. Using this phenomenological concept alone, mathematical physicists were able to build up what is called "classical hydrodynamics," which makes no reference to the molecular constitution of the idealised liquid with which it is concerned, and which therefore nowadays is rightly considered to be a branch of "applied mathematics" rather than physics. But, whereas a tremendous amount of research work has been carried out on the kinetic theory of gases on the one hand and on the study of the structure of solids on the other hand, not much information on the molecular structure and the kinetic theory of liquids is to be found in the current textbooks, apart from certain properties of liquid solutions. Only comparatively recently has the study of the liquid state by up-to-date theoretical and experimental methods again been taken up. The aim of this article is to present to the reader a sketch of our present knowledge of this subject, at least as far as simple liquids are concerned.

To begin with, it is not easy to define properly what a liquid is. The first of the above-mentioned mechanical properties, namely the

absence of rigidity, is common to the liquid and to the gaseous state which are therefore commonly called "fluids"; and as a gas can be transformed into a liquid without discontinuity through the critical region a proper mechanical distinction between them is not possible. The second mechanical property, namely the small compressibility, is shared between the liquid and the solid state, and again, as real solids are never strictly rigid but yield under the influence of external forces, there is no clear-cut distinction between liquids and solids either. Moreover, a vast number of substances, in particular biological tissues, form "gels" which can assume all intermediate stages between solid and liquid; the same holds for so-called "amorphous" substances like glasses.

However, under ordinary conditions one will usually find no difficulty in recognising a simple liquid as such, and the question arises what characteristic feature of its molecular constitution distinguishes a liquid from a solid on the one hand and from a gas on the other hand. There can be no question that it must be something intermediate between them, and distinctly different from both. But opinions on the matter have frequently changed in the past.

The first great successes of the kinetic theory of matter were achieved in its application to "ideal gases" which are assemblies of particles supposed to exert no forces upon each other. By the introduction of intermolecular forces the theory was extended so as to account for the deviations of real gases from the ideal behaviour, and van der Waals succeeded for the first time in deriving an equation of state which apparently covered the gaseous and the liquid state and, at the same time, gave an explanation for the critical phenomena and the continuous transition from one state to the other. Thus it was accepted as a matter of course that the liquid state was not essentially different from the gaseous state, and that both could be dealt with by the same comparatively simple kinetic theory.

But this conclusion is open to grave criticism. Firstly, neither van der Waals' nor similar equations really describe the behaviour of dense and condensed gases more than qualitatively. Secondly, the implication of the theory that there exists a continuous range of states between the gas and the liquid is in contradiction to the normally exhibited discontinuous transition between the two states and their co-existence at the transition point. This is usually explained by the statement that a certain region of this continuous range represents "unstable" states. But in fact these have no physical significance at all.

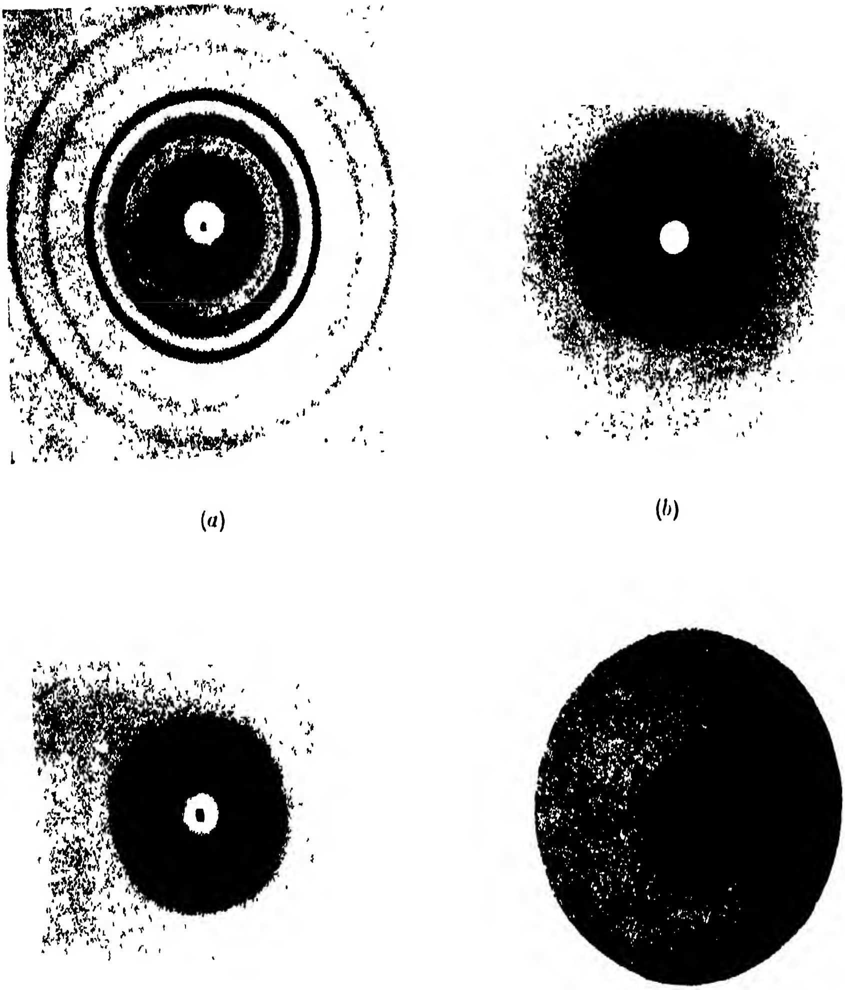
In order to describe quantitatively the behaviour of dense gases and vapours, one had to refine the theory considerably by taking into account the formation of "clusters" of molecules (Ornstein and Zernicke [1], Mayer [2], Born and Fuchs [3], Kahn and Uhlenbeck [4], etc.). It appears that these clusters are an essential feature of the structure of dense gases, and that the frequency of clusters of different size normally obeys a certain distribution law. But when the temperature is sufficiently lowered and the density sufficiently increased this distribution collapses and all the clusters coalesce into one huge cluster which is the phenomenon of condensation. This shows clearly that, apart from exceptional cases, there is no continuity between the structures of gases and liquids.

The application of statistical mechanics to solids followed the discovery of the lattice structure of crystals. An ideal crystal is an assembly of particles held together in a regular lattice arrangement by strong interaction forces; the thermal movement can therefore be considered to consist in small disturbances of a static equilibrium. This makes the task of applying kinetic theory to solids comparatively easy, and accordingly the kinetic theory of the solid state was developed by Grüneisen [5], Stern [6], Born [7], and others on this basis. Amongst other results definite equations of state for solid crystals or crystal aggregates have been obtained [8, 9, 10].

One might think that such an equation of state would show similar properties to van der Waals' equation and automatically cover both the solid and the liquid state. But this is not the case and cannot really be expected, as there is no critical point for the solidification of a liquid and no continuous transition from the liquid to the crystalline solid and vice versa. Also it is evident that a theory which is based on the assumption of a regular array of particles in a lattice cannot hold for the liquid state in which the particles are mobile. Indeed, it was shown by Herzfeld and Göppert-Mayer [11], Born [8] and others that, for a given pressure and above a certain temperature, the disturbance produced by the thermal movement will cause the static equilibrium of the particles in the lattice to break down. It seems plausible to identify this temperature with the melting point. But numerical comparison between theory and observation shows that the actual melting temperature of most substances is roughly only one-third of the "breakdown" temperature [10].

The explanation of this discrepancy is likely to be found in the fact that solids, even when they appear to be perfect crystals,

PLATE I



(*a*) (*b*) (*c*) (*d*)

FIG. 1.—(*a*) Powder photograph of corundum
(*b*) X-ray photograph of boric oxide glass.
(*c*) X-ray photograph of liquid sodium.
(*d*) Electron diffraction photograph of CCl_4 vapour.

(Reproduced from *The Diffraction of X-Rays and Electrons by Amorphous Solids, Liquids and Gases*, by J. T. RANDALL, by courtesy of the Author and the Publishers, Messrs. Chapman & Hall, Ltd.)

are in fact never completely regularly arranged systems of particles, but always show some kind of irregularities. A great deal of experimental evidence for this comes from the phenomena of plasticity and diffusion in solids, from electrical conductivity of semiconductors, from luminescence, etc. These "disorder" phenomena in solids point to a certain similarity in the structures of solids and liquids. Indeed, the fact that the change of density in melting amounts to only a few per cent., which leaves the distances of the particles practically unchanged, is already a strong indication for such a relationship.

On the other hand, there is also strong experimental evidence which leads one to assume that the arrangement of the particles in a liquid is far from being one of complete randomness, and that, at least near the solidification point, a certain amount of regularity must prevail in the liquid state. The most important of these experiments are those on the diffraction of X-rays and electrons in liquids [12, 13] where the pioneer work was done by Debye and Scherrer [14]. The corresponding diagrams show characteristic "halos" or diffuse rings produced by the scattering of the primary beam through certain definite angular regions, very little radiation being scattered through small angles (Fig. 1*c*, Plate I). There is a great similarity between these diagrams and those of amorphous solids (Fig. 1*b*, Plate I), and some resemblance to the powder diagrams of crystalline solids (Fig. 1*a*, Plate I) which, however, consist of sharp rings. On the other hand, the diagrams of gases and vapours show maximum scattering through small angles (Fig. 1*d*, Plate I), which is the outcome of the completely irregular arrangement of the molecules there. In many cases the similarity between the X-ray diagram of a liquid and the corresponding powder diagram of the solid near the melting point is striking [15] (Fig. 2). But in the critical region the liquid-type diagram is seen to go over gradually into the gas-type diagram [16] (Fig. 3).

Another argument for the assumption that the structures of liquids and solids near the transition point must be related to each other is furnished by the observation that a number of macroscopic physical properties change very slightly at the transition point or change continuously over a wide range on both sides of this point, for example the specific heat and the electric conductivity of metals, the dielectric constant of water and other polar substances, etc.

Some conflicting evidence, however, comes from experiments on the scattering of visible light in liquids, in the first place from the pioneer work of Raman [17] and also from that of many other investigators [18, 19, 20]. As the wave-length of visible light is

large compared with the atomic distances in solids and liquids, one can treat the phenomenon of scattering of light by these bodies as if they were continua. Thus no light should be scattered at all

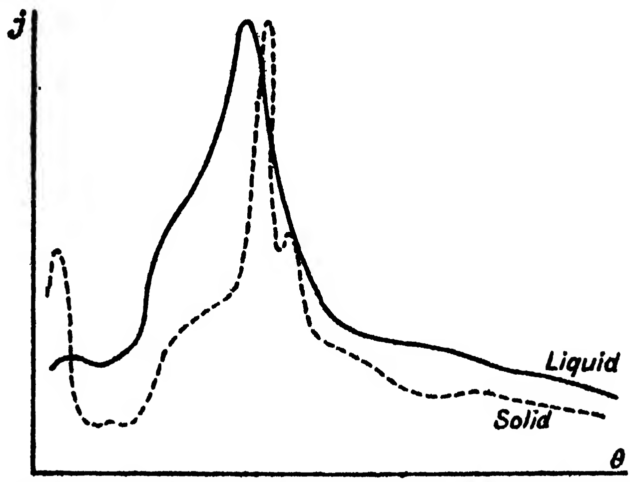


FIG. 2.—Comparison of the X-ray scattering curves of lauryl alcohol in the liquid state at 21° C. and in the solid state at 19° C. (after Stewart and Morrow).

unless the substance is optically inhomogeneous in domains of the order of magnitude of the wave-length of light. Such inhomogeneities can be produced by fluctuations in space of the value of the refractive index (or optical anisotropy) due to the thermal movement. Now, experiment shows that coherent light scattering owing to this effect is extremely small in solid crystals, but quite appreciable in liquids, being indeed of the same order of magnitude as in "amorphous" solids. It is much stronger still in gases and vapours (if related to the same number of molecules) and particularly conspicuous in the neighbourhood of the critical point (critical opalescence). This indicates clearly that the "graininess" of a liquid due to its thermal

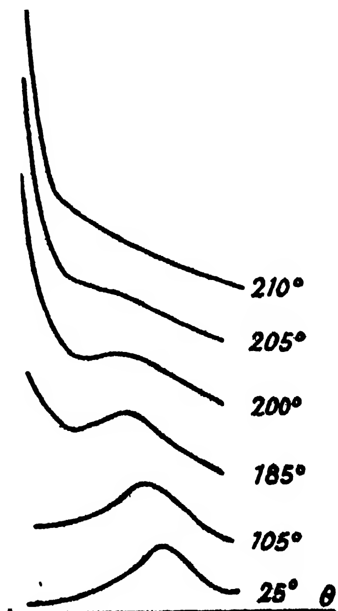


FIG. 3.—X-ray scattering curves of ethyl ether at a pressure of 44·1 kg./cm.² and various temperatures (after W. Noll and G. W. Stewart).

fluctuations is similar to that in gases, or, more precisely, that in a scale of the order of magnitude of the wave-lengths of visible light the deviations from uniformity are more or less irregular, just as in a gas.

One can resolve this apparent contradiction by assuming that the structure of a liquid has a regular "short-range order" in linear dimensions which are small compared with the wave-lengths of visible light, that is a few thousand atomic diameters, but no "long-range order" which is characteristic of a solid crystal. A rather radical suggestion on these lines was first made by Stewart [21] on account of his investigations on the scattering of X-rays by liquids. He assumed that a liquid consisted of individual groups of molecules within each of which the arrangement was the same as in a solid crystallite of the same substance, but with no discontinuous boundaries between the groups. He proposed the term "cybotactic groups" for these quasi-crystallites. From the breadth of the rings of the diagrams he concluded that a cybotactic group contained about 1000 molecules which would give them linear dimensions of about 10 atomic diameters; this conforms with the preceding considerations.

But it was later pointed out by Prins [22] that it was by no means necessary to assume the actual existence of cybotactic groups at all. For, if in an assembly of particles the thermal movement consists in irregular vibrations of the particles about *fixed* lattice points, the long-range order of the lattice will evidently not be affected by it and will only suffer a certain amount of blurring. But, if the centre of a vibrating particle is only defined with respect to the positions of its *next neighbours*, the accidental deviations of the centres of the surrounding particles from their ideal lattice sites will accumulate with increasing distance, until the long-range order is completely lost. This will happen when the mean deviation surpasses the average lattice distance. We thus obtain a structure where short-range order prevails within a certain neighbourhood of most of the particles, but where there is no long-range order, and yet no individual group formation of particles has occurred.

If this kind of structure is characteristic of a liquid, as was suggested by Prins [22], Bernal [23] and others, the previously mentioned experimental facts are explained satisfactorily: all those properties which are affected by long-range order or which depend on it entirely will change abruptly at the transition from solid to liquid, all those properties which depend on short-range order only will change but slightly at the transition point or gradually over a wide range.

In the course of the development of these ideas it was more or less generally accepted to regard a liquid as a kind of "disturbed" solid. Consequently numerous attempts were made to develop kinetic theories of the liquid state on similar lines to those worked out for the treatment of disturbances in solid crystals (Mott [24], Lennard-Jones [25], Wannier [26], Frenkel [27], etc.). One of these consists in considering the liquid as the limiting case of a polycrystalline solid; another in extending the statistical treatment of lattice imperfections which reduce long-range order in crystals to such an extent that it disappears altogether; still another in applying the "order-disorder" theory of Bragg and Williams, which was originally developed for the treatment of solid mixtures like alloys, to single-component liquids.

The result of all these theories is qualitatively the same: with increasing temperature, disorder increases continually in the solid until only a certain amount of short-range order is left which is the characteristic of the liquid state. This in itself would be incompatible with the existence of a sharply defined melting point, were it not for the fact that the states within a certain range of this continuous sequence would prove to be thermodynamically unstable. This leads to an explanation of the processes of melting and solidification on the same lines as in van der Waals' theory. In view of the much simplified assumptions of these theories one must, of course, not expect them to give more than a qualitative description of these phenomena. But they are open to the same objections which were raised before against van der Waals' theory, and even more so as the implied continuity between the crystalline and the liquid state cannot be realised at all owing to the absence of a critical point. This, of course, does not mean that these shortcomings could not be overcome by a refinement of the theory as already indicated.

Curiously enough, the "fashion" to regard the liquid as an imperfect solid, which was particularly advocated by Frenkel [28], has not remained long in favour with the physicists and is now giving way to a "new look" which, as so often is the case in fashion, is in fact a very "old look" indeed, namely to regard "fluids," that is gases and liquids, as essentially the same thing (Kirkwood [29], Born and Green [30], etc.).

Whatever the reasons for this change of opinion are, it must be admitted that there is no conclusive experimental evidence for the fundamental assumption of a "solid-like" structure of liquids, namely the assumption that the arrangement of the near neighbours of an arbitrary particle in a liquid is identical with that in the

corresponding crystal. As early as 1927 it had been pointed out by Zernicke and Prins [31] that the observed X-ray patterns of liquids could be explained by the mere assumption of the existence of a "radial distribution function." As the probability of finding a particle, in the absence of external forces, within a certain volume element, irrespective of the positions of the other particles, is constant in space, the "number density" ρ_0 of the assembly is uniform. But the probability of finding a particle at a certain distance from another particle (again irrespective of the positions of the rest of the particles) is not constant because of the existence of interaction forces. Thus, if it is assumed that these forces are central ones, the number of particles, on the average, to be found at a distance between r and $r + dr$ from any specified particle will be a function of r of the form

$$g(r)dr = 4\pi r^2\rho(r)dr \quad . \quad . \quad . \quad . \quad (1)$$

$g(r)$ is called "radial distribution function," and it was suggested by Zernicke and Prins that one might try to determine $\rho(r)$ direct from the experimentally obtained X-ray diffraction pattern.

The complete theory was worked out independently by Debye and Mencke [32] and by Warren and Gingrich [33, 13]. The main result is that the properly reduced intensity j of the radiation of wave-length λ scattered in a direction making an angle θ with the primary beam is essentially the "Fourier Transform" of the radial distribution function:

$$j(s) = 1 + \frac{4\pi}{s}\int_0^\infty (\rho - \rho_0)r \sin (sr)dr \quad . \quad . \quad (2)$$

with

$$s = \frac{4\pi \sin \theta/2}{\lambda}. \quad . \quad . \quad . \quad . \quad (3)$$

It follows from Fourier's well-known theorem that, vice versa, the radial distribution function is essentially the Fourier Transform of the measured angular distribution of the scattered X-ray intensity:

$$\rho - \rho_0 = \frac{1}{4\pi r^2}\int_0^\infty (j - 1)s \sin (sr)ds, \quad . \quad . \quad (4)$$

and that on theoretical grounds no more information than this can be obtained from such measurements about the structure of the scattering liquid.

Fig. 4 gives an example of the mutual relationship between j and g for a monatomic liquid. One sees that the characteristic feature of the function $\rho(r)$ is its marked periodicity with decreasing amplitude for increasing distance r, and the fact that it is always

zero in the vicinity of the origin which, of course, is due to the finite size of the particles.

This may be compared with the relation between the X-ray diffraction pattern of a crystal and the electronic density distribution in the lattice, which are the Fourier transforms of each other. Accordingly the structure of a crystal can be determined by means of X-ray analysis. But here again, owing to the fact that the phases of the scattered waves are not revealed in the diagram, only a limited amount of knowledge about the structure can be obtained without additional sources of information.

In the lattice structure of a solid element each atom is surrounded by others in successive shells, each shell containing a

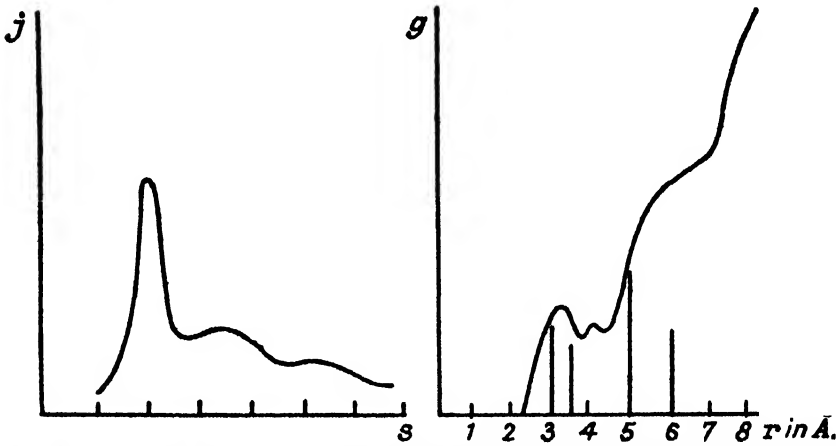


FIG. 4.—X-ray scattering curve $j(s)$ and radial distribution curve $g(r)$ for liquid lithium. The ideal crystalline distribution is indicated by vertical lines (after Gamertsfelder and Gingrich).

certain number of atoms in definite positions. The periodicity of the radial distribution function indicates a similar shell structure for the liquid state, but with less and less defined radii with increasing ordinal number, and with no definite positions of the atoms within the shells. It is very remarkable that for one and the same substance the average radii of the shells and the number of atoms within them, at least for the innermost shells, are nearly the same in the solid and in the liquid, which is indicated in Fig. 4 [13]. This is a much more precise statement regarding the similarity of short-range order between solid and liquid and the absence of long-range order in the latter, but it does not answer the question as to whether the arrangement of the nearest neighbours of a particle in a liquid is really crystal-like or nothing more than a density distribution.

So far we have seen that the assumption of the existence of a radial distribution function is completely sufficient for the explanation of the scattering of X-rays by liquids. But it can also be shown that it accounts for the scattering of visible light by liquids as well, for the radial density distribution of the particles is very closely related to the previously mentioned density fluctuations [34]. Denoting by $\delta\rho$ and $\delta\rho'$ the simultaneous deviations of the actual number density, ρ, from the average ρ_0 at two different points in the liquid a distance r apart, one can prove the relation

$$1 + \int^{\infty} (\rho - \rho_0) 4\pi r^2 dr = \frac{1}{\rho_0}\int_0^{\infty} \overline{(\delta\rho\delta\rho')} 4\pi r^2 dr \quad . \quad . \quad (5)$$

where $\overline{(\delta\rho\delta\rho')}$ is the mean density fluctuation product which measures the correlation between the density fluctuations at the two points. Now, one sees at once that the expression (2) for the intensity of the scattered radiation goes over into (5) for a sufficiently long wave-length, except for very small scattering angles. Further, in first approximation the function $\overline{(\delta\rho\delta\rho')}$, which is equal to $\overline{(\delta\rho)^2}$ for $r = 0$ and which decreases steadily with increasing r, can be approximated by making it equal to $\overline{(\delta\rho)^2}$ within a certain volume v and equal to zero outside. Thus the scattered light intensity becomes proportional to

$$j \propto \frac{\overline{(\delta\rho)^2}v}{\rho_0} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This brings us back to the notion of the "graininess" of a liquid in virtue of the density fluctuations, and it shows that the amount of scattered light is determined by the optical inhomogeneity due to this effect. The formula obtained is, by the way, in complete agreement with the theories of Rayleigh, Smoluchowski, Einstein and others [18, 19] for the scattering of light in "turbid" media. It also explains the previously mentioned "critical opalescence" in gases as $\overline{(\delta\rho)^2}$ becomes very large near the critical point as a result of the formation of large clusters.

A certain amount of correlation between the fluctuations at rather distant points in a liquid is, of course, not inconsistent with the preceding consideration. The existence of such a correlation is indeed confirmed by the phenomenon of a "fine structure" of scattered monochromatic radiation. Whereas the spectral lines of light scattered by a gas are merely broadened, in solids they are split up in a number of symmetrically displaced components, and in liquids they show displaced and undisplaced components [19]

(Fig. 5). The explanation of the displaced components in solids was first given by Brillouin [35] as being caused by the Doppler effect of light "reflected" by the system of thermal waves into which the thermal movement of the particles in a solid can be resolved. This clearly demands a considerable amount of correlation of the density fluctuations over distances large compared with the wave-length of the radiation. The complete lack of this correlation explains the diffuse broadening of the lines in gases. The observed fine structure of the lines in liquids, on the other hand, suggests a certain degree of correlation of the density fluctuations, which is to be expected from (5) on account of the similarity, previously explained, between the radial distribution function of a liquid and the atomic distribution in a solid.

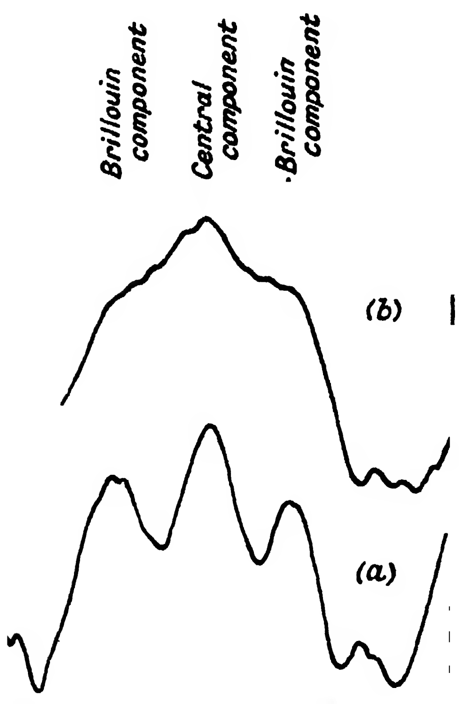


FIG. 5.—Microphotometric curves of the fine structure of monochromatic light of 4810 Å. scattered in acetone (*a*) at 28° C. and (*b*) at 54° C. (after Venkateswaran).

We can now return to the new theoretical treatment of "fluids," restricting ourselves to substances with central intermolecular forces where the potential energy $\phi(r)$ of a pair of particles depends on their mutual distance only. The fundamental assumption in the theories of Kirkwood [29] and Born and Green [30] is that there exists a whole sequence of probability functions n_h for finding h particles at definite positions, irrespective of the positions and velocities of the others, and that the whole mechanical and thermodynamical behaviour of the fluid is completely determined by them. Of these n_1 is the ordinary number density, $\frac{n_2}{n_1^2}$ is essentially the radial distribution function, etc. The most important static properties of the fluid depend on these first two exclusively. For example, the internal energy is given by the formula

$$U = \frac{3RT}{2} + \frac{\rho_0 V}{2}\int^{\infty} \phi(r)\rho(r)4\pi r^2 dr \quad . \quad . \quad . \quad (7)$$

and the equation of state by

$$P = \frac{RT}{V} - \frac{\rho_0}{6}\int_0^{\infty} \phi'(r)r\rho(r)4\pi r^2 dr \quad . \quad . \quad . \quad (8)$$

Thus it appears that all phenomena depending on these properties alone can be predicted when the structure has been determined by experiment and the intermolecular force law is known. Vice versa, the latter can be deduced from observational data on the internal energy and the equation of state, and from experiments on the scattering of radiation.

The more important and much more involved part of the theory is, however, the determination of the radial distribution function and the higher order distribution functions from first principles. If the fundamental equations which have been laid down by the above-mentioned authors could be solved rigorously, they should not only yield all the details of the structure of a liquid, but also give an explanation for the transition of that structure into a lattice structure at a certain point [36], and they should explain the similarity between those two structures. At present the theory is far from being able to cope with the formidable difficulties connected with the rigorous solution of the equations. Moreover, the theory is so far restricted to fluids with central forces between the particles, a condition which is strictly satisfied only in very few instances.

Now, if full advantage is to be taken of these rigorous theories for the precise theoretical treatment of a particular liquid, the potential function would first have to be known accurately, which is never the case. Thus, in spite of the very great theoretical progress made, when it comes to practical applications so many simplifications and approximations have to be used that the intricate apparatus of the theory is largely wasted. Therefore, the development of more primitive theories of the liquid state should not be neglected, starting with approximations right from the beginning by replacing the actual structure of the system by some simple kind of " model " structure, so that the treatment becomes mathematically more tractable. A number of such simplified theories exist already and are more or less successful within their limited spheres.

One of these models which is used by Zernicke and Prins [31], Bernal [23], Hildebrandt [37], Kirkwood and Boggs [38] and others, consists in regarding the particles as hard rigid spheres with no

interaction forces, as in the primitive kinetic theory of gases, but confined to a certain "free volume," namely the surplus of the "macroscopic" volume of the liquid over the total volume of the closely packed particles. But, whereas in a gas the free volume is large compared with the latter, it is just the other way round in a liquid. This free volume now appears as the only parameter which effectively replaces the complicated interaction of the molecules.

It was shown by Kirkwood [38], Frenkel [28] and others that a radial distribution function for such a system can be calculated from mere geometric probability considerations, and that for very small free volumes it has the character of a long-range order similar to that in solid crystals, simply as a consequence of the geometrical packing effect of the particles. Similar geometrical considerations form, by the way, the basis of V. M. Goldschmidt's theory of the crystal structure of elements and simple inorganic compounds. With increasing free volume, the long-range order is gradually replaced by a short-range order of the kind assumed by Prins, because of the increase of "blurring" with increasing distance caused by the accumulation of errors in the correct "packing" of the particles. This also explains at once the similarity in the "shell" structure of liquids and solid crystals which is revealed in the X-ray scattering experiments. It is also possible to obtain the distribution function empirically by the superposition of repeated photographic records of the fortuitous arrangement of balls in a limited space [39].

Once the radial distribution function for a given volume has been determined in one or the other of these ways, the corresponding pressure follows from equation (8), and one can then, vice versa, obtain the volume as a function of pressure and temperature, that is, the equation of state, and also, of course, the internal energy.

Another model theory starts from the opposite end, namely from the assumption that the forces between the particles are so strong that, as a rule, they always stick together, and that the close packing texture is only occasionally broken in the form of "holes." The notion of holes in the sense of unoccupied sites in a crystal lattice is very widely used. They are one sort of the previously mentioned imperfections which are responsible for the phenomenon of diffusion in solids, etc., and their existence is demanded by reason of statistical mechanics. The extension of this concept to liquids is due to Eyring [40] and his collaborators, who have used it extensively for the theoretical treatment of flow phenomena in liquids. A considerably more radical notion of holes in liquids was first intro-

duced by the author in collaboration with Ornstein and Milatz [41]; it consists essentially in replacing the real molecular structure of a liquid by a continuum with holes in it (like a Swiss cheese), and by replacing the actual intermolecular forces by the phenomenological surface tension at the surfaces of the holes.

The "hole theory" of the liquid state was extended later by the present author [42] and some others [43]. The holes are supposed to be formed by statistical fluctuations and they have therefore only a finite "lifetime." Thus they are the exact counterparts of the molecular "clusters" in dense gases. Just as the condensation of a vapour is due to the merging together of the clusters into one, the evaporation is due to the merging together of the holes into macroscopic vapour bubbles. On the other hand, the melting consists in the transformation of unoccupied lattice sites into holes. As the bulk of the liquid is supposed to have the same structure as the solid, the identical short-range order of the two states is explained at once; the lack of long-range order is the effect of the disruption of the texture by the holes. Certain differences in the thermal and mechanical behaviour of solids and liquids, for example the higher compressibility and thermal expansion of the latter, appear as immediate consequences of their "porosity."

The equation of state and internal energy of the liquid are obtained by deriving, by the methods of statistical mechanics, the size distribution of the holes for a given temperature and pressure. From this the contribution of the holes to the internal energy and to the volume follow easily, so that the total energy and the total volume can be determined as functions of temperature and pressure.

The fact that the two model theories mentioned, which represent approaches from opposite ends of the range of intermolecular forces, both give reasonably good agreement between experiment and theory leads one to expect that some less crude, but nevertheless a mathematically still fairly simple, general model theory of the liquid state may be forthcoming which will take proper account of the established features of the structure of liquids. Of course, special methods will always have to be used for dealing with liquids made up of very asymmetrically shaped molecules, or those in which the intermolecular forces are distinctly directional, or liquids with chain structure, or finally "crystalline liquids" (or "liquid crystals") in which a very considerable degree of long-range order can be established under certain circumstances.

The problem of the structure of liquids at very low temperatures may finally be indicated briefly. At very low temperatures the classical laws for the interaction of the particles are replaced by

quantum laws, and this ought to be reflected in their structure. There are, of course, very few substances which remain liquid at such temperatures at which the deviations from the classical laws become really significant. It is easy to see that only those substances will fulfil these conditions whose particles have a sufficiently large "zero point energy" and at the same time sufficiently weak interaction. Foremost amongst these is helium, and it has indeed been known for some time that there exist two modifications of liquid He, namely He I and He II with a sharp transition point between them. Of these, He II shows a number of very curious phenomena, the most conspicuous of which is its "superfluidity," that is the almost complete absence of viscosity.

One might think that, as the entropy becomes vanishingly small near the absolute zero point, the amount of disorder in the structure of such a liquid should be very much reduced, and consequently He II should have a quasi-crystalline structure with long-range order. This was indeed suggested, but it is not borne out by experiments on the scattering of X-rays, which show the typical ring pattern characteristic for an ordinary liquid with short-range order. On the other hand, one must not forget that at very low temperatures the thermal velocities of the atoms are so small that their de Broglie wave-lengths become large compared with their dimensions. This must have the effect that the atoms will not collide in the usual way, but may penetrate through each other to a certain degree like waves. Thus all "dissipation" phenomena which are governed by atomic collisions such as the viscosity, ought to be greatly reduced. These qualitative considerations [44] have been confirmed by the results of Born and Green's quantum mechanical extension of their kinetic theory of fluids [45].

The peculiar behaviour of the atoms in collision under these circumstances should also have some influence on the radial distribution function, for as the atoms can now partly penetrate each other $\rho(r)$ need not vanish within a certain sphere around $r = 0$. It ought to be possible to confirm this by a comparison between the X-ray diagrams of liquid He I and He II at the transition point, but the experiments so far carried out do not seem to have reached the necessary accuracy [46]. It is also not unlikely that the scattering of visible light could be affected by the difference in structure between the two modifications of liquid He. But here again the results so far achieved are not conclusive [46, 19].

To summarise, we can say that the structure of an ordinary liquid is characterised by a short-range order and the absence of a long-range order of its particles, and by the fact that the short-

range order bears a more or less close resemblance to the structure of the corresponding solid crystal, at least near the transition point. For the purpose of determining the static properties of such a liquid the structure is sufficiently defined by a radial distribution function, which can be determined directly from experiments on the scattering of radiation. For an approximate treatment the actual structure can be replaced by a model structure like the geometrical "free-volume" structure or the "hole" structure.

REFERENCES

1. L. S. Ornstein, *Proc. Amsterdam Acad.*, **19**, 1321, 1916.
   —— and F. Zernicke, *Proc. Amsterdam Acad.*, **19**, 1312, 1916.
2. J. E. Mayer, *J. Chem. Phys.*, **5**, 67, 74, 1937.
   —— and S. F. Harrison, *J. Chem. Phys.*, **6**, 87, 101, 1938.
3. M. Born and K. Fuchs, *Proc. Roy. Soc.*, **A166**, 391, 1938.
4. B. Kahn and G. E. Uhlenbeck, *Physica*, **5**, 399, 1938.
5. E. Grüneisen, *Ann. d. Phys.*, **39**, 257, 1912; *Handb. d. Phys.*, **10**, Berlin, 1926.
6. O. Stern, *Ann. d. Phys.*, **51**, 237, 1916.
7. M. Born, *Atomtheorie des festen Zustandes*, Leipzig, 1923.
   —— and M. Göppert-Mayer, *Handb. d. Phys.*, 2nd ed., **24**, No. 2, Berlin, 1933.
8. M. Born, *J. Chem. Phys.*, **7**, 591, 1939.
9. —— and M. Bradburn, *Proc. Camb. Phil. Soc.*, **39**, 140, 1943.
   M. Bradburn, *Proc. Camb. Phil. Soc.*, **39**, 113, 1943.
10. R. Fürth, *Proc. Roy. Soc.*, **A183**, 87, 1944.
11. K. Herzfeld and M. Göppert-Mayer, *Phys. Rev.*, **46**, 995, 1934.
12. J. T. Randall, *The Diffraction of X-rays and Electrons by Amorphous Solids, Liquids, and Gases*, London, 1934.
13. N. S. Gingrich, *Rev. Mod. Phys.*, **15**, 90, 1943.
14. P. Debye and P. Scherrer, *Phys. Zeitschr.*, **17**, 277, 1916.
15. G. W. Stewart and R. M. Morrow, *Phys. Rev.*, **30**, 232, 1927.
16. —— *Trans. Faraday Soc.*, **29**, 982, 1933.
17. C. V. Raman, *Molecular Diffraction of Light*, Calcutta, 1922; *Current Science*, **11**, 303, 1942.
18. J. Cabannes (in collaboration with Y. Rocard), *La Diffusion moléculaire de la Lumière*, Paris, 1929.
19. A. Rousset, *La Diffusion de la Lumière par les Molécules rigides*, Paris, 1947.
20. G. Oster, *Chem. Rev.*, **43**, 319, 1948.
21. G. W. Stewart, *Rev. Mod. Phys.*, **2**, 116, 1930.
22. J. A. Prins, *Naturwissensch.*, **19**, 435, 1931.
    —— and H. Petersen, *Physica*, **3**, 147, 1936.
23. J. D. Bernal, *Trans. Faraday Soc.*, **33**, 27, 1937.
24. N. F. Mott and R. W. Gurney, *Trans. Faraday Soc.*, **35**, 364, 1939; *Reports Progr. Phys.*, **5**, 1939.
25. J. E. Lennard-Jones and A. F. Devonshire, *Proc. Roy. Soc.*, **A169**, 317, 1939; **A170**, 464, 1939.
26. G. H. Wannier, *J. Chem. Phys.*, **7**, 810, 1939.

27. J. FRENKEL, *Trans. Faraday Soc.*, **33**, 58, 1937.
28. —— *Kinetic Theory of Liquids*, Oxford, 1946.
29. J. G. KIRKWOOD, *J. Chem. Phys.*, **3**, 300, 1935; **7**, 919, 1939; **14**, 180, 1946.
30. M. BORN and H. S. GREEN, *Proc. Roy. Soc.*, **A188**, 10, 1946.
H. S. GREEN, *Proc. Roy. Soc.*, **A189**, 103, 1946.
M. BORN and H. S. GREEN, *Nature*, **159**, 251, 1947.
31. F. ZERNICKE and J. PRINS, *Zeitschr. f. Phys.*, **41**, 184, 1927.
32. P. DEBYE and H. MENCKE, *Ergebn. tech. Röntgenkunde*, **2**, 1, 1931.
33. B. E. WARREN and N. S. GINGRICH, *Phys. Rev.*, **46**, 368, 1934.
34. P. DEBYE, *Zeitschr. f. Elektrochem.*, **45**, 174, 1939.
35. L. BRILLOUIN, *Ann. d. Phys.*, **17**, 88, 1922.
36. J. G. KIRKWOOD and E. MONROE, *J. Chem. Phys.*, **9**, 514, 1941.
37. J. H. HILDEBRANDT, *Proc. Phys. Soc.*, **56**, 221, 1944.
38. J. G. KIRKWOOD, and E. M. BOGGS, *J. Chem. Phys.*, **10**, 394, 1942.
39. W. E. MORRELL and J. H. HILDEBRANDT, *J. Chem. Phys.*, **4**, 224, 1936.
40. H. EYRING, *J. Chem. Phys.*, **7**, 391, 1939.
41. R. FÜRTH, L. S. ORNSTEIN, and J. M. W. MILATZ, *Proc. Amsterdam Acad.*, **42**, 107, 1939.
42. —— *Proc. Phys. Soc.*, **52**, 768, 1940; *Proc. Camb. Phil. Soc.*, **37**, 252, 276, 281, 1941.
43. F. C. AULUCK and D. S. KOTHARI, *Proc. Nat. Inst. Sci. India*, **10**, 397, 1944; *Proc. Camb. Phil. Soc.*, **41**, 180, 1945.
R. S. SILVER, *Nature*, **150**, 605, 1942.
44. R. FÜRTH, *Nature*, **159**, 738, 1947.
45. M. BORN and H. S. GREEN, *Proc. Roy. Soc.*, **A191**, 168, 1947; *Nature*, **159**, 738, 1947.
46. W. H. KEESOM, *Helium*, Amsterdam, 1942.

RECENT DEVELOPMENTS OF PALÆOBOTANY

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This account of recent work on fossil plants is to be regarded as a short anthology selected entirely according to the author's prejudices. It does not attempt to describe work too recent to have appeared in text-books, because excellent text-books have only just appeared. In fact, the selection has been all too easy, because palæobotany was going ahead rather slowly before the war and then altogether stopped in most countries. The works here selected are arranged roughly in order of the age of the rocks concerned, starting with the oldest.

The flora of the Middle Devonian is losing its position of the earliest well-represented land flora now that a number of plants of Upper Silurian and Lower Devonian age are gradually emerging into the light and, though still highly problematical, are now well enough known to be deeply interesting.

The finest of these early plants is *Prototaxites*, a plant with a misleading though valid name. It is widespread, rather abundant and has a long range, from Upper Silurian to Middle Devonian. Knowledge of it has come gradually from much patient work and aided by fortunate discoveries.

It was a large plant, for stems are known up to 1 m. thick, though the great majority are only a few centimetres. These smaller stems bore a number of great leaves which in isolation are called *Nematothallus*, though the terms leaf and stem here and elsewhere should perhaps be in inverted commas. As I have seen it, *Nematothallus* consists of isolated patches of slightly fibrous material, black in colour and singularly uninviting to study, and anyone meeting such a shapeless rag among the beautiful fossils of later periods would doubtless discard it. This has, however, been studied with devoted care, and, although we do not know how the big trunks were related to the rest, we do know already that it was a handsome plant.

Both stem and leaf consist of hypha-like tubes, apparently

without septa, but with thick walls and very often showing annular thickenings, thought by some to be the product of decay. No other type of cell enters into the construction, though there may be patches of smaller tubes occasionally simulating medullary rays in section, and the tissues as a whole show growth zones like annual rings. The leaf is made of similar tubes, which unite at the surface to form a compact layer like a cellular tissue, and the whole surface is covered by a well-developed cuticle which resists maceration and so, we may presume, was made of a material resembling cutin. When the leaf is macerated, considerable numbers of cutinised spores are found enclosed by the cuticles and lying among the tubes.

There is a conflict of evidence about the habitat of *Prototaxites*, for it is found in rocks with marine animals and also in rocks of purely freshwater origin, as for example in the Rhynie Chert. (This Middle Devonian occurrence is one of the latest in which it has been found.) Corsin, who firmly holds the view that it is marine and who considers that some small spherical bodies which he found at the base of the leaf are air floats, supposes that the terrestrial specimens are strays. Others suppose that the marine specimens have drifted from land. It is of course possible that it lived both in the sea and on land, but if so its habitat as well as its structure is anomalous.

The tubular construction recalls the siphonalean green alga, though the form of the plant is more as in *Laminaria*. It might have been what Church dreamt of when he wrote his celebrated essay the "Thallassiophyta"—a large seaweed transmigrant on to land. (As it happens, Church rejected *Nematothallus* as an alga, comparing it rather with the fungi, though of course at a time when knowledge of it was rudimentary.) Few, if any, are prepared to regard *Prototaxites* as an ancestor of anything now living on land; the cautious view is to regard it as a culmination of algal evolution which made a serious and rather long-sustained bid to occupy the land.

The other Lower Devonian and Upper Silurian land plants seem to give no lead to understanding *Prototaxites*. A common one often found with it is *Pachythea*, a little sphere of alga-like filaments. Others are tiny thalli or discs, apparently composed of parenchyma and provided with masses of cutinised spores; others again rather recall the Pteridophytes of the Middle Devonian.

One trouble with *Prototaxites* is that preservation is not good enough to make us feel quite happy about the facts outlined above, and alternative accounts are still possible. Forty years ago we would have cheerfully built a palace of theory on what we know

of *Prototaxites*, but now palæobotany is older and a little disillusioned. It is not that the basis is too small; it is not sufficiently *single* and we are not prepared to build publicly on a number of alternative sites.

I am sure the need is a search for more specimens of these early plants. Very few indeed have looked for plants in the Upper Silurian, so the quest remains hopeful. The discovery of a good series of fossils of this age might have a far-reaching effect on botanical theory, even to the extent of relating the Bryophytes and Pteridophytes to one another or to the Algae.

I now turn to the Middle Devonian. It may be recalled that for a very long time the status of such fossils as *Psilophyton* was doubtful; they were regarded almost with derision. And then came the discovery of the Rhynie flora, which provided some of the most completely known of all fossil plants, and demonstrated that they were land Pteridophytes. Since then the study of Devonian plants has been conducted at a standard which is probably higher than for any other period. Several floras have been described, one of the largest and most recently published being from Spitzbergen. These floras clearly indicate that plants like the Rhynie ones, or only a little more elaborate, were of very widespread occurrence, a matter which was once doubtful.

The Mid-Devonian plants have played a significant part in theoretical plant morphology, for many botanists are now prepared to regard nearly all Pteridophyte and even Gymnosperm construction in terms of the generalised *Rhynia* fertile shoot. It is even supposed that a plant very like *Rhynia* could be a common Pteridophyte ancestor, but here progress has been doubtful. There are certainly forms which seem to connect the Rhynia type with the fern type, and also connecting the Rhynia type with the Equisetalean type, but the Lycopods still stand out.

Aneurophyton is one of the largest of the *Rhynia* allies which had developed in the fern direction. It looked like a tree fern, though the stem which had compact secondary wood was not fern-like, but the "leaf"—a branch system in one plane and of rather limited growth—is very like the most divided fern leaves. Here we may report a step of progress, even though it is but the withdrawal of a false step. Some fossils, now regarded as belonging to this genus, were described from New York State under another name—*Eospermatopteris*. This of course often happens with little-known plants. In *Eospermatopteris* seed-like bodies were recognised among the leaves and it was hailed as the earliest seed plant. However, other workers examined these "seeds" by subjecting

them to oxidative maceration and obtained from them masses of cutinised spores. The "seeds" are just sporangia and the plant remains a—presumably—homosporous Pteridophyte.

It is a serious weakness of Palæobotany that one cannot distinguish between microspores (of a heterosporous plant) and the ordinary spores of a homosporous plant. They are merely shown to be microspores when the corresponding megaspore is discovered. Thus the homosporous status of *Aneurophyton* is always open to suspicion; true seeds might still be recognised! In this connection the recent discovery in *Archæopteris* is interesting. This is an Upper Devonian plant with somewhat similar fern-like leaves. The sporangia, which all look much alike, have been shown by maceration to have two sorts of spores, the big ones ten times the width of the small ones. This is the nearest so far that we get to the "seed" in the Upper Devonian plants, but it is hard to believe that the great evolutionary change from this moderate heterospory to the elaborate seeds of the Lower Carboniferous Pteridosperms should have occurred in such a relatively short time. However, there are still plenty of Upper Devonian fructifications which have not been subjected to maceration and it may well be that plants already known to us have much to tell on this subject.

There has been some real advance in our knowledge of the seeds of Lower Carboniferous plants. The student's definition of a seed which begins "an integumented megasporangium . . ." may be all right for its purpose in interpreting the series *Lycopodium*, *Selaginella*, *Pinus*, but seems to get into serious trouble with these fossils, and every significant word of it is open to objection.

The earliest seeds we know are also some of the most elaborate. There is a well-developed nucellus (megasporangium), often vascular at its base and sides, and elaborated to form a pollen chamber at its apex. It is surrounded by a highly organised integument, which is not only vascular, but forms a number of separate and equal finger-like lobes. There is a second sheath, the cupule, around the integument in the early Pteridosperms.

I will deal first with this cupule, selecting *Megatheca*, because it is the most beautiful of all, though it cannot be claimed to be the best known. This is the fossil which is called the "black tulip," and tulip-like it is in size and form, though the six lobes are much more substantial than tulip petals. We do not know what is inside the tulip, but Walton has found in a very similar petrified cupule, very likely the same species, considerable numbers of erect Gymnospermous seeds. The rest of the plant bearing *Megatheca* is unknown, but association of this fructification with the fern-like leaf *Telangium*

affine is suggestive, a plant whose anatomy is very likely represented by the very interesting petrifaction *Tetrastichia*. A moral of *Megatheca* is—here in what one supposes is one of the earliest of seeds the "cupule" is an elaborate and strongly developed organ enclosing more than one seed. Everyone is free to form his own view of the origin, but it is hard to dismiss it as an "enation," that is, a mere morphological nothing. It is even so substantial that Andrews, who described *Megatheca*, finds it hard to believe that each lobe represents a sterile branchlet—he feels it represents still more. A surprising feature about this radially symmetrical fossil is that the base shows a dorsiventral vascular strand, suggesting that it was borne, as Andrews thinks, on a dorsiventral leaf, or even, as Walton has suggested, replacing such a leaf.

I turn now to the early seed itself. Whatever the origin of the integument, it is most elaborate, and anyone wishing to regard it as a mere protective upgrowth around the megasporangium will find no help from these Pteridosperms, though, curiously enough, another seed-like fossil *Lepidocarpon* will give him his heart's desire. In modern seeds the integument is a tubular investment of several layers perhaps, but scarcely suggesting longitudinal division. In the Palæozoic seeds (*e.g.* *Lagenostoma* of the Upper Carboniferous), the integument is continuous below, but even there has separate bundles, but above it divides into distinct though concrescent chambers. In one of the best known of these oldest seeds, *Salpingostoma*, the upper part of the integument separates into half a dozen long fingers which together form the micropylar tube, enclosing a remarkably developed nucellar apex. Gordon, who described it, was much interested in its pollination mechanism, which must indeed have been highly specialised, and, though we may dismiss the length of the finger-like processes of the integument as the peculiarities of an individual genus, we cannot dismiss their existence.

A view of the integument only slightly modified from what Dr. Benson put forward in 1904 is now widely held. The seed is considered to represent a single fertile sporangium surrounded by a ring of other sporangia, diverted in development to form sterile protective structures collectively forming the integument. This hypothesis has received much indirect support in the last twenty years from the examination of Pterisdosperm microspore producing fructifications, though unfortunately the ones we know best are of Upper Carboniferous age. These fructifications rarely consist of individual sporangia, but instead a group united into a radially symmetrical body often looking like a bell-flower. In these bodies the sporangia are elongated and may be concrescent and in a single

ring, or more or less free and enclosing others. The whole thing looks so like a seed that it has often been mistaken for one.

It is difficult to see how such an idea could be proved, but the microsporangiate fructifications of the earlier Pteridosperms should, when better known, provide circumstantial evidence one way or the other. They should, of course, on this hypothesis, be even more seed-like than the later ones, but there must be a lingering suspicion that the later microsporangiate fructifications could themselves have evolved in the direction of a seed-like form. Palæobotany is not, unfortunately, in a position to throw full light on the megaspore of the seed. Until recently we were not conscious of anything needing much illumination, but then Thomson announced his discovery that in a large variety of seed plants the "megaspore" and the "microspore" attain about the same size at the moment when the first nuclear division occurs and each thereby ceases to be a spore. Consequently, he would drop the term "megaspore" for seeds and use another term such as "gynospore." He maintains that seed evolution never went through a *Selaginella*-like phase (this plant having true megaspores). Unfortunately no alternative theory has been developed, and as far as I am concerned I find myself unable to invent one.

Some suggestive facts are known, but they are ones which could be explained away. Several Carboniferous Pteridosperms have astonishingly large pollen grains, some 0·25 mm. wide. Then a recent piece of work on a large seed, *Pachytesta* (very like *Trigonocarpus*), provides both pollen grains in its pollen chamber, and also some eight tetrads of larger spores, over 0·5 mm. wide, which are regarded as non-functional megaspores. Of course we know nothing of the nuclear contents of these non-functional megaspores or whether they grew after the megaspore mother cells divided.

We may doubt whether Palæobotany can provide evidence that will settle this problem. What we need is to know, in *Lagenostoma*, for example, at what stage in the growth of the embryo sac did the nucleus divide? Nobody has yet seen a fossil seed preserved which anything approaches this in perfection, and anyway nearly all the seeds fossilised had attained a certain maturity.

I think we need not abandon the megaspore idea, if we invoke an additional hypothesis. We may suppose that cell division in the megaspore (as also for that matter in the microspore) tended to occur precociously—occurring before growth of the wall had been completed, and in fact occurring as soon as the young embryo sac grew to about double its volume. We can make a case for a rather closely analogous precocious development of sexuality occurring in

gametophytes (as for example in the Gnetales), and indeed plants seem appallingly casual about their young gametophytes—witness numerous flowering plants. There is scope for debate on the meaning of words.

It is a perplexing thing that, in the petrified Carboniferous seeds well enough preserved to show an embryo, there is none, but instead traces of the endosperm (female prothallus) and, in the best specimens, archegonia. Pollen is frequently met in the pollen chamber. It was suggested that, in these early seeds, pollination occurred while the seed was attached, but fertilisation was delayed till the seed was shed and was lying on the ground, and the whole embryo development occurred on the ground. There seems an approach here to the condition of the heterosporous pteridophytes, and some recent writers have urged that these bodies, though fairly called ovules, should not be termed seeds at all and that the Pteridosperms and Cordaitales should be removed from the seed plants.

This is another matter of verbal definition, but it is also a question based on very uncertain fact. Palæobotanists are so used to *not* finding things that they know existed, for example young seedlings, that few of them are willing to hang any considerable weight on negative evidence. The inference given above is a reasonable guess, but it is based on a very few species indeed, and is, moreover, not fully reliable for them. It is, for instance, possible that the great majority of fossil seeds are discarded abortions and not, properly speaking, ripe organs ready to function, for the one thing we know about them with certainty is that they did *not* function. It is not sufficient to dismiss such a hypothesis on the principle that nature is not so wasteful, for I think nature often is; witness, for example, the fate of 75 per cent. of the flowers of almost any dicotyledonous tree.

In the Upper Carboniferous, one of the most striking advances has been in the Cordaitean inflorescence. These Gymnospermous trees are well known to have produced compound catkins, the axis bearing little fertile buds; the question at issue was the nature of these fertile buds, whether they were simple, the microsporophylls (or the seeds) just taking their place in the sequence of budscales, or whether these reproductive organs were axillary, making the fertile bud itself an inflorescence. Re-examination of good petrified specimens of both male and female catkins has supplied a clear answer.

The fertile bud or "flower" arises in the axil of a bract. It has a short axis bearing spirally arranged appendages, most of which are sterile scales, but some of which are fertile "sporophylls." These

sporophylls simply replace a bract in the phyllotactic spiral. The microsporophyll is a stalk dividing at its apex to bear several pollen sacs. The megasporophyll is a stalk usually bearing one seed, but in other species branching repeatedly to bear several terminal seeds. A very interesting and rare thing has been recognised; a teratological specimen in which the "flower" is imperfectly formed, and the abortive seeds strongly suggest that they are composed of a central sporangium, the nucellus enclosed in two other sporangia forming the integument.

The oldest Conifers occur in the Upper Carboniferous along with the Cordaitales, but the group is much more apparent in the Permian, when many of the Carboniferous groups disappear. An outstanding advance has been made in the cones of these Palæozoic Conifers, one of those advances which, like the study of the Middle Devonian plants, is having an influence on the way people look on recent plants.

For a long time nearly everyone who studied conifer cones belonged to one of two schools. The one school looked on the female cone as essentially a simple fertile shoot composed of megasporophylls; the other regarded them as inflorescences with fertile axillary shoots. In each school there were several rival versions, but the main division was obvious and everyone concerned was clear that certain pieces of evidence were direct and conclusive, while that of the other side was just irrelevant.

Now there is new information which has, I think, swept all before it, and the idea that the cone is a reduced inflorescence is before us in an extreme form. The two chief genera of Palæozoic Conifers, *Lebachia* and *Ernestiodendron* (which are fragments of the old genus *Walchia*), have been made known in most of their organs, but, while nearly every part is like some modern Conifer, *e.g. Araucaria excelsa*, the female cone is extremely different, though differing in a way that can still be related.

In both, the female cone is large and rather loosely constructed, like a *Picea* cone. The woody cone axis bears 'bract scales' which are comparable with the leaves of the same plant. In the axils of these bracts there are unmistakable little fertile shoots, each consisting of an axis and several spirally arranged leaves, mostly sterile in *Lebachia*, where they again resemble the foliage, but all fertile in *Ernestiodendron*. The fertile leaf consists of a short stalk bearing an erect, or sometimes a tilted, terminal ovule. Even the apex of the fertile short-shoot has been recognised.

In the Permian and in the Mesozoic a considerable number of Conifer cones are known in some detail, and Florin, to whom we are

indebted for the information about the Palæozoic Conifers, has arranged the younger genera in a series leading in the direction of the modern Conifers. In this series there is reduction of all kinds, but in one line certain parts remain relatively conservative and developed, in other lines other parts. This reduction results in that simple-looking body the "ovuliferous scale," which is held to represent a shoot with sterile and fertile leaves all consolidated by adnation or other sorts of reduction, and sometimes adnate to its subtending bract also.

These Palæozoic female cones are remarkably close to the compound female inflorescences (catkins) of Cordaites, and this strengthens the idea that the Conifers and Cordaitales are allied. It is therefore disconcerting that the male cones differ greatly from those of the Cordaitales. They are in fact very like those of modern Conifers, *e.g. Pinus*, being simple shoots with strongly dorsiventral microsporophylls. The pollen, however, is closely similar to Cordaites, for it has a remarkably developed air sac, enveloping almost the whole grain. This discovery makes the "wings" of *Pinus* look venerable. We now no longer believe that the air sacs are organs of flight, but that they are of importance in making the grain buoyant in the micropylar fluid and so carrying it up to the nucellus (in an inverted seed.) The Conifer pollen wings can no longer be dismissed as dry blisters; they have an early origin from a larger sac, but what came before that is undisclosed.

While dealing with the Conifers we may turn to the remarkable genus *Taxus*. This genus and its immediate allies (*Torreya* and a few unfamiliar genera) first appear in the older Mesozoic rocks, where they are represented by fruit-bearing shoots and foliage remarkably like those of the living genera. The ovule which in *Taxus* is terminal on a small scaly shoot is equally terminal in these early Taxaceæ; there is not the least hint of an approach to any sort of cone, least of all to the elaborate cone of the Palæozoic Conifers.

Florin concludes that the Taxaceæ are very widely separated from all Conifers, having nothing to do with that group, or with the Cordaitales. The position is logical, though uncomfortable, for in everything but the way the ovule is borne *Taxus* is like the Conifers and its male cone is curiously near the male "bud" of *Cordaites*.

We leave unmentioned the other Palæozoic discoveries and turn to the Mesozoic. Here, work has been divided between the Pteridophytes and the Gymnosperms but progress has been made almost entirely with the Gymnosperms, because, astonishingly

enough, the Mesozoic Pteridophytes seem to have been rather well understood and progress shows that they are more and more what they were supposed to be. A shock to our complacency would be a good thing.

In the Gymnosperms there has been advance all round, and also, which may be regretted, a complication of classification. There was a time when the Mesozoic Gymnosperms were Cycads, Conifers and Ginkgos, but the Cycads split into two surprisingly distinct parts and four or five small independent groups emerged from the investigation of plants that had not been regarded as Gymnosperms at all. Gone for ever is the charming simplicity. On the other hand, not one of these Gymnosperms has indicated an origin of the Angiosperms, though we have had our moments of hope.

The Mesozoic ferns have played their part in showing which families now living are primitive. Were they and the older ferns unknown, we may be sure that, while some people would assert, for example, that *Gleichenia* and *Osmunda* are relatively primitive, another school would not be wanting to suggest the same role for *Polypodium*. We are spared that.

However, because we know very little about the fossil Polypodiaceæ, we there get no lead, and the confusion of varied schemes of classification results. There are plenty of late Mesozoic and Tertiary ferns awaiting careful investigation.

In the Gymnosperms there has been a steady but unspectacular improvement of knowledge of the existing orders. Nearly all this progress has been made by the study of the cuticle of leaves and reproductive organs—a layer which in the Gymnosperms is remarkably well developed. Thus in the Conifers we may note the fuller investigation of several kinds of cones which were already partly known. They fit neatly into the scheme of Conifer evolution, from the fully elaborate *Walchia* type to the reduced types of today. In the Ginkgos there has been a clearing up of many leaf genera (and I am afraid the creation of new ones). In the Bennettitales there has been elucidation of points in floral anatomy of known genera. The Cycads have been most spectacularly advanced. This group, once supposed to be the dominant Mesozoic family, faded from the picture as one after another "Cycad" proved to be a member of the Bennettitales, a very different group which converged on the Cycads in evolution. Now, however, some very common Mesozoic leaf genera have been shown to have Cycad reproductive organs of modern aspect. The Cycads return as a major Mesozoic group though not quite a dominant one. It would also appear that they are a group which completed their important evolutionary changes very

early and then, like Ginkgo, submitted to no further change, but only to death.

Of the new groups the Taxales (as distinct from all Conifers) has been mentioned already. The Caytoniales still remains an isolated genus. Here progress has been disappointing, for they are now realised to have Gymnospermous pollination, and they are therefore less like the Angiosperms than they originally seemed. The existence of "wings" on their pollen is interesting; presumably these grains, like those of Conifers, floated up the pollination drop; this would seem to require the young fruiting stalk to have stood erect.

Two new groups, founded by Thomas, appear to be richer in members. One, the Peltaspermaceæ, including *Lepidopteris*, may be Pteridosperms in the ordinary sense. The leaf and microsporophyll are both elaborately branched, as befits a Pteridosperm, and the megasporophyll is branched and bears a ring of seeds on a little disc. In view of what we now know of the many-seeded cupules of certain early Pteridosperms, this may be a conservative feature.

The other group, the Corystospermaceæ, is more puzzling. The leaves are rather small, but of distinctly Pteridosperm type. The "sporophylls" are both small—a very few cm. long, and somewhat branched. The microsporophyll would appear to be simply a very reduced form of the *Crossotheca* or *Lepidopteris* type.

The megasporophyll, however, is most difficult. Some of the specimens look like very reduced but branched Pteridosperm megasporophylls, with a few very small pinnules. Others, however, look more like little racemose inflorescences with small bracts, bractioles and the branches as axillary shoots. The specimens are, however, all of a group and must be taken together. Thomas, who thinks a vigorous stir is good for morphology, considers them at the same time as sporophylls and as bracteate inflorescences. This is a subject which might be simplified by more knowledge.

The latest new group is the Pentoxyleæ, an isolated genus from the Indian Jurassic. This should soon, if it is not already, be much the best known of all the new groups, since it is preserved as petrifactions as well as ordinary compressed fossils. The leaf which is called *Taeniopteris spatulata* has been known a long time, being one of the commonest Indian Jurassic fossils. Like the leaves of the three previous groups (Caytoniales, Peltaspermaceæ, Corystospermaceæ) it was formerly regarded, though without much conviction, as a sterile fern leaf. (There are still several of these unconvincing "fern" leaves left for other people to raise into new plant types, but the number grows less.) The newly recognised

parts are the stem and female organs. The stems, or at least the small ones known, are woody twigs (*Pentoxylon*). There is a ring of about five mesarch primary bundles around the pith, on to which dense, Conifer-like secondary wood is built. The female organs are a bunch of short, spike-like bodies, each consisting of a central axis thickly covered by sessile seeds. The seeds have apical micropyles and a remarkably fleshy outer testa.

I may remark that the evidence on which the plant is put together, though not complete, appears convincing. We still are without knowledge of the male organs, and there is no doubt much more to be learnt about those which have been mentioned.

This queer family, or rather plant, cannot be classified with any of the families previously recognised, unless we stretch it specially to receive it. On the whole the stretch needed to the definition of the Bennettitales might seem least, but no known Bennettitalean plant has this sort of stem structure, and none shows a gynæcium of crowded, sessile seeds without interseminal scales. Even the leaf would be somewhat anomalous, for, though various Bennettitales have "*Taeniopteris*" leaves, the cuticle is somewhat different, and indeed unlike that of any other fossil Gymnosperm. It is, however, more possible to regard it as a modification of the usual Bennettitalean type than of any other Gymnosperm type. It will be most interesting to get further information about this plant; the material seems far from exhausted.

Little enough progress has been made with the pre-Cretaceous history of the Angiosperms. The one new development has been the discovery, in the few Jurassic rocks examined, of pollen grains of the Angiosperm type, some of them with three germ furrows. Such pollen is unknown in living or fossil Gymnosperms and, until it is shown to occur in some other group as well, is to be taken as strong evidence of the existence at these early periods of Angiosperms, more or less as we know them, and in some abundance, though presumably not in the deltaic floras which give the known fossil plants.

Little can be usefully written about the Cretaceous and Tertiary floras, not because no progress has been made—it has, particularly in America—but for other reasons. The progress has been largely related to the development of whole floras, controlled chiefly perhaps by major climatic changes. It is difficult to summarise this work without giving a series of name catalogues. One may doubt the validity of identifying dicotyledon leaf genera by their shape and venation, and of seed genera by their shape and surface markings.

It is therefore only right to say that those who have studied

this material believe in it, and what is more the work of different investigators, certainly not in collusion, does in the main hang together and that in an impressive way, as when a seed is determined and a leaf is determined by someone else, and then it is realised they are always closely associated. One must conclude that it really is true that a very great part of the evolution of the Dicotyledons, at least of the woody plants that provide our fossils, was almost completed in early Tertiary times. Since then they have been as conservative as *Ginkgo* !

Palæobotany still has not given a clear lead on Angiosperm evolution. When we get it we shall at last see the end of these varied schemes of classification ; there must be half a dozen used or advocated today ! When that happens systematists will still incur disapproval by their nomenclatoral changes and their endless splitting of species, genera and families, but one major nuisance will end.

REFERENCES

The following text-books give references to most of the original work mentioned here :

An Introduction to Paleobotany, C. A. Arnold, McGraw Hill Book Co., 1947.

Ancient Plants, H. N. Andrews, Comstock Publishing Co., Ithaca, N.Y., 1947.

For *Pentoxylon*, see *Botanical Gazette*, Part I, 1948.

SCIENCE AND PROFESSOR BERNAL

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In 1939 a book,* obviously of great importance, was sent to me for review by the Editors of Science Progress, but before I was able to comply with the request the war began and Science Progress suspended publication. I had by then read the book with some care and formulated the general lines of the review I intended to write; and in the hope that the interruption of normal activities would not be unduly long I put aside for later use the notes I had made. As everyone knows, the interruption was not only long but catastrophic, and indeed of such a character that a review in the ordinary sense was put out of the question. The book is a remarkably comprehensive conspectus of a situation which has ceased to exist. It in part considers eventualities on which history has now made its own pronouncements, and to judge its forecasts in the light of knowledge unavailable when they were made would be neither just nor profitable. On the face of the matter the proper course would be to accept with regret the fact that what would otherwise have been a most instructive survey had had the misfortune to appear at a most inopportune time, and let comment be stillborn.

Nevertheless, there are reasons why such a course would in fact be very improper. In the first place, the book itself is anything but stillborn. Despite the abnormally long coma with which it began its life, the nurses have succeeded in bringing it to consciousness, and it is now a lusty and active infant announcing its existence from bookshop and advertisement in tones that cannot fail to attract attention. Such an appeal demands a response. Moreover, the significance of the book by no means lies wholly in the picture which it presents of the social activity of science: it resides in a comparable degree also in the standpoint from which the picture is drawn, and Professor Bernal has shown clearly enough in the post-war years that that standpoint is one which he has not felt called upon to abandon. The question of its appropriateness is no less

* *The Social Function of Science*, by J. D. Bernal (Routledge, 1939).

important than that of the accuracy of the survey. But perhaps the most compelling—certainly the most attractive—reason for analysing this untimely survival from the days that are no more is that it provides the solution to one of the most fascinating enigmas of the modern scientific world, namely the mentality of Professor Bernal himself.

In the years immediately preceding the war, those who took pleasure in trying to infer from the writings of representative men the types of mind from which they proceeded found fewer subjects more challenging than Professor Bernal. On the one hand, his work in crystallography and other of his purely scientific activities (as a more recent example, his admirable lecture on "Waves and Beaches" at the Brighton meeting of the British Association in 1948 may be helpfully cited) gave evidence of a quality of mind, a power of balanced judgment, and a command of the widest fields of scientific knowledge and thought that were conspicuous even among the leaders of our time. On the other hand, his apparent ignorance and irresponsibility when writing of the relation of science to other departments of thought and action—qualities such that it were safer not to try to find adjectives for them—seemed to testify unmistakably in the opposite sense. One need only quote his remark that "any process, once set going by an initial impulse, continues in the absence of external forces until, passing its equilibrium position as the result of its own momentum, it is brought to a stop and reversed",* although by that time Newton's first law of motion had undoubtedly become known at Cambridge where he was then working: and his reply to a critic, who was unwilling to adopt Communism because of an unsoundness in its principles, that "the plain answer to this is to get Communism first, and argue about it afterwards".* By what miracle had grapes and thorns been made to grow on the same bush?

The Social Function of Science provides, as I said, the answer to this question. It is clear that there is a fundamental confusion, an unresolved and unrealised inconsistency, at the root of Professor Bernal's thinking that prompts him here to utter the most flagrant contradictions in rapid succession, and there to see a simple situation in a wildly distorted state and to pronounce on it an almost incredibly irrelevant judgment. The clearest expression of this basic incompatibility is to be found in his treatment of the word "science", and it is doubtless because one cannot write of the social function of science without revealing what the word "science"

* *Aspects of Dialectical Materialism* (Watts & Co., 1934), pp. 104 and 121 respectively.

means to or conceals from him that the present book gives the clue to his mental attitude.

How would any person with an orderly rational mind approach the task of writing a treatise on the social function of science? He would see that the phrase implied that there was some recognisable phenomenon called "science" that could be said to have a social function. He would therefore begin by identifying this phenomenon and delineating as clearly as possible what it was in itself, apart from any function it might have or any relation in which it might stand to other phenomena; and he would then proceed to consider the part it played, or could play, in social life. And there is, in fact, nothing—not one single thing—that Professor Bernal wants to say that could not be expressed in terms of such a treatment, and expressed usually much more intelligibly and (so far as it is true) convincingly than it is in fact expressed in the book. Let us see, however, how he goes to work.

He begins (pp. 3–6) rightly enough by considering what science is (adding, however, "or ought to be", and so confusing the issue at the start) and cites two "sharply distinct points of view" which he describes (again introducing emotive words which can have no clarifying but only a persuasive effect) as "the ideal and the realist pictures of science". We will retain these names as labels for the sake of brevity, without, however, implying that the former view is not realistic or the latter not unreal. "In the first picture science appears as concerned only with the discovery and contemplation of truth; its function, as distinct from that of mythical cosmologies, is to build up a world picture that fits the facts of experience". "In the second picture utility predominates; truth appears as a means for useful action and can be tested only by such action". According to this view science is "the means of obtaining practical mastery over nature through understanding it". Views intermediate between the two are admitted.

There is no specific statement as to which of these views Professor Bernal adopts, so we start and remain in a state of complete uncertainty about what he is going to mean by "science" when he uses the word. It is not, in fact, possible to make sense of later statements unless we hold ourselves ready to change at short notice from one meaning to another, and this cannot be justified by supposing that a catholic view is adopted which includes both extremes because they are presented as irreconcilables. We are made perfectly aware, however, of the view which Professor Bernal *prefers* by the manner of his discussion of them. The "realist" view receives nothing but respect, the "ideal" view little but adverse

criticism. Take the following passage (pp. 5–6), for example, which is most instructive in helping us to see the basic confusion to which I have referred: "In spite of the fact that this [*i.e.* the 'ideal' view] is a view held by many scientists themselves, it is essentially self-contradictory. If the contemplation of the universe for its own sake were the function of science, then science as we know it now would never have existed, for the most elementary reading of the history of science shows that both the drive which led to scientific discoveries and the means by which those discoveries were made were material needs and material instruments".

Now what are we to make of this? We are asking what science is, and the "ideal" view is held to be self-contradictory because it is inconsistent with the history of science. But what is the history of science? If science is the contemplation of the universe then the history of science is the history of the contemplation of the universe, and contradiction is impossible. If science is the mastery of nature, then the history of science is the history of the mastery of nature, and again contradiction is impossible. To find a contradiction you have to introduce it yourself, and that is just what Professor Bernal does. He makes science mean one thing in one part of the sentence and another thing in a later part. There was a man who didn't like spinach; and he was glad that he didn't like it, because if he liked it he would eat it, and he didn't like it. That is the type of confusion that permeates the whole of the book because it is a part of the foundation of it.

That is not all that this interesting passage reveals. When we compare it with the natural, straightforward treatment of the subject which I have indicated above we see that not only is the meaning of the word "science" essentially ambiguous, but also the meaning of the word is inextricably confused with the *function* of science. "If", the passage runs, "the contemplation of the universe for its own sake were the function of science. . . ." But the view supposed to be under discussion is that science *is* "the discovery and contemplation of truth". Its "function", according to this view, has been described as "to build up a world picture that fits the facts of experience", but I doubt if anyone who holds this view of science would limit its function so drastically. Its function is also to satisfy curiosity, to purge the emotions, to improve man's estate, and a thousand other things, all of which can be achieved by means of the discovery and contemplation of truth but none of which is a necessary consequence of it. Instead of giving us first a clear idea of science, and then a discussion of that particular function of it which is called "social", Professor Bernal gives us an

essentially confused picture in which it is impossible to distinguish science from its function or to know what either is intended to include: "function is smothered in surmise and nothing is but what is not".

We shall be told, of course, that according to the Marxian dialectic it is impossible to separate a thing from its function because things can be defined only in terms of their functions, but this is no answer to the criticism. In the first place, it is simply a fact that there is such a thing as the discovery and contemplation of truth,* quite apart from any use we may make of such discovery and contemplation and any respect or derision we may bestow on it, and it is a thing to which even Professor Bernal needs to refer more than once in his book. It should therefore have a name, and if that name is not science then it should be something else; in either case the thing has a definition independent of its function. And indeed, Professor Bernal tacitly acknowledges this, not only in his title (which otherwise would be *Science—a Social Function*) but also conspicuously at the beginning of Chapter V, where we read: "Once we admit any function for science in society it is possible to ask whether that function is being carried out efficiently or inefficiently. . . . Our judgment of the degree of inefficiency of science will, however, largely depend on our ideas as to what the function of science is. Nevertheless, without prejudging that issue, which is the central problem of this book, it is still possible to talk of the efficiency of scientific research in relation to the different hypothetical functions of science". If this does not imply that science and its functions are separate concepts it is utterly meaningless.

We start, then, with an ambiguous thing called science which is different from, but inextricably confused with, its functions. Let us see to what consequences this inauspicious beginning has committed us. I do not, let me say, claim to have unravelled all the confusion in which the fundamental notions are involved; the sources of some of the passages which I shall quote will, I fear, remain as obscure to others as they are to me. It is, however, of some help to have established that there is a large amount of confusion in the premisses,

* I am not concerned here, of course, with philosophical problems concerning the nature of truth and the question whether what we do in science is to discover objective truth or construct a rational system to correlate experience. Whatever our views on these questions may be, we can agree that the process indicated by the phrase, "the discovery and contemplation of truth", is a recognisable fact of history and experience, exemplified, for instance, by Newton's well-known remark about the ocean of undiscovered truth lying before him.

for we are then less ready to cast undue suspicion on the printer than we might otherwise be. I do not know, for instance, what particular meaning the word "science" has in the statement (pp. 246–7), "As science advances it leaves the explanation of the more remote and obscure parts of life—the heavens or the mysteries of chemistry—and becomes more and more able to cope with domestic activities which have been the first to be carried out but the last to be understood." Fellows of the Royal Astronomical Society and the Chemical Society may not have noticed this. They have perhaps not become aware that their studies are either languishing or becoming unscientific. I cannot help them, beyond suggesting that perhaps they have not yet grasped the meaning of the word "science" in its protean plenitude.

On the other hand, the "realist" view of science emerges triumphant from the following remarks (p. 76) about university lectures: "A scientific lecture lies between two extremes. It may be an inspired and generalised commentary on the subject, intended to arouse interest and stimulate thought by dwelling on the present limitations rather than on the established status of knowledge and by bringing science into close relation with technical and social problems. . . . The opposite extreme is the conscientious lecture in which all the points, particularly the numerical results and mathematical arguments required to establish them, are carefully and methodically expounded . . . it is clear that in this case the purpose of the lecture would be much better fulfilled by the presenting of a typewritten sheet". But look at the way in which the contrast is drawn. The second example is clearly intended to arouse our contempt, but in fact all we are told about it is that it is characterised by conscientiousness, care and method. Are these bad things, and is the first type of lecture free from them? And might not the second type of lecture also be "an inspired and generalised commentary on the subject" and "arouse interest and stimulate thought"? And why is so little said and so much implied about the subjects of the lectures? The second is assumed to contain "numerical results and mathematical arguments", but not the first. Yet suppose the first had to be concerned with, say, the recession of the nebulæ or the solution of differential equations of the second order. Would it then "arouse interest and stimulate thought by dwelling on the present limitations rather than on the established status of knowledge and by bringing science into close relation with technical and social problems"? The whole passage is clearly not a rational statement at all but the expression of a prejudice; apart from its emotive effect it has no meaning whatever unless one is

prepared to change the meaning of words whenever it suits his ulterior purpose to do so.

This liberty is indeed perhaps the best single clue we can give to the interpretation of the book. In the contest with words, the question, Who is to be master? is to be settled in Professor Bernal's favour. His determination that this shall be so helps us to understand why, in a discussion of science since the first world war, particularly in Germany, we can read on p. 32, "The natural inefficiency of scientists was made worse rather than better by the development of bureaucracy; science was neither free to develop according to its own intrinsic tendencies nor was it effectively directed in the service of industry"; and on p. 34, in support of the general proposition that "economic factors determine the actual course of science", that "both directly and indirectly, the great revolution in chemistry [in the eighteenth century] was the product of economic forces". It appears, therefore, that when "bureaucracy" is to be castigated, science is something which needs to develop "according to its own intrinsic tendencies", but that in fact its development is "the product of economic forces".

There is here a suggestion of a subsidiary rule of interpretation which receives some support elsewhere; namely, that when the treatment of science by bureaucracy, or capitalism, or fascism (the words are apparently interchangeable, no occasion having arisen for making them contradict one another) is in question, science conforms to the "ideal" definition, but in other contexts the "realist" view is most likely to be implied. For instance, on p. 211 we read, "Fascism simply drives to their logical conclusion the tendencies of economic and intellectual nationalism, already observable in all capitalist countries. The first duty of the scientist ceases to be the discovery of truth or the well-being of mankind and becomes the service of his nation in peace and war"; and on p. 212, with regard to Nazi science, "Even more than this perhaps is the destruction of the spirit of German science, the appreciation of a patient and exact determination of the structure of the world, the belief in the intrinsic value of pure scientific truth". This is excellent: and again on p. 221, "Capitalism in its later stages is incapable of bearing objective examination; the scientist becomes necessarily a critic and criticism cannot be tolerated. The scientist must therefore hold his tongue or lose his place." In view of recent occurrences in the Soviet Union, this is arresting. Have we been mistaken about that country? May it not perhaps be a capitalist State at a late stage of development?

If this were an examination paper I would set the following questions:

(1) Explain clearly the meaning of the word "science" implied in each of the following statements:

(*a*) "Many mechanical engineers, and still more electrical and chemical engineers, are necessarily in part scientists, but their work on the whole cannot be classified as scientific research as it mostly consists of translating into practical and economic terms already established scientific results". (p. 55.)

(*b*) "What can be understood rationally . . . must be simple; but it will not be worth understanding unless it is also useful. It was consequently only at the relatively late stage of civilised city life that mathematics, mechanics, and astronomy, the simplest of sciences, began to appear". (p. 127.)

(*c*) "Except for a certain portion of the nineteenth century, it may fairly be claimed that the majority of significant technical and scientific advances owe their origin directly to military or naval requirements". (p. 165.)

(*d*) "Throughout all past history science has been considered to be above the conflict" [i.e. war]. (p. 183.)

(*e*) "The aim of education as far as science is concerned is to see that everyone not only has a general picture of the world in terms of modern knowledge, but also appreciates and can use the type of argument on which that knowledge is based. The particular contribution of science to this is the creation of quantitative reasoning and the understanding of how phenomena can be brought about by different causes, each contributing to a definite degree". (p. 248.)

(*f*) "After considering these more general aspects of scientific advance we can pass to its immediate concrete prospects. These can be looked at in two ways, from the point of view of the development of scientific technique and theory and from the point of view of the satisfaction of human needs". (p. 331.)

(*g*) "The application of science to chemistry will lead to the replacement of these by low-temperature electro-chemical, catalytic, or enzyme reactions". (p. 372.)

(*h*) "The scientist is still, though less often and less markedly than in previous times, a person of definite psychological abnormality. He is driven to satisfy his curiosity for its own sake, and, in order to be free to do this, he is willing to fit into any kind of life which will offer the least mental and material disturbance to what is his main concern. Besides this science

itself is an eminently satisfactory occupation; its pursuit withdraws interest from external things, and offers as well a means of solace and escape for those who find the events of the outside world distressing. The great bulk of scientists are therefore, as long as their science is not threatened, likely to be the most docile and amenable of citizens". (p. 389.)

(*i*) "The really positive part of science, the making of discoveries, lies outside scientific method proper, which is concerned with preparing the ground for them and with establishing their reliability. . . . Another aspect of the same defect of present-day science is its inability adequately to deal with phenomena in which novelty occurs and which are not readily reduced to any quantitative mathematical description". (p. 411.)

(*j*) "The task which the scientists have undertaken—the understanding and control of nature and of man himself—is merely the conscious expression of the task of human society". (p. 415.)

(2) (For advanced students only.) Give a definition of science in which all the above implied meanings are reconciled.

Assuming that the reader has passed on the first question and failed on the second, let us proceed from the direct evidence of the fundamental confusion itself to some of its effects. We shall not be able to trace in detail how each particular effect has issued from a particular anomaly in the premisses, but knowing that rank confusion is there, and finding palpably absurd products, we shall be justified in inferring that a connecting link must exist. Here, for instance, is a pretty piece of reasoning from one of the acutest minds of our time.

On p. 181 we are told that out of the total expenditure in 1936–37 by the Department of Scientific and Industrial Research of £460,000, "more than a third can be credited to war and is fairly closely connected with possible war uses". This is made up of £105,000 devoted to the National Physical Laboratory "in which three of the most important departments, metallurgy, aero-dynamics, and radio, have a more or less direct war importance"; £22,000 on fuel research, which is "largely concerned with hydrogenation and the production of motor fuels from coal, also of direct war importance"; and £38,000 on "food investigation, mostly concerned with methods of preservation". It seems, then, that anything that can be used for military purposes can be said to have been produced for military purposes. And what is military research? "Two characteristics separate such research from all the rest of science. It is con-

sciously directed towards a social end, that is, towards the most rapid, effective and terrifying means of death and destruction, and, secondly, it is carried on under conditions of extreme secrecy ". We will not pause to notice that on this occasion no scientific research except military research has a conscious social purpose, because of the greater interest of the implication that research on food preservation, for example, is actually consciously directed towards the most rapid, effective and terrifying means of death and destruction. We have no passion for canned lunches, it is true, but we had not realised that the College Refectory Committee was quite so devilish. But the argument is irresistible. Food preservation is useful in war; war is a terrifying means of destruction; therefore money spent on food preservation is consciously directed towards creating terrifying means of destruction.

A single sentence often reveals a profoundly anomalous attitude and clarifies much that would otherwise remain obscure. Here is an example. " It is extremely difficult to estimate the extent of the damage done to the progress of science by the organizational inefficiency of which we have spoken ". (p. 120). Yes: we have heard this kind of statement before. Salt is what makes potatoes taste nasty when you don't put it in; pins have saved millions of lives by not being swallowed; and so on. Professor Bernal has forgotten that elsewhere he has (rightly) conceived of science as something which has a history, something which has grown up from a state of pure ignorance by gradual acquisition of knowledge. He sees it now only as a sort of Platonic ideal—science as it would be in a completely Marxianised system, organised down to the last concept and the ultimate unit. He sees it not as a process but as a fulfilment, and he measures its present state not by the extent of the ground it has covered but by the amount by which it falls short of " perfection ". Every supposed defect of organisation is a positive thing imposed on it from without and inflicting " damage ", instead of the absence of something not yet achieved, which might help in its advancement. That is why he is able to proceed: " To put it in figures, the average efficiency cannot be much greater than 50 per cent. and may be as little as 10 per cent.". How, in any other terms, can we give meaning to a percentage efficiency of " the progress of science " ? If one week and £10 are spent in teaching 50 boys the meaning of specific heat, what is the efficiency of the process ?

One of the most striking aberrations is brought to light in the following anecdote (p. 84): " Independence of spirit is not at a premium in the scientific world. The young research worker who

remarked, when asked his views on scientific collaboration by an eminent professor at a selection committee, that he did not intend to be anybody's lackey, did not get the post, and it was years before his undoubted gifts and character began to win him any recognition, while far less able but more pliable contemporaries were already sitting in professorial chairs ".

Now how would any intelligent, unbiased person view this situation ? He would see that a candidate for a post requiring at least some power of consecutive thought, when asked a certain question, gave a totally irrelevant answer. What could a responsible selection committee, whether composed of eminent professors or of any twelve good men and true chosen at random—what could such a committee do but reject the candidate ? If an examinee in anthropology were asked to describe the main differences between Negroes and Chinamen, and he replied proudly, " I am an Englishman ! ", would he be entitled to full marks, however greatly to his credit the fact might be ? If we asked our election candidate for his views on the function of a Second Chamber and he replied, " My great-uncle was footman to the Earl of Tooting ", should we feel that his claim to our vote was established ? But once grant that Professor Bernal's outlook is so perverted by a fundamental defect of the organs of vision that he cannot see even the simplest situation as it actually is, and the whole thing becomes plain.

Finally (for want of space, not material), what are we to make of this comment (p. 89) on popular science : " Subjects like relativity or the origin of the universe, which are intrinsically difficult, are found eminently suitable, not for technical explanation but for providing texts on man's helplessness and ignorance and on the beneficence and intelligence of the Creator. At the same time much more significant and practically important theories of the day, such as the quantum theory, receive scant attention ".

We need not enquire whether this assertion is in any sense true or not, but we cannot help asking the prior question, what does it mean to say that quantum theory is " more significant " than relativity ? Can it mean that quantum theory will last longer ? Hardly, because one of the great advances in quantum theory was achieved when the wave equation was made relativistically invariant, so that, although relativity might conceivably outlast quantum theory, the reverse is scarcely possible. Let us look at our definitions of science. On the " ideal " view there can be no question of relative significance ; truth is truth in each degree. On the " realist " view also, although it may be held that the more significant theory is the one that leads to the greater mastery over

nature, it would appear impossible to say which that is. Do we owe the atomic bomb, for instance, more to the quantum theory of the atom or to the relativistic equivalence of mass and energy? The question seems foolish. What, then, can this relative significance be? I can think of only one way of finding a meaning in it. If we assume that science, whatever it is, must be something that conforms to the Marxian dialectic, then the situation becomes clear. A theory of discontinuity can evidently be more readily shaped to the pattern than one of continuity. We have, too, the contradiction of wave and particle—has not Professor Bernal himself elsewhere cited this as exemplifying the dialectic process? It is true that Bohr tries to make out that there is no contradiction here but only a "complementarity", whatever that may mean, but that is obviously only a bourgeois attempt to resist the logic of the actual world process. Yes, here indeed is an explanation, and I would be perfectly satisfied with it but for one alternative possibility—namely, that the remark does not mean anything at all. And on the whole I am inclined to think that that is most likely to be the truth.

As I have said, this is not a review, in which every important aspect of the book would receive due attention. Nevertheless, I cannot conclude without expressing admiration of the masterly manner in which Professor Bernal has brought within the field of his vision such an enormous mass of material and organised it into a coherent whole. Is it essentially a noble mind that is here o'erthrown. Is it too much to hope that he may be induced to clear his mind of cant, and make an effort to see things as they are and not as a fanciful speculation requires them to be? If only he would do this we might indeed look forward to reading his future work with pleasure instead of inspissated gloom.

ELECTROSTATIC GENERATORS

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Ever since it was shown that the nuclei of atoms could be broken down by bombarding them with fast charged particles, physicists have sought methods of accelerating ions to high energies in order to increase their efficiency as projectiles in experimental work on atomic disintegration. Many different types of particle accelerator have been devised in an effort to satisfy this demand. The electrostatic generator was one of the earliest of these instruments and, in spite of the subsequent invention of the cyclotron and other orbital accelerators, with which much higher particle energies can be attained, the electrostatic generator has undergone continued development and has proved to be a most suitable instrument for accelerating either electrons or positive ions to energies of a few million electron volts.

The principle on which an electrostatic generator works differs little from that underlying some of the nineteenth-century " influence " machines, but the credit for recognising the potentialities of this type of machine and for giving it its modern form belongs to van de Graaff [1] and his collaborators, who built the first large generator in a disused airship hangar at Round Hill, North America, in the early 1930's. In spite of many practical difficulties (the relative humidity on the site was 84 per cent. during the summer months, thus making the insulation problems very formidable), these workers succeeded in showing that the electrostatic generator was a very practical instrument for nuclear research. The Round Hill generator had two spherical high-voltage terminals, independently supported on separate insulating tubes, with provision for charging the one terminal positively and the other negatively with respect to earth. These terminals were each 15 feet in diameter and stretched between them was the evacuated tube in which the ions were accelerated. The space inside each high-voltage terminal was large enough to accommodate a small laboratory. With this machine, a voltage of 5 million volts between terminals was achieved

and this voltage has been exceeded by very few electrostatic generators, even today. Progress in design, however, has reduced the size of the apparatus to such an extent that a complete 2 million volt electrostatic generator of modern design could be accommodated inside one of the high-voltage terminals of the Round Hill machine.

Fig. 1 illustrates the essential features of an electrostatic generator. Electric charge is carried to an insulated high voltage terminal by a moving belt made from a non-conducting material. As the charge on the terminal increases, its potential with respect to its

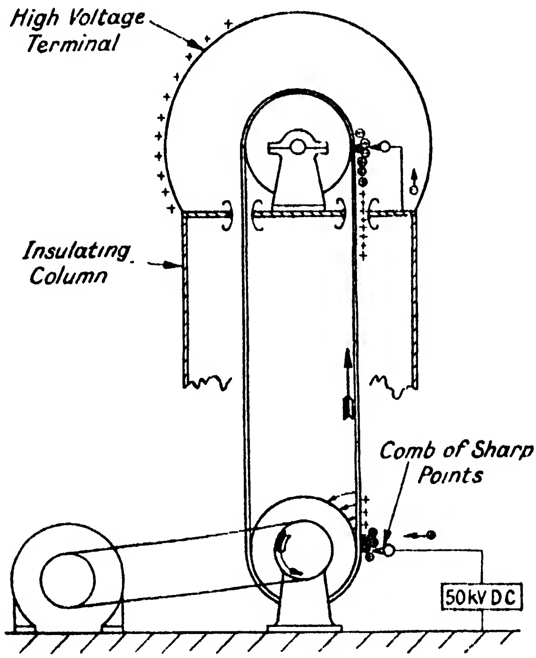
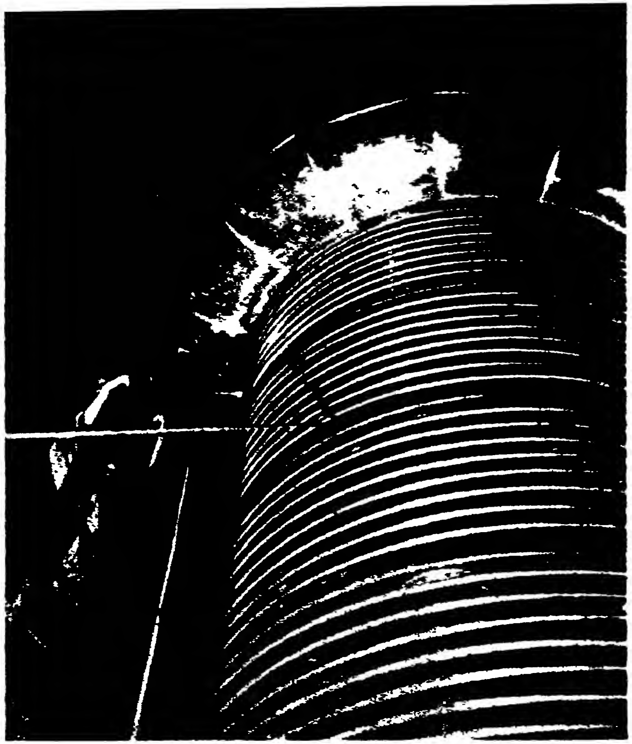


FIG 1

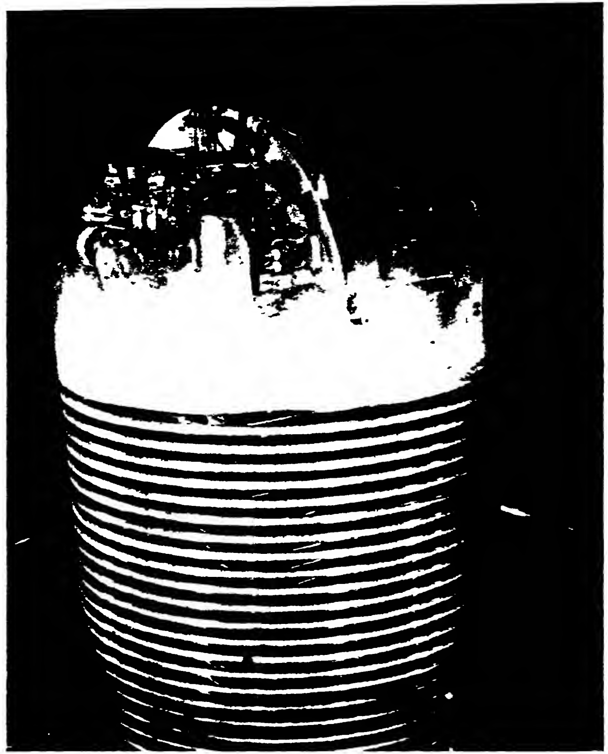
surroundings rises, and if there were no loss of charge by insulation leakage, corona current or load current, the potential would continue to rise until a spark passed, thus discharging the terminal. In practice, charge is drained away from the high-voltage terminal in all these three ways and a stable condition may therefore be reached in which charge is conveyed to the top terminal by the belt at the same rate as it is drained away: the potential of the top terminal will then remain constant. The belt surface is charged electrically as it passes through a region in which there is a stream of ionised air molecules due to a " corona " discharge. This corona discharge is produced between an insulated comb of sharp needle points facing

the belt and an earthed metal surface behind it. The belt pulley often serves as the earthed metal surface. The comb is connected to a high potential DC supply and the air molecules ionised in the high electric field around the needle points are carried along the lines of force towards the earthed metal. Many of them reach the belt surface and, being held there by induction, are carried upward by the movement of the belt. A similar comb of needles inside the high-voltage terminal and connected to it sprays charge of the opposite sign on to the belt, thus neutralising its surface charge and conveying an equivalent charge to the terminal. In the absence of any current drain, the voltage attained by the terminal is limited only by the insulation provided. The latter may fail by sparks passing along the supporting insulators, along the belt surface, or directly from the high-voltage terminal to earth. The current which can be carried on the belt is limited, as explained in a later paragraph, by the permissible charge density and by the width and velocity of the belt.

The Round Hill generator and some other early generators were "free air" machines, that is to say, they depended for their insulation on air at normal atmospheric pressure, and had to be housed in very large rooms to obtain sufficient distance between the high voltage terminal and the walls. It was soon realised, however, that if the whole machine were constructed inside a tank capable of withstanding a high gas pressure the necessary insulating gap could be kept much smaller and the size of machine for any given voltage correspondingly reduced. Additional advantages of this "pressure-insulated" design were that the gas contained in the tank could be dried efficiently and kept free from dust, while gases other than air could be used if their properties proved particularly suitable. Most modern electrostatic generators use compressed gas insulation, but the free air generator still has the advantage of greater accessibility for adjustments and repairs—a feature which is of considerable importance when a positive-ion source and its auxiliary apparatus have to be accommodated inside the high-voltage terminal. For positive potentials less than 2 million volts, it may be argued that this advantage offsets the larger space required by a free air machine. An outstanding example of the free-air type of generator was constructed in Norway in 1940–41 by O. Dahl [2] for the municipal hospital in Bergen. This machine is illustrated in Plate I (*a*) and has an output of 2·4 milliamperes at a working voltage of 1·5 million volts. The wartime difficulties under which Dahl and his assistants worked are illustrated by the fact that even the bolts and nuts used had to be made in their own workshops. In spite of these difficulties



(a) The 1·5 million-volt electrostatic generator at Haukeland Hospital, Bergen, Norway. This machine is used for the treatment of cancer by X-rays. (This photograph is reproduced by kind permission of Dr Odd Dahl.)



(b) View of the electrical supplies and control equipment inside the high-voltage terminal of a 2 million-volt pressure insulated electrostatic generator. (A double exposure was made—first with the domed cover removed, then with it in position.)

they completed the machine and it has since been in constant use for the treatment of cancer by X-rays.

Some of the salient features of the design can be seen from Plate I (*a*). The high-voltage terminal is a thin metal " mushroom-shaped " dome, in this case made up from sections welded together. In the pressure-insulated type of machine the high-voltage terminal is more usually a polished metal " spinning," an example of which is shown in Plate I (*b*). The auxiliary apparatus required for filament heating and the electrical supplies for a positive-ion source are contained inside the high-voltage terminal, being mounted on the plate which supports the dome. This plate stands on three or more insulating columns whose lower ends are fixed to the baseplate of the generator. Clamped firmly at equal intervals along these columns, there is a series of circular metal plates whose tubular outer rims are clearly seen in Plate I (*a*) and (*b*). The purpose of these plates is to control the electric field inside the generator by defining a series of equipotential planes. The voltage difference between successive plates is maintained constant by connecting them to equidistant tappings on a resistance potential divider, which is also built into the column. Alternatively, a needle point may be fitted to the lower side of each plate pointing towards the plate below. The voltage drop between plates will then depend upon the small corona current which flows down the column through this series of point-plane gaps. If the gaps are all equal in length and the needle points equally sharp the voltage drop will be the same for all. The metal plates are cut away where necessary to provide space for the charging belt, the evacuated tube in which the particles are accelerated, the resistance column and the control rods.

Modern electrostatic generators are almost invariably built for the acceleration of charged particles—either positive ions for research in nuclear physics, or electrons, if X-rays are to be produced for medical treatment or industrial radiography. A high-vacuum accelerating tube is therefore an essential part of the apparatus and this is usually built inside the generator column side by side with the charging belt. The upper end of the accelerating tube reaches into the high-voltage terminal and supports the electron gun or positive ion source. The accelerating tube itself may be constructed of glass or ceramic sections and it will contain a number of metallic electrodes connected to suitable points on the resistance potential divider. It is found to be advisable to use a large number of such electrodes in order to prevent unwanted discharges inside the tube. In the pressure-insulated generator illustrated in Plate I (*b*) the accelerating tube consists of five porcelain sections joined by rubber-gasketed

vacuum seals. In this machine there are 34 electrodes in the accelerating tube—half as many as there are plates in the column.

Factors limiting the Maximum Voltage Attainable

The voltage at which an electrostatic generator can be operated is limited only by breakdown of the insulation in one way or another. If sparkover in the gas is the method of breakdown, the factors underlying this can be studied in some detail and the design of the machine modified accordingly. We may make the simple assumption that breakdown will occur whenever a certain critical field strength, depending on the nature of the gas used and its pressure, is exceeded in any part of the apparatus. On this assumption we can study how the size and shape of the electrodes affect the maximum working voltage.

In any pressure-insulated generator similar to the one illustrated in Plate I (*b*) the electrostatic field may be taken to consist of (1) a region of cylindrical geometry between the supporting column and the outer tank, and (2) a region of approximately spherical geometry between the high-voltage terminal and the domed end of the tank. It can easily be shown mathematically that, for a given breakdown field strength in the gas, the voltage which can be maintained between two concentric cylinders is a maximum when the ratio of outer radius to inner radius is equal to 2·718. For two concentric spheres the optimum ratio of the radii is 2. In both cases the highest field strength occurs at the surface of the inner electrode, and the aim of the designer must be to reduce this maximum without increasing the dimensions, and hence the cost, of the apparatus. Ideally, the insulating medium would be most economically used if the field strength could be kept uniform across the insulation gap. For cylindrical or spherical geometry complete uniformity is, of course, impossible, but some improvement can be achieved by introducing between the high-voltage terminal and the outer tank one or more intermediate electrodes at controlled potentials. These extra electrodes are polished metal domes similar to, but larger than, the high-voltage terminal itself, which they completely envelop. Each of them is supported on the rim of one of the plates forming the column, as shown diagrammatically in Fig. 2. The potential values for the intermediate electrodes must be chosen so that the critical field strength is reached simultaneously on the outer surface of each electrode. The variation in electric field across the gap when one intermediate electrode is used is then as illustrated in Fig. 2. Both the diameter and the potential of each intermediate electrode can be chosen at will and it is easily shown that for a given number of

such electrodes there is an optimum set of diameters and a corresponding set of potentials which should be used.

For practical reasons it will usually be impossible to introduce more than one or two intermediate electrodes, and, in any case, the percentage gain in voltage for each additional electrode becomes less as their number increases. Several large machines have been designed [3, 4], however, with two intermediate electrodes giving a reduction of some 40 per cent. in the maximum field strength for a given voltage, compared with the optimum design not employing intermediate electrodes which could be built inside the same size of pressure tank.

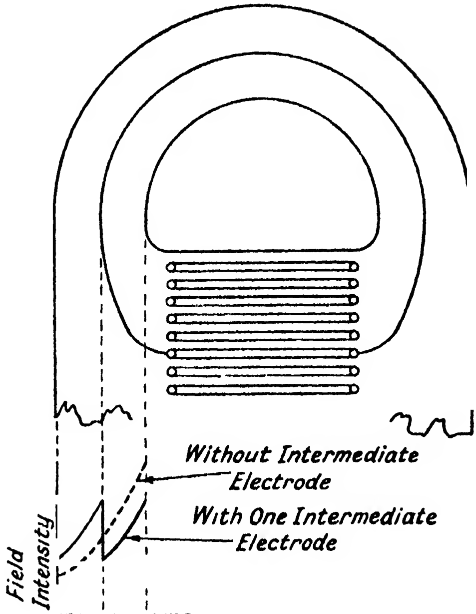


FIG. 2.

It is well known that when a small hemispherical boss is formed on a plane conductor having a surface density of electric charge the electric field close to the boss at its highest point is three times as great as it was for the smooth plane. It is therefore important to ensure that the surfaces of the high-voltage terminal and of the rings forming the column are reasonably smooth. Microscopic projections on these surfaces seldom cause a serious reduction in the sparking voltage, but they may lead to a loss of current by initiating corona or "brush" discharges from the projecting points. The curved profile of the tube which forms the edge of the plates in the column increases the field strength at its surface, however, above the value calculated for a smooth cylindrical column by a factor which may be as high as two. This "field distortion factor" can be reduced considerably by giving the rings an oval profile, as shown in Fig. 3, the minimum curvature being

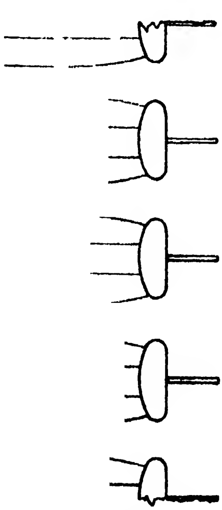


FIG. 3.

on the outer cylindrical surface. The field strength between adjacent rings will be increased thereby, since the oval profile has necessitated a smaller radius of curvature on the edges of the rings which face one another. As a general rule, however, the field between rings is much lower than the radial field and this increase raises no problem.

The other principal factors influencing the maximum voltage which a particular generator can attain are the nature and pressure of the gas used and the breakdown voltage of the accelerating tube.

Insulating Medium

In pressure-insulated generators any suitable gas can be used. Air is objectionable owing to the fire risk, and for this reason nitrogen is frequently used. The breakdown strength of nitrogen for short gaps in a relatively uniform field increases nearly linearly with gas pressure [5]. Pressures up to 400 lb. wt./in.^{2} have been used, but it is invariably found that the curve of sparking voltage against pressure for the completed machine is not linear and frequently it becomes very flat at the higher pressures. This discrepancy is particularly marked for positive polarity of the high-voltage terminal, and the consequence may be that a pressure of about 200 lb. wt./in.^{2} will prove to be the optimum when nitrogen is used as the insulating medium.

The gas known commercially as Freon 12, whose chemical composition is CCl_2F_2, has a breakdown strength more than twice as great as nitrogen [6], at the same pressure, and has been used, alone or mixed with nitrogen, in a number of machines. The maximum pressure of Freon which can be used is limited to a few atmospheres, however, since its saturation vapour pressure at 20° C. is only 67 lb. wt./in.^{2} and it is not convenient to maintain the whole generator at a much higher temperature than 20° C., owing to the necessity for cooling the electrical apparatus inside the machine. Another disadvantage is that Freon is decomposed by sparking and corona into products which are corrosive and are said to give rise to conducting deposits on insulator surfaces [3]. The expense of Freon 12, although considerably greater than that of commercial nitrogen, is not so high as to forbid its use entirely. Another gas, sulphur hexafluoride, has even better insulating properties than Freon and is not seriously decomposed by sparking. Moreover, its vapour pressure at room temperature—330 lb. wt./in.^{2} at 25° C.—is much higher than that of Freon, and in every respect it would be a very suitable insulating medium for electrostatic generators. Its cost, however, is at present prohibitive.

Factors influencing the Maximum Current

The current carried to the high-voltage terminal of an electrostatic generator is proportional to the area of belt entering the terminal per second and to the charge density on its surface. The belt area per second is simply the product of the number of belts used, the belt width and the belt velocity. In pressure-insulated machines there is usually room for only one belt and any increase in belt width puts up the linear dimensions of the whole machine in the same ratio and the cost roughly in the cube of this ratio. A limit is set to the belt speed which can be used by the wear on both belt and bearings. In practice, 5000 ft./min. is about the maximum speed which has been used without decreasing the life of belt and bearings unduly. In view of these limitations it is very important to be able to maintain as high a charge density as possible. Now, in accordance with Gauss' theorem, any surface charge of density σ coulomb./cm.^{2} gives rise to a discontinuity in the electric field normal to the surface, of amount

$$F = 4\pi\sigma . 9\cdot 10^{11}\frac{\text{volts}}{\text{cm.}} \quad . \quad . \quad . \quad . \quad (1)$$

If the lines of force from the charged surface are all directed in the same sense, the medium into which they pass has to withstand the full electric field F. In the electrostatic generator illustrated in Fig. 1, for instance, the lines of force from the charged belt as it leaves the lower pulley will practically all pass towards the pulley, as this is the nearest earthed metal surface. The wedge of gas between the pulley and the belt at the point of tangency has therefore to withstand the field F, and since sparking must be avoided this sets a limit to the charge density which the belt can carry. If the breakdown gradient for the gas used is $F_{max.}$ volts/cm. and if w is the belt width in cm. and v the belt velocity in cm./sec., then the theoretical maximum current which the belt can carry is

$$i = w . \sigma_{max.} . v \text{ amperes}$$

$$= wv . \frac{F_{max.}}{4\pi . 9\cdot 10^{11}} \text{ amperes} \quad . \quad . \quad . \quad (2)$$

A belt 50 cm. wide moving at 2500 cm./sec. should therefore be able to carry 275 microamp. in air at atmospheric pressure (breakdown field approximately 25 kV. per cm.). If air at high pressure is used, or some other gas having a higher dielectric strength than air, the belt will carry a higher current, corresponding to the higher permissible charge density. In practice, it is not possible to work

up to the theoretical limit. Under favourable conditions a belt efficiency of 80 per cent. can be attained, but in most cases the figure reached is less than 50 per cent. It may be noted that if only half the lines of force from the charged belt pass to the pulley at the point of tangency, and the other half to a similarly shaped earthed metal surface, symmetrically placed with respect to the belt, as in Fig. 4, the field strength in the gas will be halved, and the maximum charge density which can be sprayed on to the belt will be doubled.

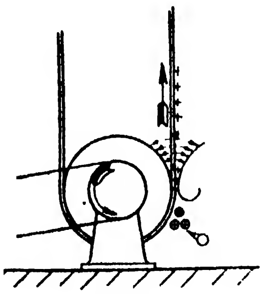


FIG. 4.

An important improvement in design introduced by Trump [7] was the addition of "belt guards." These are metal rods or tubes placed parallel to the belt surface at a distance of a few millimetres from it. They are attached mechanically and electrically to the plates forming the generator column and by their proximity to the charged belt surface they control the potential of the latter and keep the charge density relatively uniform. Only by their use is it possible to obtain a satisfactory belt efficiency.

Hitherto we have assumed that only the upgoing run of the belt is used for conveying charge, but it is clear that the total current to the high-voltage terminal can be doubled if the downgoing belt run is made to carry away charge of the opposite sign to that which the upgoing run delivers. This could be achieved by providing a spray comb facing the downgoing belt as it leaves the upper pulley and connecting it to an independent high-voltage source inside the top terminal. There is, however, an alternative method of excitation which is frequently used. This scheme is illustrated in Fig. 5. The top belt pulley is insulated from the high-voltage terminal and is connected only to a spray comb facing the upcoming side of the belt, which, we shall suppose, carries positive charge. The field due to the charged belt will produce corona discharge at the points of the comb and a stream of negatively charged particles will leave the comb and partially cancel the positive surface charge on the belt. This current will leave the insulated belt pulley positively charged. If a spray comb connected directly to the high-voltage

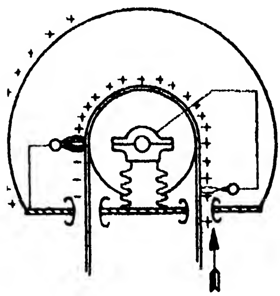


FIG. 5

terminal is now arranged to face the downgoing side of the belt, then the field from the positively charged pulley will produce a negative corona current from this comb, which will not only cancel the remaining positive charge on the belt but also charge the downgoing belt negatively. If this simple arrangement were used, a very small charge density on the upgoing run of the belt would induce saturation charge density on the downgoing run and this would make control of the total current difficult or impossible. By providing a corona leak to limit the voltage between the top pulley and the plate which supports it, and by inserting suitable series resistances in the various corona gaps, the charge densities on the up and down belt runs can be kept approximately equal. The total generator current can then be controlled smoothly from zero up to its maximum value by merely varying the voltage of the lower comb.

Charging Belt

The charging belt has sometimes been made of electrical insulating paper, at other times of silk, cotton or rubberized fabric. The properties which a suitable belt material must have are high insulation resistance, high electric strength and good mechanical properties—especially resistance to wear and to permanent stretch. Joints in the belt are undesirable as they may lead to irregularities in the charge distribution and hence in the current reaching the top terminal. It is found that the life of a belt in a pressure-insulated generator used for eight hours per day is of the order of three months. In free-air generators the belt life may be years.

Measurement of Voltage

The potential of the high-voltage terminal can be determined in a number of ways. A direct measurement of the current flowing to earth at the lower end of the resistance potential divider would, in theory, enable the top terminal voltage to be deduced if the resistance value were accurately known. In practice, however, such a method is usually vitiated by the presence of unknown corona currents at different points down the resistance column. Other ways of determining the voltage involve measuring the energy of the particles accelerated in the machine, either by deflecting the beam in a known magnetic field or by observing the threshold voltage for the occurrence of well-known nuclear disintegrations. Neither of these methods, however, provides a convenient indication of the voltage during normal operation and for this purpose a "generating voltmeter" [1] is commonly used. In principle this

consists (Fig. 6) of a series of insulated metal segments facing the high-voltage terminal. In front of the insulated segments there is a rotating vane which is earthed. This moving vane has half as many blades as there are insulated fixed segments, and as it rotates each fixed segment is alternately covered by one of the moving blades and then exposed to the electric field due to the high-voltage terminal (Figs. 6*a* and 6*b* show successive positions). During the latter period a surface electric charge is induced on the fixed segment, a charge which falls to zero again when this segment is completely screened by the next moving blade. Alternate fixed segments are connected together electrically and the two groups of segments thus formed are connected through an external circuit in which the charge fluctuating between them can be measured. The amount of charge induced is

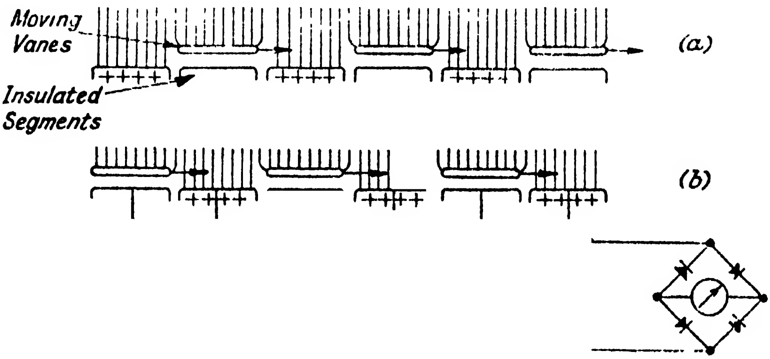


Fig 6

proportional to the electric field at the surface of the insulated segments in accordance with Equation (1) and this, in turn, is proportional to the voltage of the terminal. The generating voltmeter must be calibrated by comparing its readings with voltage measurements made in one of the other ways described above, but once this has been done it gives a reliable indication of the instantaneous voltage of the machine and the output from the voltmeter can be amplified to operate relays or feedback circuits for voltage stabilisation.

Control System

When the generator is in use it is necessary to control the auxiliary apparatus for producing electrons or positive ions, which is housed inside the high-voltage terminal. If the machine is being run with negative polarity as an electron accelerator the controls required are few and simple. The electrons are obtained from a hot filament

which can be supplied with current from a small alternator, belt driven from the main pulley. A filament control rheostat and an on-off switch are then the only essential controls, and these can be operated by insulating cords or rods running from the high-voltage terminal to the base of the machine. Remote control of these cords is usually arranged by means of solenoids or selsyn * motors. When a positive-ion source has to be operated inside the top terminal the control problems are more complicated. It may then be necessary to control six or eight separate functions such as arc current, gas flow, focusing voltages, etc., and this leads to a multiplicity of control cords. As an alternative, small servo motors can be fitted to the separate control units inside the top terminal, these being fed from the belt-driven alternator through a selector switch and relays. The relays may be controlled by cords as before or by light beams operating photoelectric cells. A system of this kind is used in the generator illustrated in Plate I (*b*). By a suitable coding of the light beams, any one of the separate functions can first be selected and then controlled up or down, the meter readings inside the high-voltage terminal being observed meanwhile through a telescope from the control desk. The arrangement of apparatus inside the top terminal is seen in Plate I (*b*), which is a double exposure taken first without and subsequently with the metal spinning in position.

Ion Source

If it is desired to accelerate positive ions in the machine these must be produced in an auxiliary apparatus situated inside the high-voltage terminal. Many different types of ion source have been suggested. In most of these hydrogen or deuterium gas is ionised in the positive column of a gaseous discharge. Some of the ions produced in the discharge are withdrawn through a small hole or along a narrow canal and, as they emerge from this, they are focused into a parallel beam which is projected into the upper end of the accelerating tube.

The amount of neutral gas escaping into the accelerating tube

* Selsyn motors are used when it is desired to transmit a controllable rotation from a "master" motor to a remote "slave." The two motors are connected together electrically and, as long as the power supply is on, the relative angular position of the two armatures remains unchanged. If the armature of the "master" is turned by hand through 90° the armature of the "slave" automatically follows. In an electrostatic generator the "slave" operates the control cords at the base of the generator column, while the master can be mounted on the control panel some distance from the machine.

along with the ions must be kept to a minimum to prevent the pressure rising to a value which might permit a discharge to occur inside the tube. An efficient ion source must produce a high proportion of monatomic ions. Such ions arrive at the target with kinetic energy corresponding to the full accelerating voltage, whereas in a molecular ion consisting of one ionised atom bound to a neutral atom the total kinetic energy is shared equally between the two particles, and these are together much less effective in producing nuclear disintegrations than is a single particle having the whole available energy.

Accelerating Tube

The problems involved in the design of the accelerating tube are three-fold: (1) electrical discharge inside the tube must be prevented; (2) sparking along the outside surface of the tube must not occur; (3) the particles must be kept focused during acceleration. The first problem requires that the gas pressure be kept lower than about 10^{-5} mm. of mercury. If the tube is used solely for accelerating electrons this vacuum is easily maintained once the tube has been "degassed." Indeed, electron accelerating tubes which were permanently sealed after manufacture have been used up to potentials as high as 2 million volts, thus eliminating vacuum pumps entirely. If positive ions are to be accelerated, however, gas has to be admitted continuously at the top of the tube and the pressure can only be kept low by pumping this gas away rapidly. The resistance to flow of rarified gas through a tube is inversely proportional to the cube of its diameter, and it is therefore important in designing a tube for accelerating positive ions to keep the diameter as large as possible. The danger of a discharge occurring inside the tube is less if the voltage gradient along the tube is relatively uniform, and this is best achieved by splitting up the acceleration into a large number of separate stages, the electrodes inside the tube being connected to appropriate points on the resistance potential divider. In a system of this kind, most of the gaps between adjacent electrodes form relatively weak electron lenses and, except for the first few stages, their focusing effect on the beam is small. It is therefore necessary to produce a well-collimated beam of electrons or positive ions from the source and to project this beam at the correct angle of divergence into the accelerating tube.

Future Trends in Design

Since the electrostatic generator is a machine in which insulation has to be provided for the total available accelerating voltage, it

cannot compete in the highest voltage region with orbital accelerators employing a multiple acceleration principle. The maximum voltage yet achieved by an electrostatic generator is about 6 million volts. Future developments may easily raise this limit to 10 million volts or perhaps a little more without a disproportionate increase in cost, particularly if sulphur hexafluoride becomes readily available. Designs have been proposed in which solid insulation is provided between the central column and the earthed outer tank, but the high cost and the danger of a single breakdown causing permanent destruction of the insulation are likely to make this design impracticable. In any case, sulphur hexafluoride at high pressure is not inferior in electric strength to most solid dielectrics.

The electrostatic generator has now passed out of the experimental stage into the stage of engineering development. While many improvements in detail remain to be made the broad lines of the present design are likely to be preserved in future machines.

Applications of Electrostatic Generators

It has already been emphasised that the electrostatic generator was developed to meet the needs of nuclear physicists for a high-voltage particle accelerator. Although outstripped as a positive-ion accelerator by the cyclotron, and as an electron accelerator by the betatron, synchrotron and centimetric wave linear accelerator, the electrostatic generator has still the advantage that it can be used with very little rearrangement to accelerate either positive or negative particles, so that radiations of many different types—X-rays, fast electrons, neutrons, protons—can be obtained from the one machine. This makes it a very valuable general-purpose machine for a small research department.

Another unique feature of the electrostatic generator is the degree of voltage stabilisation which can be achieved. Some existing generators have been stabilised to $\pm \frac{1}{10}$ per cent., and even this could be improved upon if necessary. The accurate measurement of the threshold energy of nuclear reactions is thus made possible.

The compactness and mobility of a modern 2 million volt electrostatic X-ray generator makes it particularly suitable for industrial radiography and for the radiotherapy of cancer, and it is probable that a considerable number of machines will find application in these two fields.

REFERENCES

1. Van Atta, Northrup, van Atta and van de Graaff, *Phys. Rev.*, **49**, (1936), 761.
2. Dahl. *Electrostatic X-ray Generator for* 1500 *kV*., Grieg, Bergen, 1943.
3. Buechner *et al.*, *Rev. Sci. Instrum.*, **18** (1947), 754.
4. Herb *et al.*, *Phys. Rev.*, **58** (1940), 579.
5. Skilling and Brenner, *Trans. Am. Inst. Elect. Eng.*, **61** (1942).
6. Trump, Safford, Cloud, *Trans. Am. Inst. Elect. Eng.*, **60** (1941).
7. Trump and van de Graaff, *Phys. Rev.*, **55** (1939), 1160.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By MICHAEL W. OVENDEN, B.Sc., F.R.A.S., University Observatories, Cambridge.

INTERSTELLAR MATTER.—**1.**—The space between the stars contains a diffuse but widespread distribution of matter, concentrated to a degree into separate clouds. These clouds present themselves to observation in a number of different ways, according to their nature and position.

Probably the most noticeable clouds are those which cause obscuration and apparent absence of stars over regions of the sky, being most conspicuous in the Milky Way. The properties of those obscuring clouds indicate that a high proportion of their constituent matter is in the form of particles whose diameters are much greater than the wavelength of light (although the frequent association of luminous nebulæ with dark clouds points to some general similarity of distribution of particles and a gaseous constituent). The prominent Milky Way clouds are probably relatively near the solar system, between 100 and 1000 parsecs from the sun, and the extent of the distribution of matter is indicated by the fact that Barnard's list contains over 400 such dark clouds in the Milky Way. On extending the investigation to cover all galactic latitudes, Melotte has compiled a list of 1500 obscuring clouds. Prominent also are the luminous nebulæ associated with some bright stars. That these stars are the cause of the luminosity of the nebulæ is shown by the fact, first pointed out by Hubble, that the angular extent of the luminosity is directly proportional to the apparent brightness of the associated star. The spectra of these bright nebulæ show a dual characteristic. If the illuminating star is of spectral type later than B5 the nebula spectrum is continuous, crossed by absorption lines, and is essentially similar in type to the spectrum of the parent star. Struve's work on the colour indices of these nebulæ indicates that the luminosity is due to star-light reflected or scattered by particles large in comparison with the wavelength of light ; such nebulæ are thus known as "reflection nebulæ." If, on the other hand, the parent star is of earlier type, then the nebula is excited to produce

a line emission spectrum of its own, which may be sufficient to mask any continuous reflection spectrum. Prominent in the emissions are lines of hydrogen, oxygen and calcium, and forbidden transitions are not infrequent, further evidence of a tenuous gaseous constituent associated with the dust clouds.

These clouds are exceptional concentrations of interstellar matter, with densities in some cases as much as 1000 times the mean interstellar density. More tenuous clouds are unable to cause complete obscuration, but can reduce the observed brightness of stars in their directions. The absorption is more effective with light of short wavelength, and such clouds were detected by Trumpler in an extensive investigation of the colour excesses of stars in low galactic latitudes. Trumpler showed that these tenuous interstellar clouds showed the same concentration towards the galactic equator that is exhibited by the more dense clouds.

On one unique occasion, it was found possible to obtain a three-dimensional picture of the distribution of interstellar matter over a region of limited extent. The light-signal from the outburst of Nova Persei in 1901 was observed to travel out from the star, gradually illuminating an increasing volume of gas in the neighbourhood of the star to some 50 parsecs distance. The material in this region appears to be concentrated in two or three thin layers some $\frac{1}{10}$ parsec in thickness, and extending to 10 parsecs in other dimensions.

Finally, interstellar gas clouds may selectively absorb starlight passing through them, and produce characteristic absorption lines in stellar spectra. The subject of this note is a recent extensive investigation of these "interstellar lines" undertaken by Adams and his co-workers at the Mount Wilson Observatory, and described by him in his recent Russell lecture to the American Astronomical Society. These observations are the most complete that have yet been made on this subject, and they have placed our knowledge of interstellar gas clouds on a very firm basis.

2.—Attention to these interstellar lines was first drawn by Hartmann in 1904, when he noticed that the K-line of calcium in the spectrum of the spectroscopic binary δ Orionis did not partake of the oscillations characteristic of the other lines of the spectrum; he named them "stationary lines." Similar lines in other spectra were soon noticed, and the hypothesis first suggested by Eddington that these lines originated in interstellar clouds of ionised calcium commanded attention and rapid acceptance; research on interstellar lines quickly became a significant branch of stellar spectroscopy.

In the interpretation of these early researches, it was customary

to assume that, even if the distribution of interstellar matter were not uniform, at least the density diminished uniformly with increasing galactic latitude. It was natural, therefore, to explain Struve's statistical observation that the relative intensities of interstellar compared with stellar lines increased with stellar magnitude of the star in question as an effect merely of distance: the greater the distance of the star from the observer, the fainter the star and the greater the effective optical thickness of the absorbing clouds. By reversing the argument, measurements of intensities of interstellar lines could be used to estimate stellar distances. Making the same basic assumption, Plaskett and Pearce used the Doppler displacements exhibited by interstellar lines in an investigation of general galactic rotation. Significant and useful results were undoubtedly obtained from these early researches, but it soon became apparent that, not only was the distribution of the clouds far from uniform, but individual clouds had large peculiar velocities.

Had research been restricted to the study of the spectra of spectroscopic binaries, progress would have been slow. However, interstellar lines are in general very narrow, and this characteristic has enabled them to be identified in a very large number of stars which show no orbital Doppler displacements. Lines other than the H and K lines of calcium were noticeable; for example, lines of neutral sodium were quickly recognised. Since these lines all originate in transitions from the ground state, search was made for similar lines in other elements, and neutral iron, calcium, and potassium, and ionised titanium were found not uncommon. The remaining unidentified lines presented a more difficult problem, but were finally found to be due to neutral cyanogen (CN) and neutral and ionised CH. The presence of lines of both neutral and ionised forms of atom or molecule enable estimates of the relative abundances of these different states to be obtained from relative intensity measurements of the interstellar spectra, although these estimates are liable to considerable uncertainty.

When the unevenness of distribution was realised, a means was sought of distinguishing between individual interstellar clouds. Beals, who was one of the first to approach this problem, reported in 1936 that in some spectra the interstellar lines were double or multiple in structure. The components of these complexes were interpreted as due to different clouds with different sightline velocities. It was this discovery that gave the impetus to the Mount Wilson programme.

3.—In order to appreciate the advantages of the Mount Wilson equipment for this work, it is worth considering some of the

difficulties encountered in the experimental investigation. Serious difficulty arises in those cases where the transitions causing the interstellar lines are paralleled by similar transitions in the actual stellar spectrum, for separation then depends upon the differential Doppler displacements of the two sets of lines, which may be quite small. The H and K lines appear in spectra of classes later than B5; difficulties in obtaining sufficient dispersion to separate interstellar lines in these cases has limited much of the work on interstellar lines to earlier-type stars. High dispersion is also needed if the interstellar lines are to be satisfactorily resolved into their individual components for the analysis of structure. The necessity for high dispersion places a severe limit upon the faintness of the stars whose spectra can be satisfactorily studied, and so it is advisable to have as great a light grasp and as sensitive a recording apparatus as possible. One cannot get "something for nothing" and the high dispersion is usually obtained only at the expense of compactness of apparatus. On most telescopic equipment this is a serious disadvantage; only one type of mounting, the Coudé, is free from this limitation, for in such a mounting the image plane has a fixed location, and the most satisfactory results may thus be expected from a Coudé spectrograph. The same difficulty is, of course, common in many branches of astronomical spectroscopy. The original Mount Wilson Coudé spectrograph was designed to overcome just this difficulty, for Hale had intended it to be used for the investigation of stellar magnetism, to which problem it has recently been successfully applied. The spectrograph, as described by Adams, consisted of a plane diffraction grating in conjunction with a Schmidt camera giving a dispersion of about 3 Å./mm., with a subsidiary system giving 10 Å./mm. for some of the fainter stars of the programme. The photographic plates covered the range 3500–4700 Å., into which range the principal interstellar lines fall.

4.—The programme covered some 300 stars. Most of these were in the spectral range O·B5, which do not have stellar H and K lines. It was shown by experiment, however, that given sufficient dispersion it was possible to detect interstellar lines in stars of spectral type as late as G0. Distant stars of later types present the greatest difficulties, and a search for faint K-type stars with high radial velocities would be valuable in these studies. Preliminary analysis of the results has brought to light important information on the occurrence of molecular lines, the structure of individual lines indicating multiple clouds in the line of sight, radial velocities of individual clouds, and relative intensities of different lines under various conditions. In the interpretation of these results, it must

be borne in mind that selectivity of observational data must play an important part. In all branches of experimental science, some degree of selection of observed systems must occur, for always some will present themselves more easily to observation and experiment than others. But, whereas in most laboratory sciences selectivity plays but a small part, in astronomy, where the emphasis is perforce still on "observation" rather than "experiment," it will be of major importance. In many branches of astrophysics its significance is only now being realised; for example, recent discussion has led to the suggestion that many of the characteristics of the well-known Russell diagram, linking spectral type with luminosity, may be characteristics, not of the actual stellar system, but merely of that group of stars selected for observation. So, too, in the study of interstellar absorption lines the mark of observational selection is apparent. Interstellar clouds are recognised only when in the line of sight of a star of sufficient brightness; thus estimates of the average angular extent of these clouds depend upon the few observations of bright doubles of various angular separations, and are therefore subject to some uncertainties. Mention has already been made of the restriction, because of experimental difficulties, to early-type stars. These stars show a marked preference for the galactic plane, and caution must thus be used in interpreting the observations as indicating a similar preference for the gas clouds. In the identification of molecular lines, considerations of dispersion limit the data to bright, early type stars, but again this would not warrant the conclusion that molecular lines were a peculiar property of early type stars, or that the clouds producing them had some physical connection with such stars. For similar reasons, molecular lines cannot be observed with certainty in the more distant stars, and little evidence exists for or against the possibility of an intensity-distance proportionality such as is known for the H and K lines; thus the supposition that, because such a relationship has not been observed, it can be inferred that the large-scale distribution of molecular gases differs in important particulars from that of the calcium clouds has no real basis in observation. To a large extent, our knowledge of complexity of interstellar lines is confined to the H and K lines, but information is available for molecular lines, and will be discussed.

An examination of the Mount Wilson plates for molecular lines was able to confirm McKellar's identification of bands of neutral CH and neutral CN, and Douglas' identification of ionised CH. Neutral CH was found to be almost invariably associated with neutral CN. A similar relationship was apparent between neutral

iron and calcium, and ionised calcium, lines of the neutral elements being always found in spectra showing strong H and K lines. Ionised CH was found to be more common that neutral CH. These details are of interest to the theoretician, but the real importance of this new systematic search in this field was to bring to light the prevalence of molecular lines in interstellar absorption ; the spectrum of one star in five was found to contain them. A few stars were examined which showed all the known interstellar lines in each of their spectra.

To throw light upon the problem of the average sizes of these interstellar clouds, Adams compared the spectra of a few pairs of stars of small apparent separation. The indication was that stars separated by about 1° or less showed similar interstellar lines, but by the time the separation is increased to about 5° considerable differences are found to have appeared, the most marked of these changes being in the relative intensities of molecular lines. These results are, of course, very approximate, and exceptions occur, for example in Orion and the Pleiades, where areas of large angular extent are found, which are nevertheless apparently individual clouds.

Indirect evidence of the extent of a single cloud can be obtained from a knowledge of the average number of separate clouds in a given direction, to a known distance. Such information is furnished by the structure of individual lines. For the H and K lines, complexity was found to be the rule rather than the exception. Over half the stars examined were found to have resolvable structure, and many others showed broader lines which could probably have been resolved had higher dispersion been available. Of the multiple lines, doublets were the most common, accounting for 80 per cent. of the observations, 15 per cent. were triple, and 5 per cent. showed more than three components, as many as six separate components in a single spectrum having been observed. The incidence of complexity showed a definite preference for the galactic plane, with subcentres in Orion, Sagittarius and Cygnus, but the bias is less marked than that exhibited by line intensities. The identity of individual clouds is brought out when the radial velocities given by the more intense components are examined. Very considerable differences in sightline velocity are apparent between one cloud and another, but, after allowing for the motions of the earth and sun, the indication is of random velocities of the order of 20 km./sec. The most interesting result of the radial velocity work, however, was the discovery that the most intense lines invariably show a small velocity of about 5 km./sec. only. The intensity of a line, being strongly correlated with effective optical depth, gives some idea of

the areal density of the cloud concerned. It would thus appear that the larger or more dense the cloud, the smaller is its random velocity. Intensity measurements are not capable of very high precision, and it is doubtful whether with present techniques it would ever be possible to deduce an actual velocity-optical depth relationship, even were sufficient observations available for such a statistical law to become apparent. However, as far as it goes, the evidence is strong enough foundation for the belief that some form of interaction between different interstellar clouds does occur. In fact, calculations with the approximate figures available suffice to predict that collisions between clouds will be not infrequent (astronomically speaking). Some of the more interesting results of such a collision will be mentioned later. For molecular lines, the incidence of complexity is uncertain, as very few molecular lines have been resolved. However, complexity of H and K lines is almost invariably associated with broad molecular lines, the breadth being such as would correspond to the Doppler shift of that cloud giving the strongest component of the H and K lines. In fact, there is evidence that, except in a few anomalous cases, no important difference of distribution exists between atoms and molecules in interstellar gas.

Radial velocity measures of spectra of neighbouring stars confirm the previous conclusion that individual clouds have, on the average, angular extents of only a few degrees. It is pertinent to ask whether these clouds are widely dispersed throughout the stellar system, or whether, on the other hand, they are closely associated with stars. For the H and K lines, the relation between intensity and distance points definitely to the former alternative of a widespread distribution. Radial velocity measures lead to the belief that molecules and atoms are closely associated, and, although no satisfactory intensity measures are available for molecular lines, there is no sound reason why their behaviour should differ from that of the atomic lines. It can thus be concluded that no close association exists between interstellar matter and individual stars. Such a suggestion has been made from observations of luminous nebulæ, but it is only the property of luminosity, and not the presence of matter, that requires the proximity of an early-type star. As usual, there are notable exceptions to the rule, and in Orion a concentration of early-type stars coincides with a marked concentration of matter, but no great importance need be attached to this, as the whole region is found with six-colour photometry to be anomalous in other respects.

On the question of the determination of galactic rotation by

measures of the radial velocities of interstellar clouds, Adams draws no conclusion, but states his belief that a small variation of average velocity with galactic latitude is probably to be attributed to this cause.

The most difficult part of the investigation, and one likely to contain inconsistencies, is the determination of relative intensities in a given spectrum. In many cases even rough estimates are impossible from a superficial examination, and the results of this part of the programme will only be arrived at after a complete microphotometric analysis. It is possible to say something of a negative nature, however. Relative intensities have been used to deduce the relative abundances of different atoms in the gas clouds; this process is of doubtful validity, quite apart from the observational uncertainties involved, as many unknown physical factors may influence the line intensity. Thus, for the sodium D lines, it has been found that the relative intensity of this doublet is only about 1·2 in interstellar lines, whereas in the case of a simple proportionality it should be about 2. Estimates of abundances must thus be treated with caution. The explanation in this case is probably that the atomic velocities do not have a Gaussian distribution, and no definite temperature can be assigned to the congregation. The correct method of procedure would be to construct a curve of growth for the interstellar lines, but the lines are so few that many practical difficulties must be faced.

5.—It is now possible to build up a consistent picture of the distribution of interstellar matter. From visual observation of dark and luminous nebulæ, the distribution is seen to be very irregular. This patchy character is completely confirmed by recent spectrographic work. In fact, from many lines of approach, there is no reason to believe that the distribution of solid particles and gaseous matter is other than essentially identical, although small-scale differences in detail will, of course, occur. In the neighbourhood of the solar system, there exist two large extended systems of clouds, one in the Taurus-Perseus region, and the other centred about the constellation Ophiuchus. Careful star counts have traced each of these systems to over 500 square degrees. Their distance is about 150 parsecs, and so their diameters are about 50 parsecs, with a central denser nucleus of diameter 20 parsecs, values in general agreement with the average diameters of more distant clouds deduced from spectrographic data. It is possible to obtain a rough estimate of the average extent of individual clouds by considering the number of clouds in a given line of sight for a given distance. Adams has estimated that a new component is added to an interstellar line for

every 50 parsecs increase in the distance of the star in whose spectrum the line appears. Individual clouds are thus separated from their neighbours by distances of the order of 50 parsecs. Counts of luminous nebulæ show that about 15 per cent. of early-type stars have associated nebulosity, and, since no physical connection is believed to exist between nebulosity and early-type stars, it is concluded that cloud regions occupy some 15 per cent. of the total space near the galactic plane. Combined with the previous figure, a value of about 20 parsecs for the average cloud diameter is obtained. All figures are of course highly approximate, but their agreement with the figures derived for the clouds near the solar system is encouraging. Some very tenuous matter will doubtless exist between these clouds, but it would be impossible from present observations to say whether its density was a tenth or a hundredth of the mean density. It is probable that a considerable proportion of interstellar matter is concentrated into these clouds. That they exhibit a definite preference for the galactic plane is shown by the discussion of complexity in interstellar lines, and this preference is greater for clouds with large optical depths (denser clouds ?) than for thinner ones.

With a knowledge of the average random velocity for the clouds, simple calculation with the figures given above for the sizes of the clouds shows that a given cloud must collide with another on the average once every 10^7 years. This is much less than the age of the stellar system, and such collisions will be relatively frequent. Other evidence, more indirect, for the existence of some such interaction has already been pointed out. A cloud may thus not retain its identity over astronomical periods of time.

If such collisions do occur, interesting possibilities arise, and have been considered by Oort. The relative velocity between the clouds will be greater than the velocity of sound in the clouds, and the interaction will take the form of a shock wave. An intermediate highly compressed region will be set up at rest relative to the average motion of the clouds. Temperatures within this region will rise sufficiently to cause collisional ionisation of any hydrogen present. Oort sees in this mechanism the explanation of both the bright rims frequently seen to dark nebulæ, and those luminous wisps that are seen without any apparent association with a bright star. It is possible that the wavy structure in some of the Pleiades nebulosity has a similar origin.

6.—The important results of the Mount Wilson work may be summarised thus :

(i) Molecular lines are prevalent in interstellar spectra, and no

fundamental difference in distribution exists between atoms and molecules.

(ii) Multiplicity is a general feature of interstellar lines, and the complexity is a function of the distance of the star concerned.

(iii) A large proportion of interstellar matter is concentrated into relatively small clouds, which are truly interstellar and show no preferential association with any particular type of star.

(iv) These clouds have random velocities whose magnitude is a function of the optical thickness of the cloud.

(v) Interstellar matter shows a preference for the galactic plane that is more marked for thick clouds than thin ones.

These results do not, perhaps, tell us anything surprising or unexpected, but they are the result of a painstaking research over a number of years with the best equipment available working at the limit of its power, and the progress of science depends upon the steady accumulation of data for discussion. Complete analysis of the photographic plates will be a lengthy task; the results are eagerly awaited. Whatever they may be, this extensive Mount Wilson investigation will for long be the standard of observational reference for all discussions of interstellar matter, for it has put on a definitive plane much that previously had been little more than speculation.

PHYSICS. By F. A. Vick, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

Phenomena at Very Low Temperatures—Part II

Superconductivity.—**1.**—The phenomenon of superconductivity of certain electrical conductors at very low temperatures is perhaps the major outstanding puzzle in the general theory of the behaviour of electrons in solids. When some metals, alloys or compounds are cooled to very low temperatures, their resistance suddenly drops (often discontinuously) to practically zero. The transition temperature of the pure metals varies from about 9·2° K. for Nb to about 0·35° K. for Hf. Some of the Nb compounds exhibit this property at temperatures as high as 15° K. In a pure metal (*e.g.* Pb) there is good evidence that the specific resistance below the transition temperature is less than 10^{-15} of the value at 0° C. Superconductivity is thus the property of apparently infinite electrical conductivity at temperatures appreciably above the absolute zero. If a superconductor is placed in a gradually increasing magnetic field, its resistance begins to increase at a particular value of the field strength and goes on increasing rapidly until the normal

resistance at that temperature is reached. The threshold value of the field (usually defined as that field required to restore half of the normal resistance of the specimen) depends on the temperature of the superconductor, and is of the order of hundreds or thousands of oersteds. Until the normal resistance is reached, the magnetic field fails to penetrate a crystal of a pure superconductor placed parallel to the field (*i.e.* $B = 0$). Also, when a crystal in a longitudinal field is cooled from above the transition temperature, B suddenly becomes zero when this temperature is reached. A superconductor is, therefore, in this case a perfect diamagnetic substance. When a sphere is placed in a magnetic field, the transition is not sharp, and over a range of temperature or field some regions are superconducting and others are not. According to Shubnikov and Nakhatin (*Nature*, **139**, 589, 1937), in the intermediate range a sphere is superconducting in a direction parallel to the field, but has normal conductivity in a direction perpendicular to this. The electric current flowing through a superconductor is superficial, being confined to a very thin surface layer, as may be derived from Maxwell's equations for a perfect conductor. If the nominal current density in a superconducting wire is raised above a critical value, the normal resistance again appears. The critical current is that required to produce the threshold value of the field at the surface of the conductor. Many investigations of the physical properties of superconductors have naturally been made, but very little light has been thrown on the fundamental reasons for the phenomenon. X-ray examinations have shown that there is no observable crystal change when superconductivity sets in. The thermo—e.m.f. between two metals disappears when both of them become superconducting. The transition temperature depends on the average distance between the metal atoms; compression (*e.g.*) lowers this temperature. Accounts of earlier experimental and theoretical work are given in several books and review papers (*e.g.* D. Shoenberg, *Superconductivity*, 1938; L. C. Jackson, *Low Temperature Physics*, 2nd edition, 1948; K. Mendelssohn, "Superconductivity," *Phys. Soc. Rep. Progr.*, **10**, 358, 1944–45; *Phys. Soc. Int. Cfce. Report*, Vol. 2, 1947). The report by Dr. Mendelssohn is particularly valuable for references to work carried out on the Continent during the war.

2.—Among the most interesting of recent investigations are those of the high-frequency resistances of superconductors, carried out largely in the Royal Society Mond Laboratory, Cambridge. H. London had already made some measurements (*Proc. Roy. Soc.*, **A176**, 522, 1940; *Phys. Soc. Int. Cfce. Report*, **2**, 109, 1947) on the

heat generated in a pure tin ellipsoid when immersed in an alternating magnetic field of 1460 megacycles per sec. and a peak value of less than 0·37 oersted. The heat generated was measured by the evaporation of liquid helium. The calculated resistance fell at first rapidly and then more slowly as the temperature was lowered through the transition point (3·71° K.), and did not immediately fall to zero as happens for the d.c. resistance. In the newer work in the Mond Laboratory, a resonator constructed of the metal under investigation is excited by a suitable variable-frequency oscillator. The amplitude of the induced currents in the resonator is determined by picking up a fraction of the power in a loop coupled to the resonator and connected through a rectifier to a galvanometer. A measurable galvanometer current is obtained only when the frequency of the oscillator is adjusted to be near the natural frequency, ω_0, of the resonator. A preliminary account of the construction and use of the apparatus is given in the Conference Report already mentioned (*loc. cit.*, **2**, 113, 1947) by A. B. Pippard, and in more detail in the first of a series of papers (A. B. Pippard, *Proc. Roy. Soc.*, **A191**, 370, 1947).

3.—The resistance is calculated from measurements of the band width of the resonator defined as $\Delta\omega$, the width of the resonance curve at one half of the peak power at ω_0. Then the resistance R is proportional to $\Delta\omega/\omega_0 = 1/Q$ (where Q is the selectivity of the resonator). The superconducting wire is a hairpin-shaped loop hung vertically so that at resonance there is a current node at the bottom (open end) and an antinode at the top. The loop is surrounded by a cylindrical sheath, closed at the bottom and immersed in liquid helium. In the first experiments the specimens were tin wires of diameter between 0·07 and 2 mm., the thinner ones being supported in silica capillaries, and mercury in similar tubes. A standard oscillator was used, variable over the range 1100–1250 megacycles per sec. The power dissipated in the superconductor at resonance was insufficient to cause appreciable heating (a rise of temperature of 0·001° C. would have been detected), and the magnetic field due to the current was not large enough to affect the transition temperature. The temperature of the helium bath was altered by controlling the vapour pressure, which was used as a measure of temperature. The Q for metal in the normal state was a few thousand, but in the superconducting state was much larger, up to $5{\cdot}7 \times 10^5$ for one of the tin loops. At such values of Q, a change in vapour pressure of the helium by only 0·1 mm. Hg, produced an appreciable change in dielectric constant of the vapour, and hence in the resonant frequency. It was also most important

to prevent relative movement of the limbs of the resonant loop. To reduce losses in the cylindrical metal sheath it was lined with lead, which is superconducting at the temperatures used. The results for tin are in good agreement with those reported by H. London. Those for mercury are similar but the resistances relative to the normal metal are smaller. There is some evidence that the resistance at these high frequencies is not zero at 0° K. for tin, though it probably is for mercury. The H.F. resistivities of the metals above the transition temperature are greater than predicted from classical skin-effect theory.

4.—This latter point was investigated in greater detail and an account is given in Pippard's second paper (*Proc. Roy. Soc.*, **A191**, 385, 1947). H. London had already observed the anomalously high resistivity of metals at high frequencies and at low temperatures above the transition point, and he attributed the phenomenon to the long mean free path of the electrons compared with the thickness δ of the region carrying the H.F. surface currents. The same method as just described was used for wires of Cu, Ag, Au, Al, Sn and Hg of diameters between 1 and 2 mm. The measurements were carried out at the temperatures of liquid nitrogen (77° K.), hydrogen (14–20° K.) and helium (2–4·2° K.) as well as at room temperatures. The d.c. resistances were measured at room temperature and at 4·2° K., and the values at intermediate temperatures calculated. All the wires were left in the high state of surface polish in which they were prepared (*e.g.* by drawing) to prevent the increase of measured resistance due to surface roughness. The errors due to existing surface irregularities were believed not to exceed 5 per cent. In order to measure the normal skin conductivity of mercury below 4·2° K., a magnetic field was applied to destroy the superconductivity. Although the measurements are for the metals not in a superconducting state, they are important as probably being essential to an understanding of what happens in superconductors below the transition temperature. The results obtained indicate that, at least for Au, Ag and Sn, the H.F. skin conductivity is independent of the bulk (d.c.) conductivity when the electron mean free path is sufficiently long. Pippard has extended London's theory of the H.F. conductivity in cases where the electron mean free path is longer than δ and his conclusion is that the observed results may be explained qualitatively in terms of this relatively long free path. In effect, only a small proportion of the "free" electrons contribute to the H.F. current, those which strike the metal surface at a glancing angle. The theory is not rigorous and may need modification in detail (see below).

5.—In the following paper (*Proc. Roy. Soc.*, **A191**, 399, 1947), Pippard extends the theory to the case of a superconductor, below the transition temperature. Expressions are derived for the skin resistivity R and the skin reactance X, both involving the penetration depth λ, the depth to which a static magnetic field penetrates below the surface of a superconductor. The expression for R is a complex function of λ, but at temperatures below the transition, $X = 4\pi\omega\lambda$. Hence the measurement of X provides a simple method (in principle) of determining λ. The method used by Pippard depends on the velocity of an H.F. wave along a transmission line, which is different from that of an extended wave in the material bounding the line. Absolute measurements of the velocity would be very difficult, so only changes in X are measured. In effect, a change in skin reactance causes a change in resonant frequency of the simple resonators described above. The reactance was measured at each temperature first with the metal in a superconducting state, and then in the normal state with the superconductivity destroyed by a magnetic field of appropriate value. Measurements were made on mercury and tin. For mercury, $\lambda =$ about $7{\cdot}5 \times 10^{-6}$ cm., in good agreement with the result reported by Désirant and Shoenberg (*Nature*, **159**, 201, 1947). The temperature variations of λ for the two metals are recorded. λ increases rapidly with temperature as the transition temperature is approached. Measurements of λ are important in relation to theories of superconductivity, *e.g.* according to F. and H. London (*Proc. Roy. Soc.*, **A149**, 71, 1935),

$$\lambda^2 = \frac{m^* c^2}{4\pi n e^2}$$

where m^* is the effective mass of those electrons taking part in superconductivities, and n the number of them per unit volume.

6.—Désirant and Shoenberg have now published details of their determination of λ (*Proc. Phys. Soc.*, **60**, 413, 1948). Their method depends on the measurement of changes of magnetic susceptibility of thin cylinders with temperature. If χ_0 is the ideal susceptibility of a superconducting cylinder of very large radius, and χ that for a cylinder radius r, then

$$\chi_0 \qquad \frac{2\lambda}{r}$$

and $\chi_0 = 1/4\pi$. If T_1 is some fixed temperature, and $\chi(T_1) - \chi(T) = \Delta\chi$, then

$$\frac{\Delta\chi}{\chi} = \frac{2\Delta\lambda}{r}$$

The delicate methods of measurement of $\Delta\chi$ are described fully in the paper. The resulting values of $\Delta\chi$ for tin and mercury agree fairly well with Pippard's (as already reported above). There is some evidence, however, that χ and λ change with applied magnetic field if the latter is strong enough. To obtain the absolute values of λ and $\Delta\lambda$ for mercury, the measurements of the magnetic properties of colloidal mercury are used (D. Shoenberg, *Proc. Roy. Soc.* **A175,** 49, 1940).

7.—E. Laurmann and D. Shoenberg (*Nature*, **160,** 747, 1947) have repeated an experiment of Casimir's (*Physica*, **7,** 887, 1940) in which the mutual inductance of two coils wound on a superconducting core was measured at various temperatures. From such measurements $\Delta\lambda$ may be deduced, and the results again agree reasonably well with those obtained by the other methods.

8.—More recently, Pippard has extended his measurements to higher frequencies, about 9200 megacycles per sec. (*Nature*, **162,** 68, 1948). The results are not fully in accord with the predictions made by the theory already reported, and imply that certain modifications in detail are necessary.

9.—Mention has already been made of the transition state which occurs when the long axis of a superconducting cylinder (*e.g.*) is perpendicular to a magnetic field, within a certain range of values. In this state, the metal is a mixture of superconducting and normal phases. Detailed work on this state, particularly the relation between the first appearance of resistance and the threshold value of magnetic field, is described in three more papers from the Mond Laboratory (M. Désirant and D. Shoenberg, *Proc. Roy. Soc.*, **A194,** 63, 1948; E. R. Andrew, *ibid.*, 80 and 98).

10.—There has been some discussion, too, on the relationship between the threshold value of the magnetic field parallel to a superconducting wire and the temperature of the wire (J. G. Daunt, *Phys. Rev.*, **72,** 88, 1947; J. de Launay and M. C. Steele, *ibid.*, **73,** 1450, 1948; Daunt, Miller, Pippard and Shoenberg, *ibid.*, **74,** 842, 1948). If $\lambda(0)$ and $\lambda(T)$ are the penetration depths at 0° K. and T° K., and T_c is the transition temperature, the results so far available satisfy the relation

$$\left[\frac{\lambda(0)}{\lambda(T)}\right]^2 = 1 - \left[\frac{T}{T_c}\right]^4$$

This relation and others are consistent with theories postulating two "states" of electrons in superconductors below the transition temperature, one responsible for the superconducting properties and one behaving normally. This is reminiscent of the two-fluid theory

of the properties of He II discussed in the previous article. There have been many attempts to obtain a satisfactory quantitative theory of superconductivity, but a discussion of these attempts would be lengthy at the present stage when there is still much uncertainty. Important papers include F. and H. London, *Proc. Roy. Soc.*, **A149**, 71, 1935; Gorter and Casimir, *Physica*, **1**, 306, 1934; *Phys. Zeits.*, **35**, 963, 1934; M. von Laue: *Theorie der Supraleitung*, 1947; *Z. Phys.*, **124**, 135, 1947; *Phys. Soc. Int. Cfce. Rpt.*, **2**, 90, 1947; W. Heisenberg, *Z.f. Naturkunde*, **2a**, 185, 1947; F. London, *Phys. Rev.*, **74**, 562, 1948; M. Born and K. C. Cheng, *Nature*, **161**, 968 and 1017, 1948.

It may be possible to return to this subject in a later article.

GENERAL AND PHYSICAL CHEMISTRY. By J. W. Smith, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic.

Oxygen Overpotential.—During the past fifty years a very large number of investigations have been made upon the subject of hydrogen overpotential, and many theories have been advanced to explain the origin of this overpotential and its dependence upon the nature of the electrode and electrolyte used, the current density, and other variable factors (Science Progress, 1947, **35**, 675). On the other hand, there is by no means a comparable amount of data on the complementary subject of oxygen overpotential.

It would be anticipated that at an inert electrode in contact with both gaseous oxygen and an aqueous solution a definite equilibrium of the type $O_2 + 2H_2O + 4\varepsilon \rightleftharpoons 4OH'$ should be established, and hence that the potential of such an electrode with respect to the solution should vary with the hydroxyl ion concentration of the solution in a manner analogous to the variation of the potential of a hydrogen electrode with the hydrogen ion concentration of the solution.

Excepting under very special circumstances for which reversible behaviour has been claimed, such a reversible oxygen electrode has not been produced, but the value of its potential can be calculated from free energy data. During electrolysis, however, it is found that, whatever materials are used for the electrodes, the potential of the anode with respect to the solution is always appreciably positive relative to this theoretical reversible oxygen electrode when oxygen is being liberated at the electrode. The difference between the potential of the anode and the reversible oxygen potential, that is the excess potential which has to be applied over the theoretical potential, is called the oxygen overpotential or overvoltage. This subject is of very great practical importance, since

the course and the yield of many anodic oxidation processes are very closely linked with the oxygen overpotential at the electrode.

Early measurements were directed principally towards the determination of the so-called " minimum " oxygen overpotential, that is the least positive potential relative to the theoretical standard potential at which free evolution of oxygen from the electrode could be detected. As a result of such investigations, Coehn and Osaka (*Z. anorg. Chem.*, 1903, **34**, 86) found that the oxygen overpotential was relatively low at nickel and cobalt anodes, but higher at silver, platinum, and gold electrodes. This led to the impression that the metals with a high hydrogen overpotential generally have a low oxygen overpotential and vice versa.

Many difficulties were experienced, however, in obtaining reproducible values of the oxygen overpotentials at definite current densities. One of the first series of precise measurements was that of Bowden (*Proc. Roy. Soc.*, 1929, **A125**, 446; **A126**, 107), who studied the electrodeposition of oxygen at platinum electrodes in acid solutions. He found that, not only did the variation of the overpotential (η) with the current density (I) follow the Tafel relationship $\eta = a + (RT/\alpha F) \ln I$ or $\eta = a + b \log I$, where $b = 2{\cdot}3RT/\alpha F$, as in the case of hydrogen overpotential, but the value of α was again about 0·5, the same value as had been observed for hydrogen.

As a result of this observation, however, there seems to have been rather too great a tendency to regard the phenomenon as completely parallel with that of hydrogen overpotential. This has probably been accentuated by the facts that, apart from some studies on base metal alloys normally used as electrodes in electrolytic cells, the majority of measurements have been made with the one anode material, platinum, and that the marked variation of the measured value of the overpotential with time, though frequently remarked upon, has not always been borne in mind in interpreting the results. Study is complicated, of course, by the fact that there are very few anode materials which are not attacked, particularly in acid solutions. Hence comparisons of the behaviours of different metals have normally to be made with alkaline electrolytes, but even under such circumstances the formation of oxide layers on the surface of the electrode may still occur. Evidence has been obtained by Bowden and others that even in the case of platinum a layer of platinum oxide is formed on the surface of an electrode on which oxygen is being deposited.

Through these circumstances, beyond attempts to treat this phenomenon by analogy with hydrogen overpotential, the theoretical aspects of the subject have been scarcely developed. This

is hardly to be wondered at in view of the fact that the processes involved in passing from the state of hydroxyl ions in the solution to gaseous oxygen must be extremely complex and are little understood. The apparent obeyance of the Tafel relationship, *i.e.* the linear variation of the overpotential with log I, is most simply interpreted as arising from some step for which an activation energy is demanded. If E is the activation energy of this step when the electrode is at its reversible potential, then, when a current is flowing, this energy is partially supplied by the overpotential, and is effectively reduced to $E - \alpha\eta F$, where α is a proportionality constant. Hence the number of molecules reacting per second will be proportional to $e^{-(E-\alpha\eta)F/RT}$, and hence, since this controls the current density, $I = Ae^{-(E-\alpha\eta F)/RT}$, and $\ln I = \alpha\eta F/RT + E/RT + \ln A$. From this it follows that $\eta = (RT/\alpha F) \ln I - E/\alpha F - (RT/\alpha F) \ln A$. As the last two terms are constant at constant temperature this is the Tafel relation. It is evident that from measurements of the overpotential at different temperatures this equation permits the calculation of the energy of activation E.

This does not assist, however, in deciding at which stage the activation energy is required, that is which is the slow rate-determining stage of the discharge process. Following closely on the apparent analogies to hydrogen overpotential, the theories which have been advanced can be classified according to which of the possible stages which may arise during the deposition of the oxygen is regarded as this slow process.

It was originally suggested by Foerster (*Z. Elektrochem.*, 1910, **16**, 353) that this step was the combination of oxygen atoms on the surface of the electrode to form oxygen molecules. This theory is analogous to Tafel's original theory of hydrogen overpotential, and like that theory leads to the Tafel equation, but in this case α should be 4, and not 0·5, as was observed by Bowden. Nevertheless, as will be indicated later, this theory has much to commend itself as compared with some of the other views of the phenomenon.

On the other hand, Newbery (*J. Chem. Soc.*, 1916, **109**, 1359; 1924, **125**, 511) considered that oxygen overpotential was due to the formation of definite higher oxides on the anode, these oxides being generally of types stable only under the high pressures set up within the electrode during electrolysis. According to this theory the oxygen evolution at, for instance, copper, nickel, and platinum anodes occurs through the intermediate formation of Cu_2O_3 and CuO_2, Ni_2O_3 and NiO_2, and PtO_3 and PtO_4, respectively. Although, of course, such oxides as PbO_2 are known to be formed during anodic treatment of certain metals, and observation of the dis-

charge potential of various anodes after the polarising current has been interrupted suggests that oxides are frequently present on the surface of the anode, it does not follow that they are the cause of overpotential. At relatively high current densities the anode potential observed may be actually that of an unstable oxide, but this would not account for all the phenomena observed.

In a different approach, based on quantum mechanical arguments, Gurney (*Proc. Roy. Soc.*, 1931, **A134**, 137; 1932, **A136**, 378) has treated oxygen overpotential by the method used in the case of hydrogen overpotential, supposing the transfer of electrons to occur between hydrated oxygen on hydroxyl ions in the solution and unoccupied levels in the electrode metal. As the potential is built up at the electrode their energy is increased until the electron can pass from the ion to the metal. This theory leads to a relationship similar to that derived for hydrogen overpotential in which the value of α can be less than unity.

A theory in which the hurdle is regarded as arising prior to the discharge of the ions has been developed by Eyring, Glasstone, and Laidler (*Trans. Electrochem. Soc.*, 1939, **76**, 331; *J. Chem. Physics*, 1939, **7**, 1053). They suggest that in aqueous solutions layers of water molecules become attached to the electrodes, and corresponding adjacent layers are associated with the solvent. Oxygen overpotential is attributed to the slow transfer of protons from a water molecule in the anode layer to a water molecule or other proton acceptor in the solvent. As the water molecule is regarded as existing on the electrode surface as H and OH, the discharge process can be pictured as a transfer of electrons from the hydrogen atoms to the metal to yield protons. The hydroxyl radicals left on the electrode by this discharge process react in pairs with the ultimate formation of oxygen gas. As the attachment of the water molecules to the polarised electrode is associated with the adsorption of protons, this theory accounts for the observation that metals such as platinum, palladium, and gold, which adsorb hydrogen strongly and hence do not permit the ready transfer of protons to molecules in the solution, have high oxygen overpotentials.

Finally, it was suggested by Bowden that the oxygen initially evolved is adsorbed on the electrode surface and deformed, yielding oriented electric dipoles, the potential difference across the interface being controlled by the number and moment of these dipoles. Further changes in the potential with the establishment of the overpotential are regarded as due to the increased deformation of the dipoles, with increase in electric moment. The actual evolution of oxygen is regarded as a surface reaction involving the evaporation

of these adsorbed gas dipoles from the electrode to form neutral molecules. As only the dipoles with a certain energy of activation can leave the surface, the usual Tafel type of logarithmic relationship between the overpotential and the current density follows once again.

In so far as observations on platinum surfaces are concerned, Hoar (*Proc. Roy. Soc.*, 1933, **A142**, 628) found that the Tafel relation also held for the liberation of oxygen in alkaline solutions and for the reverse process, the electrolytic dissolution of oxygen, although α had not always the value 0·5. In a further extension of this work, Bowden and Keenan found that α was about 0·54 in very dilute alkaline solutions, but increased steadily with increasing hydroxyl ion concentration, becoming 1·74 in 14N-sodium hydroxide solution. In sulphuric acid solutions the overpotential was independent both of the concentrations of the hydrogen and sulphate ions, but in sodium hydroxide solutions it decreased with increasing hydroxyl ion concentration (Bowden and Agar, *Ann. Rep. Chem. Soc.*, 1938, 102).

In view of these facts, Stout has suggested (*Discussions Faraday Soc.*, 1947, **1**, 246) that the rate-determining process may be different in acid and alkaline solutions. From his measurements of the temperature coefficient of the overpotential in acid solutions Bowden had found the energy of activation for the deposition from 0·2N-sulphuric acid at the reversible potential to be 18·7 kg.-cal. per mol. By similar measurements Stout has now shown that the energy of activation in alkaline solution is 25·3 kg.-cal., thereby confirming his suggestion.

Very useful information on this subject has also been furnished by measurements of the rate of establishment of oxygen overpotential at inert electrodes and the quantity of current required for this purpose. Bowden found that at platinum electrodes in acid solutions $3{\cdot}8 \times 10^{-4}$ coulombs per volt per cm.^{2} were required to change the potential around the oxygen overpotential. If the real area of the electrode is 3·3 times its apparent area the capacity becomes $1{\cdot}1 \times 10^{-4}$ coulombs per volt per cm.^{2}

This phenomenon was studied in detail by Butler and his co-workers (*Proc. Roy. Soc.*, 1932, **A137**, 604; 1933, **A143**, 89), who found that if the potential of a platinum electrode were about + 0·6 volt with respect to the standard hydrogen electrode, then, when an e.m.f. was applied, the potential changed relatively slowly to the oxygen overpotential value, about 9×10^{-4} coulombs per cm.^{2} being required in all to establish this potential. If the current were then reversed, the potential reverted, at first linearly and much

more rapidly, and if it were not allowed to become less than + 0·9 volt with respect to the hydrogen electrode before being made an anode again, the potential rose once more at the same rapid rate to the oxygen discharge potential. The quantity of current required for the original processes corresponded approximately to that needed to liberate sufficient oxygen to give a single layer of adsorbed oxygen atoms over the electrode surface. They suggested, therefore, that a layer of adsorbed oxygen is formed before the oxygen overpotential is reached. So long as this is not destroyed the quantities of current required to change the potential corresponded to the double layer capacity of the electrode. If, however, the electrode is cathodically polarised, this oxygen layer is destroyed and an anodic polarisation curve corresponding to the greater quantity of current is again obtained.

Butler and Drever (*Trans. Faraday Soc.*, 1936, **32**, 427) obtained evidence that palladium, rhodium, and indium also give an adsorbed layer of oxygen prior to the establishment of the oxygen overvoltage.

Results in close accord with these were also obtained by Hickling (*Trans. Faraday Soc.*, 1945, **41**, 333), who studied the change in electrode potential during the anodic polarisation of a smooth platinum electrode in acid, neutral, and alkaline electrolytes. In each case the potential showed an initial and somewhat irregular rise, followed by a rapid linear rise and then a slow and almost linear rise, ending with oxygen evolution. The initial stage was attributed to ionisation of adsorbed hydrogen, and was eliminated in the presence of catalytic poisons such as cyanogen or carbon disulphide. The second stage was regarded as corresponding to the charging of a double layer, and the third to the deposition of oxygen. The potential at the beginning of this last stage corresponded closely in all three solutions to that of PtO, and it was suggested that in the process an unimolecular layer of this oxide is formed on the electrode. In the case of gold electrodes the initial stage was found to be absent, and the last slow increase was intrepreted as corresponding with the formation of a layer of Au_2O_3 on the surface of the gold.

With regard to the variation of the oxygen overpotential with the anode metal, in addition to the observations of Coehn and Osaka, early measurements along these lines were also made by Knobel, Caplan, and Eiseman (*Trans. Amer. Electrochem. Soc.*, 1923, **43**, 55), who studied several metals in N-potassium hydroxide solution over fairly wide ranges of current density. They allowed, however, only an arbitrary time of one minute at each current density before making their measurements, and hence

in some cases their figures differ appreciably from the final steady values which would have been attained. Garrison and Lilly (*ibid.*, 1934, **65**, 171) made similar measurements with polished metal electrodes in 0·1N-sodium hydroxide solution. They claimed that a steady representative value of the oxygen overpotential could be obtained by approach from either higher or lower current densities, and for a given metal the overpotential was linear with current density, that is $\eta = \eta_0 + kI$, where k had the same value for all the metals studied. The values of η_0, the overpotential at zero current density, increased in the order nickel, cobalt, iron, iridium, platinum, and gold. When the same metal surfaces were cathodically polarised whilst air was bubbled around them, the electrode potential was also a linear function of current density, and its slope k_0 was interpreted as a measure of the catalytic effect of the metal on the ionisation of oxygen. For the various metals k_0 was found to increase in the reverse order to that for η_0, and log k_0 was a linear function of η_0, as would be expected from the theory that the overpotential is due to the slow combination of oxygen atoms on the surface of the electrode.

The dependence of the oxygen overpotential on the nature of the electrode material and on current density has been reinvestigated recently by Hickling and Hill (*Discussions Faraday Soc.*, 1947, **1**, 238) utilising N-potassium hydroxide solution as electrolyte. They found that in nearly all cases an appreciable time was required for the overpotential equilibrium to be set up. They also confirmed that the oxygen overpotentials are considerable, and appreciably higher than corresponding hydrogen overpotentials; thus at a current density of 10^{-5} amp. per cm.^{2} none of the electrode materials studied showed an overpotential of less than 0·2 volt, whilst at 1 amp. per cm.^{2} the values for all electrodes except cobalt, iron, and copper were well over 1 volt, and at gold, graphite, and platinum reached very high values. The dependence of the overpotential on current density varied so much with the different electrode materials that it was impossible to arrange them in any order which had a real significance. Over limited ranges of current density the variations for cobalt, iron, nickel, platinum, platinised platinum, palladium, silver, and copper all showed agreement with equations of the general type $\eta = a + b \log I$, but at lower current densities there seemed to be a general tendency for the overpotential to approach constant minimum values. At high-current densities the overpotential of platinum also showed a tendency to approach a constant maximum value. Certain of the η — log I curves, however, showed a striking stepwise character. At about 10^{-3} amp.

per cm.[2] the overpotential at palladium changed abruptly by nearly 0·4 volt, and the η — log I graph formed two approximately straight lines joined by an almost vertical line. Gold and graphite anodes led also to stepped curves, but in these cases the steps, although quite definite, were less abrupt and it was possible to obtain points on the steep section of the curve. With silver and copper electrodes there were signs of steps at very low current densities. It was suggested that these steps are due to changes in the surfaces of the anodes, which were readily observable in the cases of palladium and gold after use at high current densities.

It was inferred, therefore, that the general Tafel relation $\eta = a + b \log I$ has only a limited application to oxygen overpotentials in alkaline solutions. In the cases where it does apply, the values of the constant b were found to vary from 0·07 to 0·30, as indicated in the table. Hickling and Hill note that none of these values bears any simple relation to the quantity $2{\cdot}3 \times 2RT/F$ (0·12 at 20° C.).

Anode Material.	b	Anode Material.	b
Iron	0·07	Nickel	0·15
Cobalt	0·07	Silver	0·15
Copper	0·07	Platinised Pt	0·24
Palladium (low c.d.)	0·08	Pt	0·30
Palladium (high c.d.)	0·13		

Four different types of behaviour could be distinguished in the manner in which the steady value of oxygen overvoltage was attained. The most common type, exemplified by platinised platinum, was that the overvoltage rose from the start of the polarisation, rapidly at first and then more slowly, finally approaching a steady value after a period which varied from about 30 minutes to 4 hours and which was the greater the lower the current density. Less frequently the overvoltage was initially higher than the final value, to which it decreased in a short time; this behaviour was shown particularly by lead at higher current densities. In other cases, where the electrode could show more than one overpotential, as for instance with palladium and gold, the variation of the overpotential with time was very prolonged in the region of the critical value. Finally, when the electrode was one which was attacked and dissolved, the potential was initially very negative, but after a time passivity set in and the anodic behaviour then fell into one of the previous types. This was the course shown by lead and copper at low current densities.

In another important approach to the problem, Bockris (*ibid.*, p. 229) has studied the oxygen overpotential on smooth platinum surfaces at 10^{-3} to 10^{-1} amp. per cm.^{2} in molar solutions of sulphuric acid in acetic acid-water and dioxan-water mixtures at room temperature. He finds that the addition of either non-aqueous component causes the overpotential to increase, rapidly at first and then more slowly, towards an apparent maximum in the case of dioxan. This increment was the same at all current densities within the range studied. The rate of build-up of the oxygen overpotential was shown to be analogous to that of the hydrogen overpotential, and to be similar qualitatively in both aqueous and non-aqueous solutions, although equilibrium was attained more rapidly in the latter. At the lower current densities the Tafel equation applied in all systems, slight negative deviations occurring especially with water and aqueous dioxan at higher current densities. The value of the factor b was increased by the addition of either non-aqueous solvent from about 0·16 to 0·35 and 0·27 in solutions of acetic acid and dioxan, respectively.

These results are regarded by Bockris as contrary to the quantum mechanical theory, according to which the overpotential should be either independent of the solvent or dependent upon the solvation energy of the hydroxyl ions, according as to whether Gurney's interphase potential is taken as the metal solution potential difference at the anode or the anodic potential. The variation of the solvent effect with current density in the case of acetic acid solutions does not support a simple dependence on solvation energy, which probably differs little from that in pure water.

On the basis of Eyring, Glasstone, and Laidler's view the addition of non-aqueous solvents would be expected to decrease the energy of proton transfer at the surface and to weaken the force between the metal and proton owing to adsorption of the non-aqueous component on the electrode. Both effects would lead to a decrease of overpotential upon addition of non-aqueous component, in contradiction to the results observed.

Bockris suggests tentatively that his results support the catalytic mechanism for oxygen overpotential. If it is assumed that the slow stage is the combination of oxygen atoms on the surface of the electrode, and that solvent molecules adhere to the surface by specific adsorptive or electrostatic forces, the addition of acetic acid or dioxan should cause replacement of some water molecules on the anode surface, so that the fraction of the electrode surface available for oxygen atom combination is reduced. As long as no complex interactions between the adsorbed non-aqueous component

and water occur, this would be expected to cause an increase in the overpotential, which would continue with increased additions of the non-aqueous component until the anode surface is largely saturated with the non-aqueous component. This concept of adsorption of a comparatively large molecule on the anode leads to a Tafel equation of increased slope, as was observed.

BIOCHEMISTRY.

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Some Reactions and Enzymes concerned in Glycolysis

It is now some fifteen years since the essential pattern of the course of an anaerobic carbohydrate breakdown in muscle and yeast was elucidated by the pioneer work of Emden and Meyerhof. The scheme of consecutive and partly interlocking chemical reactions suggested by these authors and summarised in an earlier number of this journal (Science Progress, 1935, **30**, 295) has been elaborated in detail, but its essential features have been confirmed by a large amount of later investigation. About a dozen distinct reactions are recognised and for convenience these are summarised in Table I. Each of these is a relatively simple reversible chemical reaction, promoted by a specific enzyme present in the living cells. The reversibility of the separate reactions ensures that the system as a whole is reversible, so it provides a mechanism for the synthesis of carbohydrate from lactic or pyruvic acid as well as for its breakdown.

To illustrate the general nature of the individual reactions, we may briefly survey a few of the earlier stages in the breakdown of glucose, starch or glycogen. All of these are converted into glucose-6-phosphate. In the case of starch, or glycogen, this conversion takes place in two stages: the polysaccharide first acquires inorganic phosphate to form glucose-1-phosphate, and this compound undergoes an isomeric change to give glucose-6-phosphate. Both these reactions are reversible and are promoted by the enzymes phosphorylase and phosphoglucomutase respectively. Similarly, the first step in the breakdown of glucose is its conversion to glucose-6-phosphate, but here the situation is slightly different. To produce the ester linkage from glucose and inorganic phosphate, a certain amount of energy is needed, and in practice phosphorylation is effected by adenosine-triphosphate (A.T.P.), which may be regarded as containing an energy-rich phosphate group, and which in the presence of the enzyme hexokinase converts glucose to glucose-6-phosphate.

Two more reactions lead us from glucose-6-phosphate to fructose-1 : 6-diphosphate (hexose diphosphate), the famous ester isolated by

TABLE I

Reactions and Enzymes of Glycolysis

Reaction No.	Reaction.	Enzyme.
I .	Starch or Glycogen + Phosphate ⇅ Glucose-1-phosphate	Phosphorylase
II .	Glucose-1-phosphate ⇅ Glucose-6-phosphate	Phosphoglucomutase
III .	Glucose + A.T.P. ⇅ Glucose-6-phosphate + A.D.P.	Hexokinase
IV .	Glucose-6-phosphate ⇅ Fructose-6-phosphate	Phosphohexose isomerase
V .	Fructose-6-phosphate + A.T.P. ⇅ Fructose-1:6-diphosphate + A.D.P.	Phosphohexokinase
VI .	Fructose-1:6-diphosphate ⇅ Glyceraldehyde-3-phosphate + dihydroxyacetone phosphate	Zymohexase (Aldolase)
VII .	Dihydroxyacetone phosphate ⇅ Glyceraldehyde phosphate	Triosphosphate isomerase
VIII .	Glyceraldehyde-3-phosphate + phosphate + D.P.N. ⇅ 1:3-Diphosphoglyceric acid + D.P.N. (H_2)	Phosphoglyceraldehyde dehydrogenase
IX .	1·3-Diphosphoglyceric acid + A.D.P. ⇅ 3-phosphoglyceric acid + A.T.P.	Diphosphoglyceric dephosphorylase
X .	3-Phosphoglyceric acid ⇅ 2-Phosphoglyceric acid	Triosomutase (Phosphoglyceromutase)
XI .	2-Phosphoglyceric acid ⇅ 2-Phosphoenolpyruvic acid	Enolase
XII .	2-Phosphoenol pyruvic acid + A.D.P. ⇅ Pyruvic acid + A.T.P.	Phosphopyruvate dephosphorylase
XIII .	Pyruvic acid → acetaldehyde + carbon dioxide	Carboxylase
XIV .	Acetaldehyde + D.P.N. (H_2) → Alcohol + D.P.N.	Alcohol dehydrogenase
XV .	Pyruvic acid + D.P.N. (H_2) → Lactic acid + D.P.N.	Lactic acid dehydrogenase

A.D.P. = Adenosine diphosphate.
A.T.P. = Adenosine triphosphate.
D.P.N. = Diphosphopyridine nucleotide.
D.P.N. (H_2) = Dihydro-derivative of D.P.N.

Note: Reactions III–XIV are involved in yeast fermentation of glucose; Reactions I, II, IV–XII and XV in muscle glycolysis.

Harden and Young from the products of the fermentation which is induced by the action of cell-free yeast juice, zymase, on a mixture of glucose and inorganic phosphate. The Harden and Young ester is a derivative of fructose, and we know now that in its formation glucose-6-phosphate is first converted into fructose-6-phosphate under the action of an enzyme, phosphohexose isomerase, and that further esterification of this fructose-6-phosphate then takes place in presence of the enzyme phosphohexokinase, adenosine triphosphate again supplying the phosphate group.

All the reactions so far mentioned are, in a sense, of a preliminary character, and none of them involves any real breakdown of the sugar molecule. It is only at the next step that the first splitting of the molecule of glucose takes place. Under the influence of zymohexase, often called Aldolase, the hexose-diphosphate breaks up into two molecules of triose phosphate. One of these is dihydroxyacetone phosphate, the other is glyceraldehyde-3-phosphate. These two isomeric three-carbon atom compounds are themselves in equilibrium with each other in presence of the enzyme triose-isomerase. The dihydroxyacetone phosphate does not itself participate directly in subsequent degradation reactions, but only after it has been first transformed into glyceraldehyde-3-phosphate.

The next step, the last to which we shall refer here in detail, is a somewhat complicated one. It is the first which involves oxidation, or, more properly speaking, dehydrogenation. In the presence of inorganic phosphate, and under the influence of the enzyme glyceraldehyde-3-phosphate dehydrogenase (briefly, triose phosphate dehydrogenase), glyceraldehyde-3-phosphate (I) is converted into diphosphoglyceric acid (II). The two hydrogen atoms

$$Na_2PO_3.OCH_2.CHOH.CHO \quad \text{(I)} \qquad Na_2PO_3.OCH_2.CHOH.COO.PO_3Na_2 \quad \text{(II)}$$

$$\underset{\text{(III)}}{Na_2PO_3.OCH_2.CHOH.\underset{\displaystyle OH}{\underset{|}{C}}H.O.PO_3Na_2} \qquad \underset{\text{(IV)}}{H_2PO_3.OCH_2.CHOH.COOH}$$

lost by the triose are transferred to a molecule of diphosphopyridine nucleotide, otherwise known as D.P.N. or coözymase, to form reduced D.P.N. The details of the reaction are not quite clear (*cf.* Meyerhof and Oesper, *J. Biol. Chem.*, 1947, **170**, 1), but the inorganic phosphate may be imagined as forming, under the action of the enzyme, a readily reversible addition product of the type (III) with the glyceraldehyde-3-phosphate, which then loses two hydrogen atoms to form (II). If the glyceraldehyde-3-phosphate simply underwent oxidation to form a salt of phosphoglyceric acid (IV) the reaction

would involve the loss of a considerable quantity of energy, and would be irreversible. Instead of phosphoglyceric acid, the compound actually formed is a derivative in which a second phosphate group is united to the carboxyl group. Such phosphocarboxyl configurations are of relatively high energy content ; in other words, the second phosphoric acid group in diphosphoglyceric acid involves an energy-rich-phosphate bond. In this way, the energy liberated by the dehydrogenation is not lost, but trapped in the new compound. This has two consequences. The first is that the reaction in question is reversible, the initial compounds, glyceraldehyde-3-phosphate and inorganic phosphate, and the product, diphosphoglyceric acid, involving nearly equal free energies. The second is that the free energy liberated by the dehydrogenation process, instead of being lost, can be used for bringing about reactions requiring extra energy. In the next stage, in fact, phosphoglyceric acid monophosphate is formed by a process of dephosphorylation, but the phosphate group, instead of being split off as free phosphate, and the energy lost, is transferred to adenosine diphosphate (A.D.P.) to form adenosine triphosphate (A.T.P.), which, as we have seen, is the high-energy phosphate-containing compound which brings about phosphorylations and other processes requiring energy.

So far we have mentioned some eight enzymes, and it is an indication of the extremely rapid advance of biochemistry that during the past ten years five of these (Table I : I, III, VI, VII, VIII) have been isolated as crystalline proteins.

CRYSTALLINE ALDOLASE AND GLYCERALDEHYDE-3-PHOSPHATE DEHYDROGENASE.—Two of these enzymes, aldolase and triosephosphate dehydrogenase, have recently been investigated in some detail in the laboratories of Professor C. F. Cori (Taylor, Green, Cori, *J. Biol. Chem.*, 1948, **173**, 591 ; Cori, Slein, Cori, *ibid.*, 605). As will be recalled, Professor Cori and his wife, Gertie T. Cori, lately shared a Nobel prize for their outstanding work on carbohydrate metabolism, extending over more than twenty years. Some of their investigations on the biosynthesis of starch and glycogen were recently reviewed in another article of the present series (SCIENCE PROGRESS, 1946, **136**, 779). Their researches on aldolase and triosephosphate dehydrogenase constitute but a small fraction of their very extensive investigations on carbohydrate metabolism.

The enzyme aldolase, whose properties were first described by Meyerhof and Lohmann (*Biochem. Z.*, 1934, **271**, 89, 273, 413), was first crystallised by Warburg and Christian (*Biochem. Z.*, 1943, **314**, 149), who prepared the enzyme from rat's muscle. The same

workers achieved the crystallisation of triosephosphate dehydrogenase from yeast (*Biochem. Z.*, 1939, **303**, 40) and in a short note Cori, Slein and Cori (*J. Biol. Chem.*, 1945, **159**, 565) first described the crystallisation of this enzyme from rabbit muscle. In the same year, Dixon and Caputto (*Nature*, 1945, **156**, 630) also crystallised muscle triosephosphate dehydrogenase.

Both aldolase and triosephosphate dehydrogenase are present in an aqueous extract of finely minced rabbit muscle; but if it is desired to isolate the dehydrogenase as well as the aldolase, it is highly advantageous to extract with 0·03N-sodium or potassium hydroxide, as the yield of dehydrogenase is then increased several fold. The general process used to obtain aldolase was to bring the extract first to 0·5 per cent. saturation with ammonium sulphate at *p*H 7·4–7·6 and then remove the precipitated inactive proteins; sufficient ammonium sulphate was then added to bring the degree of saturation to 0·52. After some hours at 0° C., the aldolase separated as fine needles, which on recrystallisation sometimes separated as needles and sometimes as large, well-formed, elongated hexagonal plates. On recrystallisation at *p*H 5·8 the enzyme was obtained as hexagonal bipyramids. All three forms possessed approximately equal enzymatic activity, and this activity did not appreciably change on interconversion. Crystalline aldolase was also obtained from rat muscle and the aldolase activity of this preparation was the same as that of the rabbit protein. However, its isoelectric point (in phosphate buffer of ionic strength 0·1) was different from that of rabbit aldolase, being 5·7 instead of 6·05. The turnover number of crystalline aldolase corresponded to the splitting of 1670 g. mols of fructose diphosphate by 150,000 grams of protein (approx 1 g. mol.) in one minute at 30° C. and *p*H 7·6. The yield of the first crystals of aldolase corresponded to about 75 per cent. of the aldolase activity in the crude aqueous extract and amounted to about 10 per cent. of the extracted proteins or 0·35 g. per 100 g. of muscle.

To obtain triosephosphate dehydrogenase, the mother liquors from which the aldolase has separated are brought to 0·72 saturation by the addition of more ammonium sulphate. Any precipitate is removed, and the *p*H is raised to 8·2 by the addition of ammonia. After some time in the cold, crystals of the enzyme separate in hexagonal plates, which may be recrystallised from 0·66 saturated ammonium sulphate at *p*H 8·2. The yield amounts to 7–12 per cent. of the extracted protein corresponding to about 0·3 per 100 g. of muscle.

The amino acid composition of aldolase and of triosephosphate

dehydrogenase from rabbit muscle has been investigated by Vellick and Ronzoni (*J. Biol. Chem.*, 1948, **173**, 627), who employed microbiological and chemical methods. They conclude that the total *N* content can be accounted for by eighteen amino acids, along with, in the case of the dehydrogenase, diphosphopyridine nucleotide (D.P.N.), to which we shall refer again in a moment. As compared with the structural protein myosin, the glutamic acid contents of these two enzymes are relatively low. Amongst the differences between aldolase and dehydrogenase, it may be noted that methionine is about twice as abundant in dehydrogenase as in aldolase. Aldolase, probably identical with the myogen A of Gralen (*Biochem. J.*, 1939, **33**, 1342), has a molecular weight of about 140,000, whilst the molecular weight of dehydrogenase is probably very close to 100,000.

TRIOSE PHOSPHATE DEHYDROGENASE; ITS PROSTHETIC GROUP.—Triosephosphate dehydrogenase is an especially interesting enzyme, not only because of the key position it occupies in the process of glycolysis in yeast or muscle, but also because of its characteristic properties. The active enzyme contains free —SH groups, and if oxidation takes place, as readily happens on exposure to air with conversion of the —SH groups to —S.S—, then the enzyme is inactivated. For full activity of the enzyme, it is necessary to prevent this oxidation, and measurements of its activity are consequently carried out in buffer mixtures containing cysteine. For the same reason the dehydrogenase is very sensitive to the presence of iodoacetate, for this reagent can combine with the —SH groups and so inactivate the enzyme. Indeed, the well-known inhibition of lactic acid fermentation in muscle and of alcoholic fermentation in yeast, when low concentrations of iodoacetate are used, is at the point controlled by triosephosphate dehydrogenase.

Another interesting feature of this enzyme is that it contains a prosthetic group. This was suspected on the basis of the ultra-violet absorption of solutions of the crystalline enzyme, and careful investigation by Taylor, Vellick, Cori, Cori and Slein (*J. Biol. Chem.*, 1948, **184**, 619) has furnished conclusive proof of the presence of diphosphopyridine nucleotide (D.P.N.), (co-zymase). As will be recalled, the enzyme promotes the transfer of two atoms of hydrogen from glyceraldehyde-3-phosphate to D.P.N., and so the prosthetic group is in fact one of its own substrates. One mol. of D.P.N. is present in about 50,000 g. of the protein; as the molecular weight is probably about 100,000 there are evidently two prosthetic groups per enzyme molecule. The D.P.N. is firmly attached to the protein,

for dialysis overnight, between pH 5·3 and 9, fails to remove it, though it does largely inactivate the enzyme. The content of D.P.N. is not increased when the crystals are recrystallised from a solution containing free D.P.N. However, treatment of the enzyme with norite or with intestinal phosphatase removes the D.P.N. without inactivating the enzyme, but the latter no longer crystallises. In connection with this last observation, it should be remembered that tests for activity are carried out in solutions containing D.P.N. as substrate, and so the protein can regain its missing prosthetic group in the course of the test. It is found that, when the non-crystalline enzyme resulting from norite treatment is supplied with D.P.N., it is reconverted to a crystalline enzyme containing the original proportion of D.P.N. to protein.

The turnover number of triosephosphate dehydrogenase for 100,000 g. of protein (1 g. mol.) corresponds to a reduction of 6700 g. mols. of D.P.N. per minute in a cysteine-containing buffer solution at pH 8·6 and 27° C. The turnover number is the same at pH 9, but at pH 8·1, 7·7 and 7·1 it is 81, 57 and 13 per cent. respectively of the maximum. It will be seen that the turnover number at the pH of the muscle is considerably lower than the maximum. The relatively high concentration of this enzyme in muscle may be related to its somewhat low catalytic activity in the conditions prevailing there. Yeast triosephosphate dehydrogenase has approximately the same activity as the muscle enzyme and, like the latter, requires the presence of cysteine in the buffer solution for its full activity.

Effect of Arsenate.—In investigating the amount of D.P.N. present in the triosephosphate dehydrogenase, Taylor, Vellick, Cori, Cori and Slein (*loc. cit.*) make use of the fact that, if in the reaction inorganic phosphate is replaced by arsenate, the dehydrogenation of the glyceraldehyde phosphate with simultaneous reduction of the D.P.N. is an irreversible reaction, the product in these circumstances being monophosphoglyceric acid. This device allows complete reduction of the prosthetic group to be achieved, and so the amount present can then be readily determined by measuring the light absorption of the reduced co-enzyme.

It is not difficult to make a guess as to the explanation of this curious effect of arsenate. Evidently arsenate resembles phosphate sufficiently closely to enable it to take the place of phosphate in the enzymic reaction. The expected product would be the arseno-3-phosphoglyceric acid (v). However, this is apparently much more

$$Na_2PO_3.OCH_2.CHOH.COOAsO_3Na_2$$

(V)

unstable than the diphospho analogue (1 : 3-diphosphoglyceric acid (II)) and spontaneous decomposition to arsenic acid and phosphoglyceric acid (IV) takes place.

YEAST FERMENTATION AND THE HARDEN–YOUNG REACTION.—As is well known, when glucose is fermented by cell-free yeast juice (zymase) the progress of the reaction in some ways differs sharply from that obtained with whole yeast. In both cases, glucose is converted into alcohol and carbon dioxide, but, whereas with whole yeast the reaction is approximately that expressed by the equation

$$C_6H_{12}O_6 = 2C_2H_5OH + 2CO_2$$

with the cell-free yeast juice rapid fermentation proceeds only in presence of inorganic phosphate, which is esterified in the course of the reaction. The process is expressed by the famous equation of Harden and Young:

$$2C_6H_{12}O_6 + 2Na_2HPO_4 = 2C_2H_5OH + 2CO_2 + C_6H_{10}O_4(PO_3Na_2)_2 + 2H_2O,$$

and the first rapid phase of the reaction comes to an end when the supply of either glucose or of inorganic phosphate has been exhausted. The hexose diphosphate (the so-called Harden and Young ester) accumulates, and during the second phase of the reaction undergoes a process of slow fermentation, the rate of production of alcohol and carbon dioxide being only a small fraction of that realised during the first phase. Not only is there no accumulation of hexose diphosphate when whole yeast is used, but whole yeast ferments hexose diphosphate freely when this is supplied as substrate.

The reason for this curious difference in fermentation by whole yeast and by cell free yeast juice has attracted much interest for many years and has recently been elucidated by Meyerhof (*J. Biol. Chem.*, 1945, **157**, 105). Let us first consider the cell-free juice reaction, which essentially consists of the series of enzymes (III to XIV) listed in Table I, along with the phosphate-transporting co-enzyme, adenylic acid (in its forms of adenosine diphosphate, A.D.P., and adenosine triphosphate, A.T.P.) and the hydrogen transporting co-enzyme, co-zymase (D.P.N.). The key to the Harden and Young reaction is to be found in the peculiarity of reaction (VIII), (Table I), the step we have already discussed at some length. As we have seen, the oxidation of glyceraldehyde-3-phosphate is accompanied by the uptake of an extra molecule of inorganic phosphate. The 1:3-diphosphoglyceric acid which is formed must lose the extra molecule of phosphate and, to do so, it transfers it to A.D.P., so converting the latter into A.T.P. (Reaction IX). In the absence of available A.D.P. (or adenylic acid) the diphosphoglyceric acid

accumulates and so normal fermentation is held up. Now, as we have seen, A.D.P. is regenerated from A.T.P. when, in the early steps of glucose fermentation (Reactions III and V), phosphate is transferred first to glucose and then to hexose monophosphate, to form hexose diphosphate. The fact that a new molecule of inorganic phosphate is taken up for every molecule of triosephosphate which is dehydrogenated means that, for every molecule of glucose which is fermented to alcohol and carbon dioxide, two fresh molecules of phosphate are converted from the inorganic to the organic form. These extra molecules are passed on through A.T.P. to glucose to form extra hexose diphosphate. When the supply of glucose comes to an end, A.D.P. is no longer regenerated from A.T.P., and once the supply of A.D.P. is exhausted the whole process of fermentation is held up at the diphosphoglyceric acid stage. The reactions all being reversible, this means that all the earlier stages also come to a standstill. If the inorganic phosphate supply gives out before the glucose is exhausted, then it will be the step which involves the uptake of phosphate that will be blocked; triosephosphate, no longer able to be oxidised in the absence of phosphate, will accumulate and hold up all earlier stages.

This picture affords an adequate explanation of the Harden and Young equation, and of the accumulation of hexose diphosphate. Why then does not this accumulation of hexose diphosphate occur when the fermentation is effected by whole yeast cells? Meyerhof has given the answer (*loc. cit.*). Yeast contains an enzyme called apyrase (adenosine triphosphatase or adenosine pyrophosphatase) which splits phosphate from A.T.P., so that the phosphate can be liberated again in the inorganic form instead of being transferred to glucose or some other phosphate acceptor. Thus in the yeast cell, with its abundant supply of apyrase, there is no accumulation of hexose diphosphate; inorganic phosphate is regenerated as required and all the glucose is smoothly fermented to alcohol and carbon dioxide. Now apyrase is a somewhat unstable enzyme and, besides, tends to be firmly attached to the cell substance. Consequently, little of it is found in the cell-free yeast juice. The result is that the fermentation produced by the latter proceeds in the presence of minimum apyrase concentrations, and the result is the Harden–Young reaction.

It may be remarked that Meyerhof and his co-workers (*Archives Biochem.*, 1947, **12**, 405; **13**, 485; **14**, 71) have found that brain homogenates and brain extracts differ in much the same way as yeast cells and cell-free zymase; the former contains apyrase and the latter little or none. In this way the rather marked differences

in the course of fermentation by brain homogenates and brain extracts can be explained.

Effect of Arsenate on the Harden and Young Reaction.—It has long been known that the presence of arsenate very greatly increases the power of cell-free yeast juice to ferment hexose diphosphate. Harden and Young thought that this was due to the presence in the zymase of a hexose diphosphatase, the activity of which was much increased by arsenate. This explanation has been shown to be untenable, and the curious effect of arsenate in the Harden and Young reaction has been traced to the remarkable action of arsenate in the oxidation of triosephosphate. As we have already seen, arsenate can replace phosphate in the sense of allowing the dehydrogenation to proceed, but the product is no longer diphosphoglycerate but monophosphoglycerate. Thus when arsenate replaces glucose, there is no uptake of the inorganic constituent, and the whole process of glucose fermentation is cyclic as far as the inorganic components of the system are concerned.

It is evident, then, that it is not difficult to account for the fact that, in the presence of arsenate, glucose is fermented by cell-free yeast juice without the accumulation of hexose diphosphate. However, to explain the rapid fermentation of pre-existing hexose diphosphate, something more is required. Here, there is an actual setting free of inorganic phosphate, and at first sight it looks as if some second effect of arsenate must be involved. However, Meyerhof and Jonowicz-Kocholaty have shown that the system of reversible reactions outlined in Table I is such that the arsenate effect on Reaction VIII is sufficient to account, not only for the ability of the process to proceed without uptake of phosphate, but also for the fermentation of hexose diphosphate with liberation of phosphate.

As will be seen from Table I (XII) the phosphate, originally in the diphosphate, ultimately finds itself in the form of phosphopyruvic acid, and the problem is to explain the liberation of this phosphate as inorganic phosphate. It must first be transferred to A.D.P. by Reaction XIII, and so the problem is to find a dephosphorylating mechanism for A.T.P. in the absence of apyrase. Careful consideration shows that, in the presence of arsenate, this is actually provided by Reactions VIII and IX. In the absence of arsenate, we would have an equilibrium mixture of the five compounds, glyceraldehyde-3-phosphate (I), diphosphoglyceric acid (II), 3-phosphoglyceric acid (IV), A.T.P. and A.D.P. Now, the presence of arsenate allows of the irreversible oxidation of glyceraldehyde-3-phosphate to 3-phosphoglyceric acid, and this in turn means that diphosphoglyceric

acid becomes glyceraldehyde-3-phosphate plus inorganic phosphate (reversal of Reaction VIII, due to fall of concentration of glyceraldehyde-3-phosphate). Also the increase in the concentration of 3-phosphoglyceric acid and decrease in that of diphosphoglyceric acid is bound to reverse Reaction IX, so that A.T.P. is converted into A.D.P., whilst the monophosphoglyceric acid takes up another molecule of phosphate and becomes diphosphoglyceric acid. The net result is that inorganic phosphate is set free, while A.T.P. is dephosphorylated (see Fig. 1). As far as glyceraldehyde-3-phosphate and the two phosphoglyceric acids are concerned, the

A.D.P. + DIPHOSPHOGLYCERIC ACID → INORGANIC PHOSPHATE LIBERATED

3-PHOSPHOGLYCERALDEHYDE

IN PRESENCE OF ARSENATE

A.T.P. + 3-PHOSPHOGLYCERIC ACID

FIG. 1.—Illustrating effect of Arsenate in bringing about Liberation of Phosphate from A.T.P.

reaction is a cyclic one. This ingenious explanation of the effect of arsenate in providing a dephosphorylation mechanism for A.T.P., and thus accounting for the effect of arsenate on the fermentation of hexose diphosphate, is supported by various experimental observations, and seems to account for the essential facts.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

PETROGRAPHY AND PETROGENESIS OF IGNEOUS ROCKS.—The memoir by F. A. van Baren "On the Petrology of the Volcanic Area of the Goenoeng Moeria (Java)" (*Comm. General Agric. Exper. Stn., Buitenzorg, Java*, No. 60, 1948, 69 pp.) describes a suite of potash-rich lavas including latites (with monzonitic inclusions) leucite-tephrites and basanites, leucitites, augitites and limburgites, absarokites and trachytes, which compares well with suites from other East Indian occurrences. It differs from the Bufumbira (Uganda) and Vesuvian provinces in the abundance of latites (30 per cent. of whole field) and tephrites and leucitites (50 per cent.), the relative paucity of olivine-bearing rocks, and the occurrence of trachytes. Between the carbonate-syntexis theory of Daly, and the potash-rich emanation theory of Holmes for the origin of such suites, the author prefers the last-named. His general conception of origin is that of a primary hornblende-augite-andesite magma acted upon by potash-rich emanations. It is highly improbable that sufficient superheat was available for limestone syntexis.

"The Muriah Volcano (Central Java) and the Origin of its Leucite-bearing Rocks" is the theme of a short but important paper by R. W. van Bemmelen (*Proc. Kon. Nederl. Akad. Wetensch.*, **50**, 1947, 3–8). The volcano is situated on the labile border of the Sunda Shelf and is of Middle Pleistocene age. It breaks through a sedimentary series, mainly limestones. The bulk of the lavas consists of leucite-basalts, basanites and tephrites, with a late eruption of trachyte. Near contacts with limestones, potassic trachy-andesites with a narrow glassy selvage containing ægirine-augite, alkali-feldspar and nepheline, occur. Daly's limestone-syntexis theory is regarded as the best explanation for the differentiation. The original magmas were Pacific andesites and basalts. Assimilation of limestone caused the crystallisation of diopside and the evolution of carbon dioxide. The resurgent gases carried up some soda into the contact facies, causing the relative concentration of potash in the magma itself. This, in essentials, is regarded as the main mode of origin of the so-called Mediterranean Suite.

The study of "The Association of Alkali Rocks and Metamorphic Limestone in a Block ejected by the Volcano Merapi (Java)" by H. A. Brouwer (*Proc. Kon. Nederl. Akad. Wetensch., Amsterdam*, XLVIII, 1946, 166–89) shows that in a volcano like Merapi, which erupts pyroxene-andesites, a great variety of alkali-rocks can be produced *on a small scale* by the assimilation of limestone. But the phenomena raise the question whether, under more favourable circumstances, a large-scale production of alkali-lavas as, for example, in the volcanoes of Eastern Java, is possible through large-scale reaction of igneous magmas with limestone basements. The liberation of carbon dioxide augmented the volatile constituents which produced some crystal differentiation and caused the abundant vesiculation of the lavas.

"A Limstone Block in Hypersthene-dacite from the Koeda Neck," from the Kromong Complex near Cheribon, Eastern Java, is described by R. W. van Bemmelen (*Ingen. in Nederl.-Indie, IV. Mijnbouw en Geol. "De Mijningenieur,"* **7**, No. 3, 1940, 37–41). The volcanic necks are intrusive into Miocene limestones and Pliocene marly clays, and consist of hypersthene-dacites, some of which contain hornblende in addition. The Koeda vent is a pipe of glassy lava in which a large xenolith of limestone is enclosed. Large hornblende crystals intergrown with diopsidic augite have been formed by reaction. On this basis a far-reaching theory of limestone-syntexis followed by crystallisation-differentiation (mainly by sinking of hornblende crystals) is formulated to explain the course of differentiation of the Kromong magmas.

H. A. Brouwer has described "Granodioritic Intrusions and their Metamorphic Aureoles in the Young-Tertiary of Central Flores" (*Geol. Exped. Lesser Sunda Islands*, Leader: H. A. Brouwer, IV., 1942, 291–317). The plutonic rocks are intruded into a series of sediments and lavas (andesites and dacites) which, in all probability, belong to the late Tertiary of the island. The paper consists of routine petrographic descriptions of granodiorites with basic border facies, and the rocks of the contact aureoles, with numerous chemical analyses.

A paper by W. P. de Roever on "Olivine-basalts and their Alkaline Differentiates in the Permian of Timor" (*Geol. Exped. Lesser Sunda Islands*, Leader: H. A. Brouwer, IV, 1942, 209–89) describes olivine-basalts, *poeneites* and spilites, trachybasalts, alkali-trachytes and alkali-rhyolites. Poeneite is defined as a rock with the mineral composition and texture of basalt except that the feldspar is potassic. Elsewhere it is stated to be the potash-feldspar-bearing equivalent of the albite-bearing spilites. In the same way the term *barisanite* is used for the exclusively potash-feldspar-bearing equivalents of the albite-bearing keratophyres and quartz-keratophyres. The poeneites and spilites are regarded as the adularised and albitised representatives respectively of the basalts. Olivine-basalt was the initial magma and the other rocks of the above series have been formed by a "complex gravitative crystallisation-differentiation." The series constitutes "a kind of pre-geosynclinal magmatic phase" connected with faulting. The actual geosynclinal phase in Timor contains the classical ophiolite-spilite suite.

In a memoir on "Igneous and Metamorphic Rocks in Eastern Central Celebes," W. P. de Roever (*Geol. Explor. in Celebes*, Leader: H. A. Brouwer, 1947, 65-173) describes a long series of igneous rocks including peridotite and serpentine, eucrite, diorite and quartz-diorite, dolerite and albite-dolerite, spilite, augitite and albitite, which he rightly regards as a typical ophiolitic differentiation series comparable to that described from Timor (see preceding paragraph), and to that of the Ligurides in the Apennines. Prehnite, occurring abundantly as veins in both igneous and metamorphic rocks, is considered to be genetically connected with the ophiolitic series, as it is in many other occurrences.

T. Nyström's paper "Some Alkaline Rocks of Shansi Province, North China" (*Bull. Geol. Soc. China*, XXVII, Grabau Memorial Volume, 1947, 85–92) is a summary of an earlier memoir (*Bull. Geol. Inst. Upsala*, XXII, 1927). Three main areas are described, in each of which akerite seems to have been the parent magma from

which nepheline- and sodalite-syenites and nordmarkites have been developed. This series is similar to that of the famous Oslo province in Norway.

C. C. Wang describes one of the above areas in greater detail in a paper entitled "On the Magmatic Differentiation of the Tzuchinshan Alkaline Rocks, Linhsien, Shansi" (*ibid.*, 229–42). The rocks include trachyandesite, augite-syenite, nepheline-syenite, with tinguaites in the form of dikes. The various plutonic types seem to form a more or less stratified body with trachyandesite at the base and nepheline-ægirine-syenite at the top. The parent magma is believed to have had a composition approaching that of the augite-syenite.

A paper by C. J. Peng "On the Intrusive Rocks of the Fulin Igneous Complex, Eastern Sikang" (*Bull. Geol. Soc. China*, XXV, 1945, 155–84) describes a series of plutonic rocks ranging from dunite, through pyroxenite and gabbro, to granite and granophyre, with a few dike rocks including dolerite, hornblende-lamprophyre, aplite and quartz-porphyry. These are intimately related both in space and time, and are believed to be the result of differentiation of a common magma. Further, the pre-Sinian volcanic rocks of the district are believed to be genetically connected with the intrusive rocks, and to form with them a complete igneous cycle. All the rocks have undergone a low-grade metamorphism which has converted the ultrabasic rocks into serpentine and tremolite-talc-schists, and the gabbros to saussuritised meta-gabbros.

S. C. Chatterjee has described "The Gabbro Rocks found near Gorumahisani Pahar, Mayurbhang State, Orissa; and the Origin of the Associated Magnetite Deposits" (*Proc. Nat. Inst. Sci., India*, XI, 1945, 255–82). The main complex covers 20 sq. miles. The rocks range from norites to anorthosites, and grade into granulitic varieties which also cut the complex as dikes. The mass is a source of titaniferous and vanadiniferous magnetite deposits. Several analyses of anorthosite and gabbro are given, and the former is regarded as a differentiation facies of a gabbro magma. The magnetite ores are of two generations; a minor early generation, and a more important precipitation at a much later stage in the sequence of crystallisation. The view is taken that the second stage is not hydrothermal in the accepted sense, but truly magmatic in origin.

In his paper on "Geology and Petrology of the Bezwada Hill Ranges (Mysore)," M. S. Rao (*Proc. Indian Acad. Sci.*, XXVI, No. 3, Sect. A, 1947, 133–66) states that the acid and intermediate varieties of the charnockite series occur as huge massifs, while the basic and ultra-basic varieties occur either as dikes intrusive into

the " khondalite " paragneisses, or as minor differentiation products of the acid and intermediate types. Chemical analyses are given of the four main types of charnockite. While it is held as proved beyond doubt that the charnockite series is of igneous origin, yet assimilation of the paragneiss country-rocks may have played a part in the origin of some minor varieties. A unique association of chromite with charnockitic rocks is reported from this district. The mineral occurs as lenses and veins in the basic and ultrabasic members of the series.

An important memoir on charnockites from the type locality in Madras has been published by C. Rajagopalan (" Studies in Charnockites from St. Thomas Mount, Madras," Part I, *Proc. Indian Acad. Sci. Sect. A*, **24**, 1946, 315–31 ; Part II, *ibid.*, **26**, 1947, 237–60). The writer has not yet seen this paper.

A paper by A. F. Wilson deals with " The Charnockitic and Associated Rocks of North-Western South Australia. Part I. The Musgrave Ranges—an Introductory Account " (*Trans. Roy. Soc. S. Austr.*, **71** (2), 1947, 195–211). The Musgrave Ranges consist of a complex series of Pre-Cambrian gneisses and granites. The oldest rocks are paragneisses with garnet, cordierite, spinel and sillimanite. These are followed by hypersthene-bearing gneisses not showing obvious sedimentary origin and not containing the fluorescent zircon which characterises a succeeding group of hypersthene-gneisses. Continuing the succession, there are two series of non gneissic intermediate and basic charnockitic types with a granite formation between them. In a final stage dolerite dikes were injected and there was extensive mylonisation of all the rocks. The ubiquity of hypersthene, the comparative rarity of hornblende and biotite, and the marked paucity of pegmatites are held to indicate the " dry " conditions under which these rocks finally crystallised.

A. Hietanen has elucidated the " Archæan Geology of the Turku District in Southern Finland " (*Bull. Geol. Soc. Amer.*, **58**, 1947, 1019–84) and shows that there are four main rock groups : (1) the oldest complex of gneisses of sedimentary (kinzigites) and volcanic origin ; (2) intrusions of the first cycle, which include rocks of the trondhjemite and charnockite suites, both stated to be complete differentiation series ; (3) intrusions of the second cycle which include a complete differentiation series from hornblendites and gabbros to granites and pegmatites ; and (4) emplacement of diabase dikes, anorthosite and Rapakiwi granite masses. The last-named are later than a general migmatisation and granitisation which has affected the first three groups, in which metasomatism, anatexis and palingenesis have played an important part.

W. Pulfrey describes "A Suite of Hypersthene-bearing Plutonic Rocks in the Meru District, Kenya" (*Geol. Mag.*, LXXXIII, 1946, 67–89) which are intrusive into the Archæan Basement Complex of East Africa but may nevertheless be Pre-Cambrian. The lower parts of the hills are composed of norites and hyperites, with subordinate gabbros often rich in hornblende (bojite). Anorthosite occurs at higher levels, and pyroxenite appears as clots, patches and bands within the basic members of the complex. While it is difficult to ascertain the form of the intrusion, certain evidence suggests that it is a cross-cutting "floored" mass which may be at least 2000 ft. thick. A detailed petrographical classification of the rocks, making provision for the hornblende-bearing types, is given.

Describing "The Phosphate Deposits of the Eastern Province, Uganda," K. A. Davies (*Econ. Geol.*, XLII, 1947, 137–46) touches upon an igneous problem of very great interest. The phosphates occur in connection with a group of five ancient volcanoes within 20 miles of the south-western foot of Mt. Elgon. These are probably of post-Karroo and pre-Tertiary age. They are marked by a core of carbonatite which stands up above the plain level. Investigation is much impeded by the prevalent hard crust of laterite or ferricrete which covers the area to an average depth of 11 feet. All of the volcanoes are set in granite and are marked by five concentric belts which, proceeding inwards, consist of granites with sodic hornblende; kali-syenites, pulaskites, fenites, and melanocrates of the same mineral composition; mixed rocks, prevailingly ijolite, but with nepheline-syenite, melteigite, urtite, pyroxenite, dunite, etc.; a magnetite-apatite-phlogopite band with insignificant amounts of silicates; and finally a carbonatite core with widespread magnetite, apatite, phlogopite, pyroxenes, etc. The resemblances of these bodies to the famous Fen and Alnö occurrences, but especially to Afrikanda and others in the Kola Peninsula of Russia, are obvious.

A paper on "The Akaroa Volcano" is Part II of R. Speight's memoir on "The Geology of Banks Peninsula" (*Trans. & Proc. Roy. Soc. N.Z.*, **74**, Pt. 3, 1944, 232–54). The volcano breaks through a basement of plutonic rocks and trachytes, and consists mainly of basalts with intercalated tuffs and breccias. From the analyses given the "basalts" appear to be relatively soda-rich olivine-trachybasalts. A subsequent radial dike-swarm of trachytes and basalts ribs the skirts of the eroded cone. There seems to be a steady increase in acidity from the earlier flows to the later.

In the fifth part of his memoir on "The Basic Igneous Rocks of Eastern Otago and their Tectonic Environment," entitled "The Quartz-Porphyry of the Shag Valley and its Relation to the Mid-

Tertiary Basaltic Magma of North-East Otago; A Petrogenetic Essay," W. N. Benson (*Trans. & Proc. Roy. Soc. N.Z.*, **76**, Pt. 1, 1946, 1–36) describes the petrography of a mass of quartz-porphyry which is genetically related to the Oligocene basaltic activity of the province, and takes occasion to review current hypotheses concerning the genetic relations of acidic and basic igneous rocks. He thus discusses the hypothesis of the derivation of acidic rocks by fractional crystallisation of uncontaminated basaltic magma, and hypotheses involving assimilation with subsequent fractional crystallisation, gaseous transfer, and transfusion. While he does not come to a definite conclusion, either in regard to the Shag Valley intrusion, or to the general association of acid and basic igneous rocks, Professor Benson's thorough discussion will be of great value to petrologists.

In C. O. Hutton's paper on "Some Igneous Rocks from the New Plymouth Area" (*Trans. & Proc. Roy. Soc. N.Z.*, **74**, Pt. 2, 1944, 125–53), the mineralogy and petrology of andesites, dacites and hybrid rocks, have been investigated and, in addition, a study has been made of a porphyrite from a boring. To explain the distribution and structures displayed by these rocks the view has been advanced that they are remnants of thick cone-sheets which have been injected along tension cracks due to the rise of a magma now represented by the porphyrite sill. These sheets are much thicker than the typical examples of Skye and Mull, but not so thick as the described examples from the Kudaru Hills of Nigeria.

The sixth memoir in the series of "Studies on the Igneous Rock Complex of the Oslo Region" is by C. Oftedahl, and is "On Akerites, Felsites and Rhomb-Porphyries" (*Norske Vidensk.-Akad. Oslo, I. Mat.-Naturv. Kl.* 1946, No. 1, 51 pp.). It is full of detail which is difficult to summarise. The author first gives an authoritative definition of the term *akerite*. Akerite is a fine-grained monzonitic rock with plagioclase and alkali-feldspar in roughly equal proportions, colour index < 30, and quartz about 10 per cent. Its texture is marked by rectangular plagioclase crystals which are edged with alkali-feldspar. By this definition the akerites are distinguished from the truly plutonic types larvikite and kjelsaasite of similar chemical composition. Apparently akerites can occur as separate intrusions, as basic marginal facies to nordmarkites, and as acid marginal facies of the "Oslo essexites." Porphyritic akerites have a composition similar to that of the rhomb-porphyry lavas. "The relations of the akerites, breccias and felsites make it probable that these rocks form a continuous rock series." The genesis of the rock types described is exhaustively discussed.

In a memoir "Rochas e Minérios da Região Bragança-Vinhais

(North-East Portugal)," J. M. Cotelo-Neiva (*Portugal: Minas e Serviços Geológiços. Serv. de Fomento Mineiro*, No. 14, 1948, 251 pp. English Summary, pp. 229–48) describes a varied region of Archæan amphibolites, pre-Silurian chlorite- and sericite-schists with marbles, migmatites, gneisses and eclogites possibly of Carboniferous age, with large ultrabasic and basic intrusive masses. The latter are of economic interest as they are comparatively rich in chromite and platinum, and carry economic deposits of nickel ore. The ultrabasic rocks include peridotites, serpentinites, pyroxenites and hornblendites, with chromitite and magnetitite which are regarded as magmatic products. These rocks are associated with smaller bodies of gabbro, norite, diorite, porphyry and granite. Five new and perhaps unnecessary rock names are given for varieties of the ultrabasic rocks. A valuable feature of the memoir is the large number of new analyses of the igneous rocks.

In his paper "Some Petrogenetic Aspects of the Northern Basalt Plateaux," A. Noe-Nygaard (*Dansk Geol. Foren.*, **11**, 1946, 55–65) compares the rocks and the successions in the Tertiary igneous provinces of West and East Greenland, Iceland, Färoes and Western Scotland. In its syenitic intrusive centres East Greenland differs from all the others. The plateau (flood) basalts at the base of the succession in the various regions were undoubtedly emitted as fissure eruptions from several rift systems. This very extensive activity may have caused an upward bulge of the thermal front in the crust. The consequent rise of temperature may have caused fusion of the alkali-rich differentiate from the primitive olivine-basalt stratum, thus accounting for the trachytic and syenitic rocks of some areas. The thermal front may even have risen into sialic levels in the crust, leading to the production of the gabbro-granite or dolerite-granophyre sequence which commonly follows the early flood-basalt phase.

The memoir by H. I. Drever and P. M. Game on "The Geology of Ubekendt Ejland, West Greenland. Part I, A Preliminary Review" (*Medd. om Grönland*, **134**, No. 8, 1948, 35 pp.) is concerned with a terrain consisting almost entirely of Tertiary igneous rocks. Flood lavas of richly olivine-bearing basalts and oceanites, 5000 feet (possibly 10,000 feet) thick, form the basement of the island, and correspond to the early regional lavas of the Scottish fields. These were followed by a period of central eruption consisting of flows of tholeiitic type with trachytes and rhyolites, followed by an explosive phase of acid magmatic material forming volcanic vents, tuffs and breccias. Associated with this local episode are plutonic masses of gabbro and granite with hybrid types recalling those of

Ardnamurchan and Árran. There is no evidence in this locality of a late regional dike-swarm, although dikes are associated with each of the major phases mentioned above.

In a paper on " Some Basaltic Rocks from West Greenland," H. I. Drever and D. F. Livingstone (*Proc. Roy. Soc. Edin.* (Sect. B), LXIII, Pt. II, 1948, 97–114) have fully described three representative rocks from the little-known West Greenland Tertiary Igneous Province. The rocks include a picrite-basalt (or oceanite) which occurs as a dike; an olivine-basalt lava; and an olivine-free (tholeiitic) basalt lava. These three rocks have been chemically analysed, and their minerals examined microscopically in sufficient detail to give their approximate chemical composition. There is a brief discussion of their petrogenetic affinities with special reference to the Skaergaard magmas of East Greenland.

L. R. Wager's memoir on " Geological Investigations in East Greenland, Part IV. The Stratigraphy and Tectonics of Knud Rasmussens Land and the Kangerdlugssuak Region " (*Medd. om Grönland,* **134**, No. 5, 1947, 64 pp.) deals mainly with the Tertiary flood basalts and associated rocks. The sequences vary somewhat in different localities, but a thick series of tuffs believed to represent a definite period of explosive activity affords a reliable means of stratigraphical correlation. The basaltic lava series is bent into a monoclinal flexure which is associated with a dense dike-swarm. There are four major plutonic complexes within the area dealt with; the Kangerdlugssuak complex consisting of syenites ranging from quartz-bearing to nepheline-bearing types; the Kap Edvard Holm complex, largely gabbro but penetrated by a quartz-syenite mass (C. Deichmann); the famous Skaergaard intrusion of gabbros and ferro-gabbros, the subject of the classical memoir by Wager and Deer; and the Lilloise complex of syenites and nepheline-syenites at present known only by erratics. Professor Wager also provides valuable chapters on the Thulean tectonics of East Greenland and on the formation of the North Atlantic Ocean.

As the title indicates, the paper by K. F. Chudoba and J. Frechen on " Die frühmagmatische Bildung der Olivinausscheidungen vom Finkenberg (Siebengebirge) und Dreiser Weiher (Eifel) " (*Geol. Rundsch.*, **32**, 1941, 257–78) is a vindication of the view that olivine nodules are of early magmatic derivation from their respective magmas, and not xenoliths from neighbouring or underlying peridotites. Early separations of olivine are more magnesium-rich than the later. In the Finkenberg mass the mineral sequence is: olivine, olivine and enstatite, enstatite, enstatite and diopside, diopside, diopside and biotite, biotite; in the Dreiser Weiher it is

olivine, olivine and enstatite, olivine and diopside. The differences depend on the differences between the two magmas concerned, the Finkenberg being regarded as calc-alkalic, the Dreiser Weiher as alkalic. The formation of olivine nodules is most favoured in the "dry" laccolithic foci so frequently found in the Rhenish lava province.

In his paper on "Xenoliths and Skialiths," G. E. Goodspeed (*Amer. Journ. Sci.*, **246**, 1948, 515–25) distinguishes between xenoliths, cognate inclusions and skialiths. He proposes to restrict the term xenolith literally to "stranger rocks" (*i.e.* inclusions definitely foreign to the enclosing igneous rock). Xenoliths are broadly classed as epixenoliths when fragments are rifted from roof or walls near the surface, and hypoxenoliths when they clearly come from considerable depths. Cognate inclusions originate in the same way but represent an earlier phase of the magma or a derivative of it. Xenoliths and cognate inclusions are travelled fragments; *skialiths* (= shadow rocks) are static relics enclosed in rocks of metasomatic origin. They represent relatively unchanged remains left in rocks which have undergone metasomatic changes such as granitisation. These "inclusions" look somewhat like xenoliths but are merely vestiges which are distinguishable from the changed matrix, although often so altered themselves as to appear mere shadows of the original country rocks.

PEDOLOGY. By G. V. Jacks, M.A., Commonwealth Bureau of Soil Science, Rothamsted Experimental Station, Harpenden.

Tropical Soil Types.—The universally recognised genetic classification of soils originated in Russia and was worked out in detail by Russian scientists for the soils of their country. These soils embraced all the main climatic types from those of the tundra to those of the sub-tropics, but they did not include the soil types of the true tropics. Russian workers have extended their theoretical interest to tropical soils, and Z. Y. Shokalskaya has published tentative soil maps of India (*Dokuchaev Soil Inst., Contributions to the Soils of Asia*, 1932, **2**, 53–132) and of Africa (*Pedology*, 1944, 419–25), but Russian workers have had little actual experience of the tropics. Consequently, the classification of tropical soils has always been rather generalised in the Russian system, and tropical pedologists have found difficulty in establishing a genetic classification of the soils they have actually found. It is questionable, indeed, whether our knowledge of tropical soils is as yet sufficiently detailed to enable us to formulate a comprehensive taxonomic system. The

importance of topography in determining the distribution of tropical soils has been recognised in the development of the concept of the *catena*, to which reference was made in SCIENCE PROGRESS, 1947, **35**, 705.

Two main groups of tropical soil types are distinguished by H. Greene (*Soil Sci. Soc. Amer. Proc.*, 1946, **10**, 392–5)—the leached soils called red loams, red and yellow earths or laterites, and lowland clays representing the group designated as black cotton soils or tropical black earths. Greene suggests that these groups are topographically related, the former representing highly leached, acid soils occurring on the upper parts of slopes, and the latter alkaline soils occurring in the lowlands and containing material washed down from the lower slopes. He proposes a modification of Marbut's original definitions as follows, to suit tropical soils: *Pedocals:* I. Basic pedocals; containing calcareous horizon, but little or no iron accumulation; *e.g.* desert soils, dark alkaline calcareous clays. II. Ferric pedocals; containing both calcareous and ferruginous concretions. Both groups may be enriched with silica and bases. *Pedalfers:* III. Concretionary pedalfers; with localised iron stain or newly formed iron concretions and little or no accumulation of $CaCO_3$, *e.g.* podzolic soils and non-podzolic soils, the latter having low SiO_2/R_2O_3 in both surface and subsoil. IV. Red-loam pedalfers; leached of SiO_2 and bases, but retaining R_2O_3, finely dispersed and in admixture with kaolinitic clay. Iron-oxide concretions inconspicuous or absent.

G. Rodrigues and F. Hardy (*Soil Sci.*, 1947, **64**, 127–42) have described the weathering of, and soil formation from, a sedimentary foraminiferous clay under a humid tropical climate in Trinidad. The parent rock was mainly kaolinite, illite and quartz, with some montmorillonite. Weathering, which occurred to a depth of about 40 ft., has affected mainly the accessory minerals. The chief weathering processes have been oxidation and hydration, and the chief soil-forming process podzolisation, causing a slight accumulation of limonite at 20–25 ft. Calcium carbonate has been leached completely from the upper 25 ft., and has been reprecipitated at 30–35 ft., and leached calcium sulphate has similarly been precipitated at 30–40 ft.

Few studies of the pedology of tropical black soils have appeared recently. J. K. Basu and S. S. Sirur (*Indian J. Agric. Sci.*, 1938, **8**, 637–697) classified the black cotton soils or *regurs* of India as " immature chernozems " mainly on account of their high base status and well-developed crumb structure. They recognised, however, that *regurs* differed fundamentally from chernozems in

bleached when the soil was derived from quartzitic materials and red when derived from basic rocks.

The prevailing views on the nature of laterite (as distinct from lateritic soils) are also summarised by C. G. Stephens (*Counc. Sci. Indust. Res. Australia*, 1946, Bull. 206, pp. 21), who states that laterite is a pseudo-illuvial horizon, accumulating in the soil profile, in the zone of oscillation of the top of a seasonally fluctuating water table which, by causing alternate reducing and oxidising conditions, leads to the deposition from solution of hydrated oxides of iron and aluminium in the form of concretionary gravel. This concretionary gravel becomes cemented, where conditions favour its formation, into a massive structure by the progressive union of enlarging aggregates. Laterite is rather resistant to erosion and weathering, and sometimes appears on the surface of the ground, although it is formed as a subsoil horizon. In southern Australia a lateritic profile is essentially a podzol consisting of A, B and C horizons, with an accessory laterite horizon usually situated at the top of the B horizon. Sometimes there are several laterite horizons in one profile.

R. L. Pendleton and S. Sharasuvana (*Soil Sci.*, 1946, **62**, 423–40) define a laterite soil as one in which a laterite horizon is found in the profile. A lateritic soil is one in which an immaturely developed laterite horizon is found in the profile, or in which a laterite horizon will develop if prevailing conditions persist long enough. In Siam laterite develops either in a peneplain or a stable alluvial plain where the zone of annual oscillation of the water table is more or less fixed over a very long period of time and is close enough to the surface for ferrous compounds to be oxidised and thus immobilised, or in the soil or on the surface of the lower slopes of hills where ferrous compounds in the seepage water are oxidised to laterite. The character of the parent material is of secondary importance, although vesicular laterite tends to develop in a clayey, and pisolitic laterite in a sandy, matrix. In the great majority of laterite samples analysed the Fe_2O_3 content much exceeded the Al_2O_3 content, and the authors consider that definitions of laterite that place emphasis on the aluminium content are misleading.

R. Smith (*J. Aust. Inst. Agric. Sci.*, 1948, **14**, 15–17) obtained evidence in Western Australia suggesting that dolerite gave rise to " massive " laterite rich in iron oxide, whereas granite, which contained few iron-bearing minerals, produced easily transported, pisolitic laterite.

R. P. Humbert (*Soil Sci.*, 1948, **65**, 281–90) found laterite (in the above meaning of the term) only in regions with alternating wet

and dry climates. In permanently wet (rain-forest) regions the most advanced stage of lateritic weathering was indicated by the presence of a horizon of concretions that usually occurred at the upper boundary of the soil that remained continuously moist. True laterite was found almost exclusively on level or gently sloping ground. Microscopic evidence is adduced that indicates that the iron crust of laterite develops from nuclei on which hydrous iron oxides, formed by oxidation of iron compounds released during decomposition of ferromagnesian minerals, are precipitated. Moisture is lost steadily by diffusion, and the dehydration curve shows no evidence of the presence of hydrates. As dehydration progresses, the surface of the precipitated material is reduced, and eventually a compact concretion is formed. The concretions grow in size by a process similar to the formation of Liesegang rings, and many of the crystalline constituents of the matrix are enmeshed by the growing concretions and by the connecting veins of iron oxide that develop. Ultimately an indurated crust is formed. Humbert recognised the two types of laterite, pisolitic and vesicular, distinguished by Pendleton and Sharasuvana. The pisolitic type is distinguished by an abundance of globular concretions about the size of a pea, the vesicular type by its porosity, its vein-like cementing structures and the relatively small number of concretions.

H. Scaëtta (*Ann. Agron.*, 1940, **10**, 101–26) explained the formation of laterite as the climax of an erosion cycle that is completed fairly quickly in tropical climates. In the first phases of the cycle intense decomposition and leaching cause the accumulation of a residue of lateritic products (sesquioxides) which, on exposure to the air at the soil's surface, form a hard crust which protects the surface soil from erosion and the subsoil from lateritisation. The soil has then reached its climax of evolution—a state of equilibrium and inertness. Subsequent weathering ultimately destroys the crust, which is removed by erosion, exposing the formerly protected subsoil to lateritisation.

BOTANY. By P. R. Bell, B.A., University College, London.

Morphology and Phylogeny of Ferns.—The Ferns, a relatively small group of plants with undoubted primitive features and a long fossil history, have always excited morphological interest and phylogenetic speculation.

N. W. Radforth (*Trans. Roy. Soc. Edinb.*, **59**, 385, 1938) has re-examined two well-known fossils of the Coal Measures, *Dactylotheca plumosa* Artis and *Senftenbergia ophiodermatica* Göppert. The material was in the form of compressions and was removed

from the rock by the cellulose transfer method. As well as direct examination in the film, some of the material was macerated in nitric acid and potassium chlorate and mounted in pure corn syrup. In this way mounts of unbroken sporangia and delicate groups of spores were obtained. Radforth shows that, despite slight differences in the form of the frond, the structure of the spores and sporangia is almost identical and concludes that the two "species" are nothing more than different specimens of the same plant. These compressions should now be referred to *Senftenbergia plumosa*. The sporangia are shortly stalked, not sessile as was formerly thought, and the annulus is definitely Schizæaceous, a feature long suspected but not before conclusively demonstrated. Continuing these studies (*Trans. Roy. Soc. Edinb.*, **59**, 745, 1939) Radforth shows that *Dactylotheca Sturi* is actually a species of *Senftenbergia*. He also describes a method for estimating the number of spores in the unopened sporangium. The sporangial output of *Senftenbergia Sturi* from the Lower Carboniferous is found to be 940, of *S. plumosa* from the Upper Carboniferous 200, and of *S. pennæformis* from the highest horizons of the Upper Carboniferous 1300. Certain morphological trends in these three species are brought to light. There is an increasingly distinct differentiation of the cells of the annulus, which also tends to become narrower in outline. In addition, there is a progressive reduction in the number of cells forming the distal plate of the sporangium and a progressive shortening and thickening of the stalk. Both *S. Sturi* and *S. pennæformis*, as well as *S. plumosa*, show resemblances to the modern *Anemia* in the form of the sporangium and spores.

O. H. Selling (*Bernice P. Bishop Museum, Special Publ.*, **37**, 1946) has described the spores of the Hawaiian Pteridophytes. The value of spore characters in phylogenetic and systematic studies is demonstrated in a further paper (*Meddel. fr. Göteborgs Bot. Trädg.*, **16**, 1944) in which are identified for the first time spores which are almost certainly of species of *Schizæa* from the Tertiary of Central Europe. After establishing that the spores of the genus *Schizæa* are furnished with a single instead of a triradiate scar, Selling considers the various developments of the exospore found within the genus. Referring to Prantl's sub-divisions of the genus, based principally on the degree of development of the frond, he shows that in Section I, 1 (frond simple), the exospore deposits are thick (up to 5 μ) and may be either smooth or furnished with a complicated alveolate structure. In Section I, 2 (frond once or twice dichotomous), the deposits are thinner (about 2 μ) and may be minutely tuberculate. In Section II (frond with several segments tending to

be digitately arranged), some species show thick deposits (4–5 μ), others show unique types with oblique or longitudinal striations, with or without pitting. This suggests that Section II may be heterogeneous. In Section III (true lamina present to a greater or lesser degree) the secondary thickening of the exospore is very much reduced. There is also present in some species of this section a fine granulated membrane outside the exospore; this membrane has also been observed as an early and transient feature in the spores of some species belonging to Sections I and II. Assuming Section I to contain the most primitive species and Section III the most advanced, an hypothesis supported by anatomical evidence, there appears in the course of evolution to have been a progressive retarding of the disintegration of this outer membrane together with a thinning of the exospore deposits. Spores possessing exospore deposits similar to those of *Schizæa* (which are unlike those encountered in any other genus), but not referable to any living species, have been found in the Tertiary of Central Europe and the Quaternary of the Hawaiian Islands. The Hawaiian spores are very similar to those of *S. pusilla*, a boreal species, whilst, of the three European records, one pertains again to *S. pusilla* and the other two to species now South Asiatic in distribution.

Examination of material in various herbaria (*Svensk. Bot. Tidskr.*, **38**, 207, 1944, **40**, 273, 1946; and **41**, 431, 1947) has shown that in the past a complex of four species of differing distributions has been included in *Schizæa digitata*. These species are defined by the structure of the sporangia and spores, but are also recognisable macroscopically by the form of the frond. The distribution of *S. digitata* s. str. is mainly Asiatic, whilst the areas of the other three species of the complex are more southern, are very much more limited and tend to be insular. This series of papers forms not only a valuable contribution to our knowledge of evolution within the genus *Schizæa*, but also of the vegetational and land changes that have taken place in the Southern Hemisphere.

R. E. Holttum (*J. Linn. Soc.* (*Bot.*), **53**, 123, 1946) has published a revised classification of the Polypodiaceæ (as defined by Christensen in Verdoorn's *Manual of Pteridology*, 1938). The scheme is frankly tentative, but is of great value in incorporating the author's wide knowledge of tropical ferns. Christensen's Polypodioideæ, with the exception of *Grammitis* and some allied genera, is elevated to the rank of a family. Holttum sees the primitive Polypodiaceous plant as one with pedate-dichotomous branching of main veins and lamina with reticulation of the smaller veins (*e.g. Dipteris*). From this there has been a tendency to the formation of a simple lamina (*e.g.*

Lepisorus) and then by way of pinnate lobing to simply pinnate fronds (*e.g. Polypodium*). This sequence is held to be quite different from that seen in other families of ferns, where the tendency is the development of simple fronds with reticulate venation from much-divided free-veined types.

The largest family of Holttum's scheme is his Dennstædtiaceæ, which is divided into eleven sub-families. The early sub-families, such as the Dennstædtioideæ and the Davallioideæ, belong to Bower's Marginales, but the later ones, amongst which the Asplenioideæ, Blechnoideæ, Dryopteroideæ and Athyrioideæ are of especial interest, Bower regarded as belonging to his Superficiales. Holttum shows that on morphological and anatomical grounds *Asplenium* has probably an origin distinct from that of *Athyrium*, although hitherto the two genera have been closely associated. It is suggested that *Asplenium* is derived from a Davallioid type, *Asplenium* Sect. *Loxoscaphe* forming a significant morphological intermediate, while *Athyrium* shows Dryopteroid affinities. The Asplenioideæ are thus associated with the Davallioideæ and should be regarded as belonging to the Marginales rather than to the Superficiales. In considering *Dryopteris*, Bower's arguments for deriving this genus from the Cyatheaceæ are re-examined. It is suggested that on all counts a derivation from some *Dennstædtia*-like form is much more probable. In addition, recent studies of the prothalli of Dryopteroid and Cyatheaceous genera do not support the view Bower formed of their similarity. The Dryopteroideæ and the allied Athyrioideæ are thus transferred to the Marginales. The Blechnoideæ are still a puzzling group. Bower's theory of the leaf flange is held to be improbable and a fresh origin of the group is sought in Asplenioid types, the interrupted sorus seen in *Blechnum puncticulatum* var. *Krebsii* forming a transition leading to the cœnosorus. The *Lomaria* condition is thus reduced. The position remains obscure, however, and the full anatomical and morphological evidence is yet to be assembled. A great part of the value of this paper lies in the many stimulating views that are put forward.

R. Pichi-Sermolli (*Nuovo G. Bot. Ital.*, N.S. **53**, 129, 1946) describes a fern from Abyssinia which he assigns to a new genus, *Negripteris*, the sole representative of a new family, the Negripteridaceæ. The sporangia are very large, spheroidal, and furnished with a broad, uniseriate, massive annulus, slightly oblique and interrupted at the base. The sporangia are almost sessile and are only very few (1–3) in the sorus. They tend to be gradate and almost marginal in origin, being protected in the mature state by a reflexed indusioid leaf margin. There is no stomium and dehiscence

is irregular. The rhizome is solenostelic and the petiolar trace V-shaped. These features, together with its geographical isolation, point to *Negripteris* being a relatively primitive genus, but unlike any hitherto described. Its affinities remain for the moment rather obscure.

An attempt to include the fossil and living species allied to *Schizæa* in one classification has been made by C. F. Reed (*Bol. Soc. Broteriana*, **21**, (2ª Ser.), 71, 1947). A new Order, the Schizæales, and many new families are created. Amongst the fossil genera one finds *Senftenbergia*, a placing justified by Radforth's work, but there are also included and placed confidently in families *Hapalopteris*, *Acrostichopteris* and *Onychiopteris*, the Schizæaceous nature of all of which must at best remain speculative until conclusively demonstrated. On the other hand, Gothan's *Norimbergia*, a Schizæaceous fern from the Bavarian Rhætic with habit resembling that of *Lygodium*, is given no place at all. In the diagnosis of the new Order the spores are said to lack an exospore but to possess a perispore when, assuming the terminology to be that normally used, the reverse would appear to obtain. No convincing arguments are given to justify the creation of as many as eight families within the Order, nor the extensive splitting of the usually accepted genera. The work is, in fact, largely speculative and very much more evidence must be forthcoming if the many phylogenetic assertions in it are to be generally accepted.

E. B. Copeland (*Annales Cryptogamici et Phytopathologici*, **5**, 1947) has published a reclassification of all the living ferns. Bower has clearly demonstrated that in attempting a phylogenetic classification of this group, considerations of anatomy, morphology and past and present distribution must enter. It is, therefore, disappointing to find a modern work claiming to be phylogenetic in which these features receive little if any recognition. One will turn to Copeland's *Genera Filicum* for a mine of information about the ferns themselves, but one will meet also many assertions that are mainly subjective and in at least one instance (for example, that the species of *Schizæa* are " of the most evidently Antarctic ancestry ") not in accord with recent researches.

Finally, mention should be made of an interesting paper by A. C. Long (*Ann. Bot.*, N.S., **7**, 135, 1943) in which he describes buds on what in the past have been taken to be the rachides of *Botryopteris hirsuta*. Although these may be similar in nature to the buds that are regularly found on the fronds of certain ferns, there is also the possibility, though it is not discussed by the author, that a thorough reinvestigation of this and allied plants

by modern methods may fundamentally alter our conceptions of their morphology.

PLANT PHYSIOLOGY. By PROFESSOR WALTER STILES, Sc.D., F.R.S., The University, Birmingham.

MINERAL NUTRITION OF PLANTS.—In the last article in this series the absorption of mineral salts by plant cells was dealt with. In the present contribution some recent papers dealing with the fate and function of the mineral constituents are considered.

C. P. Sideris and H. Y. Young ("Effects of Potassium on the Nitrogenous Constituents of *Ananus comosus* (L.) Merr," *Plant Physiol.* **21**, 218–32, 1946) have examined the nitrogen metabolism of pineapple plants grown in cultures containing little (4 mg. per litre) and much (205 mg. per litre) potassium, and found that the relative amounts of different nitrogen fractions were very different under the two conditions of potassium supply. Protein nitrogen in the leaves, calculated per gram of tissue, was greater in the low-potassium plants, but in the stem the protein nitrogen was greater in the high-potassium plants, and when the amounts of protein per plant or organ were calculated it was found that for both leaves and stem the protein value was higher in the plants supplied with much potassium. The authors consider that a physiological function of potassium is concerned in the conversion of soluble organic nitrogen to protein. In the high-potassium plants this function is more noticeable in the stem than in fully expanded leaves. The higher content of protein in the stems of the plants supplied with much potassium the authors relate to the presence in the peripheral and apical regions of the stem of much meristematic tissue which, possessing potential growth activity, requires more protein for protoplasm formation than the tissues of low-potassium plants which have not the same potentialities for growth.

Sodium is not generally regarded as an essential element in plant nutrition, but field experiments described by J. J. Lehr ("The Importance of Sodium for Plant Nutrition. IV. Influence of Nitrate Fertilizers on the Equilibrium of Cations in Fodder Beet," *Soil Sci.*, **63**, 479–86, 1947) indicate that this element is necessary for maximum yield of beet, and that sodium must be regarded as an essential element for beet, a view put forward by Stoklasa and Matoušek as long ago as 1916. Lehr lays emphasis on the fact that sodium should not be regarded as a substitute for potassium in plant nutrition, but that potassium and sodium play independent parts in the nutrition of the beet. Emphasis is also

laid on the importance of the ratios of the various nutrient ions in the medium in determining growth and hence crop yield.

A symposium on magnesium in plants, consisting of eight papers, has been published in *Soil Science.* In the first of these papers M. Zimmerman (" Magnesium in Plants," *Soil Sci.*, **63**, 1–12, 1947) summarises what had been discovered concerning the specific functions of this element within the plant. These are discussed under the following headings: role of magnesium in plant constituents and processes, the quantity of, and need for, magnesium in plants, and magnesium absorption by plants as affected by other ions. In the second paper W. S. Eisenmenger and K. J. Kucinski (" Relationship of Seed Plant Development to the Need of Magnesium," *Soil Sci.*, **63**, 13–17, 1947) put forward the view that among flowering plants resistance to magnesium deficiency is greater the higher the species is in the evolutionary scheme. Plants belonging to a number of families were grown on plots with and without added magnesium, and in general it was found that among dicotyledons resistance to magnesium deficiency was least in the Ranales, Magnoliaceæ and Anonaceæ and highest in Compositæ, Campanulaceæ and Labiatæ. Among monocotyledons resistance was least in the Alismales and greatest in the Orchidaceæ and Gramineæ. Exceptions to the rule were found in the Solanaceæ and Cucurbitaceæ in which cultivated species exhibited symptoms of deficiency more quickly than naturally occurring species.

In the third paper of the symposium, by E. Truog, R. J. Goates, G. C. Gerloff and K. C. Berger (" Magnesium-Phosphorus Relationships in Plant Nutrition," *Soil Sci.*, **63**, 19–25, 1947), field experiments with peas are described in which the effect of manuring with magnesian limestone on the utilisation of phosphorus was examined. It was found that the content of phosphorus in the pea seeds appreciably increased with increasing supplies of magnesium. The results were confirmed by water culture experiments in which the magnesium, supplied as magnesium sulphate, varied from 1 to 100 p.p.m. (parts per million) in the culture solution and the phosphorus, supplied as potassium dihydrogen phosphate, varied from 10 to 90 p.p.m. Thus in the experiments with the lowest level of phosphorus the content of phosphorus in the seeds rose from 0·36 per cent. to 0·43 per cent. with an increase in the magnesium content of the culture solution from 1 to 100 p.p.m., while the yield of seed increased from 0·5 to 7·5 g. In the experiments with phosphorus supplied at the highest level the phosphorus content of the seeds increased from 0·39 per cent. to 0·53 per cent. with increase in the magnesium content of the culture solution from 1 to 100 p.p.m., while the

yield of seed increased from 1·0 g. to 8·5 g. The results recorded are held to support the view that magnesium functions as a carrier of phosphorus.

In the fourth paper of the symposium, H. P. Cooper, W. R. Paden and W. H. Garman (" Some Factors influencing the Availability of Magnesium in Soil and the Magnesium Content of Certain Crop Plants," *Soil Sci.*, **63**, 27–41, 1947) discuss ionisation potential and electrode potential as factors in determining the availability of magnesium in soils and claim that chemical analyses of a number of plants indicate a close correlation between normal electrode potential and the mineral content of plants. Analyses show that, in plants of humid climates, as regards the metallic content magnesium ranks next to potassium and calcium, which suggests the advisability of adding magnesium as a fertiliser to the soil, and it is pointed out that the yield of crops containing a relatively large amount of magnesium, such as potatoes, tobacco, tomatoes and cotton, is often increased by the inclusion of magnesium in mixed fertilisers.

The next three papers in the symposium deal with magnesium requirements of particular crop plants, namely, Citrus fruits, dealt with by A. F. Camp (" Magnesium in Citrus Fertilization in Florida," *Soil Sci.*, **63**, 43–52, 1947), apples by D. Baynton (" Magnesium Nutrition of Apple Trees," *Soil Sci.*, **63**, 53–8, 1947) and tobacco by J. E. McMurtrey (" Effect of Magnesium on Growth and Composition of Tobacco," *Soil Sci.*, **63**, 59–67, 1947). These papers all stress the importance of magnesium as a nutrient and deal with the occurrence and symptoms of magnesium deficiency and the means of controlling it.

The final paper of the symposium, by A. L. Prince, M. Zimmerman and F. E. Bear, deals mainly with magnesium in the soil ("The Magnesium-supplying Powers of 20 New Jersey Soils," *Soil Sci.*, **63**, 69–78, 1947). Among the conclusions of these authors is the important one that the most important factor determining the uptake of magnesium by plants is the amount of available potassium present. If the latter is large the magnesium content of the plant is low, and as the potassium supply diminishes the magnesium content of the plant increases.

C. Olsen (" The Mineral, Nitrogen and Sugar Content of Beech Leaves and Beech Leaf Sap at Various Times," *Comp. rend. trav. lab. Carlsberg, Sér. chim.*, **26**, 197–230, 1948) has examined the amounts of various mineral elements in beech leaves at twenty-seven different times between the expanding of the leaves and leaf fall. The elements determined were nitrogen, phosphorus, potas-

sium, calcium, magnesium, iron, manganese and silicon. The quantities of these in the expressed sap as well as in the entire leaves were determined. The sap was obtained by pressure of the leaves after freezing. The data so obtained showed not only what proportion of each mineral constituent was held in solution in the sap and what was bound elsewhere in the cells, but also showed whether each constituent after reaching the leaf remained there entirely or was to any extent returned to the stem or other organ.

The amount of nitrogen in the leaf was found to increase rapidly during May and to reach a maximum about the time when the leaves were fully developed at the beginning of June. Then for the first half of June there was a passage of nitrogen from the leaf to the stem, about 10 per cent. of the total nitrogen of the leaf being translocated into the stem during this time, after which the nitrogen content remained constant until late September when translocation from the leaf started again until by leaf-fall about 45 per cent. of the nitrogen of the leaf had been translocated away. The nitrogen content of the sap fell during May, remained constant through the summer and rose during the period preceding leaf-fall.

The amount of phosphorus in the leaves also rose rapidly during May, and then, like nitrogen, phosphorus was translocated from the leaves, which lost 33 per cent. of their content of this element during the first half of June. Thereafter the total phosphorus content of the leaves remained constant up to leaf-fall. The proportion of phosphorus in the sap, however, rose continuously during September and October.

The potassium content of the leaf rose rapidly during the spring, reaching a maximum in July, after which there was a regular translocation of potassium from the leaf so that at leaf-fall only about 55 per cent. of that present in July remained in the leaf. About 70 per cent. of the potassium of the leaves was in solution in the sap; the other 30 per cent. was bound in some way in the cells.

The amounts of calcium, silicon, iron and manganese in the leaf all increased continuously from the opening of the buds until leaf-fall, there being no translocation away from the leaf of any of these elements. There was also no translocation away from the leaf of magnesium, but the amount of this element in the leaf reached a maximum by about the beginning of June and thereafter remained constant until leaf-fall. About 60 per cent. of the magnesium was in solution in the sap, about 9 per cent. was present in combination in cholorophyll and about 30 per cent. must have been bound in some other way. On the other hand, only about 12 per cent. of the calcium was present in solution in the sap in the summer months,

while 65 to 70 per cent. was present as calcium oxalate, the remaining 20 per cent. being accounted for as calcium pectate and other combinations.

In a second paper Olsen returns to the question of potassium in beech leaves ("Absorptively Bound Potassium in Beech Leaf Cells," *Physiologia Plantarum*, **1**, 136–41, 1948). As noted above, Olsen found that about 30 per cent. of the total potassium content of frozen beech leaves subsequently thawed was bound in some way. From late September onwards this amount continuously decreased until shortly before leaf-fall all the potassium in the leaf was present in solution in the sap. This bound potassium could be completely displaced by addition of a decinormal solution of calcium chloride, while addition of a solution of potassium chloride brought about a further binding of potassium. By repeated washing with water all the bound potassium could be brought into solution. It is supposed that the potassium is adsorbed on the cell proteins, since the amount of bound potassium decreases parallel with the breaking down of the proteins during senescence, reaching zero shortly before leaf-fall.

Hoagland and Broyer had previously shown that the uptake of ions by the excised roots of barley plants depended on the salt status of the roots, ions of potassium bromide being absorbed if the roots contained a low concentration of salt and a high concentration of carbohydrate (the low-salt condition), but not if the roots contained a high concentration of salt and a low concentration of carbohydrate (the high-salt condition). T. Alberda ("The Influence of Some External Factors on Growth and Phosphate Uptake of Maize Plants of Different Salt Conditions," *Rec. Trav. bot. néerlandais*, **41**, 542–601, 1948) has now examined the uptake of phosphate by the roots of entire maize plants using both low-salt and high-salt plants. The plants were grown in water culture, the low-salt plants being obtained by transferring the cultures to tap water some time before an experiment and the high-salt plants by providing the cultures with a frequently renewed nutrient solution. In some experiments the plants were provided with a continuously flowing nutrient solution. The uptake of phosphate was determined by analysis of the external solution.

Both the low-salt and high-salt plants absorbed phosphate continuously over several days. The rate of absorption by the low-salt plants increased for two or three days and then remained constant, whereas with the high-salt plants the rate of absorption was constant from the beginning, provided other conditions were constant. Generally more phosphate was absorbed by the low-salt

than by the high-salt plants, though the rate of uptake was found to be dependent on the external conditions during the cultivation of the plants.

As the concentration of phosphate in the external solution was increased the rate of uptake of phosphate increased until with a certain concentration a maximum was reached; the value of this concentration was much higher (about 24 mg. P_2O_5 per litre) with low-salt plants than with high-salt plants (about 12 mg. P_2O_5 per litre).

The effect of light on absorption was found to be different with the two kinds of plants. With high-salt plants absorption stopped in darkness and was even replaced by exosmosis, whereas low-salt plants were able to absorb phosphate in darkness.

There was always found a close relationship between absorption of phosphate and growth, the rate of absorption and growth running parallel with both low-salt and high-salt plants.

Alberda considers that the results obtained on the effects of concentration of phosphate and of light on absorption indicate that the growth of the shoot determines absorption by high-salt plants. In these, part of the phosphate which enters the xylem of the root and is then carried upwards in the transpiration stream is carried back to the root in the phloem if the supply from the root exceeds consumption in the shoot.

In some short-period experiments absorption of phosphate was found to be independent of the ions in the culture solution. This is attributed to the removal of the ions by metabolic processes in which they are bound by the protoplasm, the binding being specific for any one ion in that it can only be replaced by an isotope or a closely related ion.

By means of ash analyses, M. Rottová ("The Movement of Iron, Manganese and Copper in the Germination of *Æsculus hippocastanum*," *Acta Fac. Rerum Nat. Univ. Carolinæ*, No. 179, 18 pp., 1947) has followed quantitatively the movement of iron, manganese and copper from the seed to the various parts of the seedling during the germination of the horse-chestnut. The results indicate that with each element the most rapid movement is always into the region of the seedling which is undergoing most active growth. While of the three elements manganese was present in greatest amount, copper showed the highest rate of movement relative to its concentration and iron the lowest.

The rate of movement of phosphorus, as well as water, to the swelling buds in hornbeam and beech has been examined by H. Burström ("The Rate of the Nutrient Transport to Swelling Buds

of Trees," *Physiol. Plantarum*, **1**, 124–35, 1948). From analyses of buds and branches it is concluded that during the swelling period phosphorus travels to the buds from distances between 0·15 and 1·1 m. in the hornbeam and between 0·3 and 1 m. in the beech. If the phosphorus is conveyed passively to the buds in the water stream it is concluded that this cannot be through the xylem, since the phosphorus content of the water in the latter is too low. It is possible that the transport is through the phloem, for the observed rates of water movement of 5·7 and 10 mm. per hour for hornbeam and beech respectively are less than those which have been previously calculated for rates of flow in the phloem, namely up to 300 mm. per hour.

ENTOMOLOGY.

By A. D. Lees, M.A., Ph.D., Agricultural Research Council, Unit of Insect Physiology, Cambridge.

Modern Concepts of Instinctive Behaviour.—Entomologists interested in the problems of animal behaviour will welcome the recent article by W. H. Thorpe (*Bull. anim. Behav.*, Feb. 1948) in which the views developed by K. Lorenz over the last decade are expounded and discussed. As Thorpe points out, one outcome of Darwin's work was the almost independent growth of two theories of animal behaviour, the mechanistic-physiological and the psychological. Followers of the former school tended to focus attention on the fixity of instinct and frequently sought to explain away all animal behaviour in terms of current physiological concepts. The second line of enquiry, deriving from "The Expression of Emotions in Man and Animals," and culminating in modern comparative psychology, became concentrated upon the modifiable aspects of behaviour (types of learning); indeed, some of its exponents came near to denying the necessity of the term "instinct." It is the great merit of Lorenz's thesis that these divergent approaches to animal behaviour are harmonised.

According to Lorenz, there resides in all true instinctive behaviour a core of inborn fixed automatism (*Erbkoordination* or "instinct" in the restricted sense) which is no less constant than an anatomical structure and which can equally well be used in classification. Each instinct has the property of building up a specific tension (or "reaction specific energy") in the nervous system. If the animal does not find itself in a situation appropriate for the release of the instinct, the "reaction specific energy" is dammed up and the threshold of effective stimuli lowered. Sometimes indeed the instinct may go off without an external stimulus, yielding so-called "vacuum activity." This process is responsible

for the signs of emotional stress which have led some observers to lay particular emphasis on the non-physiological aspect of instinctive behaviour. An instinctive act in this sense, it is pointed out, is distinct from a reflex act, since the latter involves nothing comparable with reaction specific energy. And it is distinguishable from a taxis insomuch as the instinct is released but is not guided by the environmental situation. Further, the releasing situation is usually complex (a *Gestalt*) and not a simple physical stimulus.

Although imparting to the behaviour of the animal an essential "drive," a pure instinct would by itself be useless because of its inflexibility. In practice even the simplest instinctive actions comprise in addition a whole series of undirected (kineses) and directed (taxes) reactions which may be modifiable by conditioning and other types of learning. These reactions, which go to make up "instinctive behaviour" in its broad sense, are characterised here as the "appetitive behaviour." If the appetitive behaviour leads the animal to a situation which is appropriate for the satisfaction of the physiological need, the internal tension is dissipated. But the attainment of this goal requires the recognition in the goal situation of a "releaser" which fits a "receptory correlate" in the animal's nervous system as a key fits a lock.

This concept of instinct possesses several advantages. It invites the causal analysis of behaviour. It does not eliminate the psychological approach, but delimits the field where it is useful, namely in the adaptive phases of appetitive and goal-finding behaviour. A contradiction frequently found in earlier work regarding the "intelligence" or "stupidity" of a particular animal is also explained. If the experimenter is studying the fixed automatic element in instinctive behaviour, and the instinct runs to completion in a situation which is biologically inappropriate, the behaviour will appear as "stupid." On the other hand, the same reaction in the correct environment may appear remarkably purposive.

The place of "releasers" in animal behaviour has recently been discussed by N. Tinbergen (*Wilson Bull.*, 1948, **60**, 6). The nature of the releasing stimulus, which usually forms only a small part of the total perceptual field, is often related to the development of the special senses in the animal, visual releasers being common in animals with well-developed eyes and so on. It is pointed out that, in order that the reaction may not go off in the biologically wrong surroundings, the releaser must be improbable in the biological sense; at the same time, when the receptory correlate is innate, the releaser must be simple, since the animal's conception of its environment will be modified continually by conditioning. Among

insects, the appetitive behaviour commonly consists of a chain of separate reactions, each set in motion by a particular releaser; or several releasers may summate and set off one reaction.

The theme may be illustrated by the observations of N. Tinbergen, J. D. Meeuse, L. K. Boerema and W. W. Varossieau on the courtship of the Grayling butterfly, *Eumenis semele* (*Z. Tierpsychol.*, 1942, **5**, 182). Here, presumably, the reproductive (physiological) need leads to the building up of an internal tension (reaction specific energy), which is dissipated in random movement (appetitive behaviour). The male butterfly will pursue any object that is fluttering, dark in colour and close at hand. These three releasers must be additive, for the male evinces less interest in any model that lacks one or more of these qualities. When both partners are at rest the male performs a dance, "bowing" before the female with wings outstretched so that the antennæ of the latter brush against the scent glands on the dorsal surface of the male's forewings. This is the signal (releaser) which secures the further co-operation of the female in coitus.

Although insect behaviour is particularly rich in instances of releasing stimuli, there are far fewer clear examples of the manifestation of instinct in the restricted sense of Lorenz. Something akin to "vacuum activity" seems nevertheless to occur in some social and nomadic insects, although in these neither the goal nor the releaser situation are usually easy to define. An example is provided by the remarkable cyclical colony activities in *Eciton hamatum*, as revealed by the researches of T. C. Schneirla (*J. N.Y. ent. Soc.*, 1944, **52**, 153). During the rainy season in Panama colonies of this army ant pass cyclically through *statory* and *nomadic* periods, each lasting about 20 days. During the statory phase the bivouac, to which the single queen is confined, remains *in situ* and raiding activities are minimal. During the nomadic phase the position of the bivouac is changed each nightfall and strong raiding parties emerge from the colony. This activity is in no way related to the abundance or scarcity of food in the neighbourhood, but is governed by the reproductive functions of the queen. Every 35 days or so this individual liberates a mass of about 20,000 eggs over a very short period, with the result that all members of the brood develop in synchronisation. Oviposition takes place during the statory period when the existing brood is in the pupal stage. The nomadic period begins with the emergence of the callow workers from the pupæ; as their energising effect is diminishing it is supplemented by the emergence of larvæ from the egg mass. When these pupate at the beginning of the statory period, nomadic activities

die down. The " drive " (reaction specific energy) imparted by the presence of larvæ in the colony is explained in terms of Wheeler's trophallaxis concept. The eggs and pupæ provide no social-stimulative effect.

THE DETERMINATION OF PATTERN AND SHAPE IN THE WINGS OF INSECTS.—Inherited variations in insect wing patterns continue to provide material for the study of gene action. Examples are the observations of A. Kühn and M. v. Engelhardt on the dark scale patterns in a lethal and two melanic mutants of *Ptychopoda seriata* (*Biol. Zbl.*, 1940, **60**, 561 ; 1943, **63**, 251). An interesting example where the genetic aspect has been very thoroughly analysed is the determination of the elytral colour pattern in *Harmonia axyridis* (C. C. Tan, *Genetics*, 1946, **31**, 195). In this ladybird beetle the very variable pattern can be accounted for by assuming the existence of 12 alleles of an autosomal locus. Homozygotes of the variety *succinea* have an elytral pattern of black spots on a yellow background, the remainder having yellow or orange spots on a black background. In crosses the *succinea* allele behaves as a recessive, save that any black spots typical of the *succinea* parent will appear in the offspring if they overlap yellow areas in the other parent. In crosses involving other alleles also, any portion of the elytrum that bears black pigment in either parent will be found to be black in the heterozygous offspring. The physiological basis of this ' mosaic dominance ' is unknown as yet.

One of the most important elements in the wing pattern of many Lepidoptera is the more or less concentric and symmetrical system of bands, composed of variously coloured scales, running transversely across the wing. The determination of the central symmetry system has been studied in *Ephestia* by noting how the pattern elements are accommodated when small burns are made in the developing wing epithelium. A comparable examination by K. Henke (*Biol. Zbl.*, 1944, **64**, 98) of the wings of the Saturniid *Antheræa pernyi* revealed that the proximal and distal zones of the symmetry system are already partly determined in the larva. The inner bands are determined successively during the pupal period, there being no less than four, probably concentric, " determination fields," one for dark and the others for white scales. As in *Ephestia*, the embryological picture built up from evidence of this kind is that of successive waves of some necessary precursor spreading outwards from the centre of the symmetry system.

Although diffusion processes of this type have been freely invoked in explaining the origin of scale colour patterns, their reality has never been proved. A step in this direction has been

taken by G. Stehr (*Rev. Suisse Zool.*, 1947, **54**, 573) who has studied the relation between the circulation of hæmolymph and the scale pattern in the *Ephestia* wing. Blood can enter or leave the pupal wing through two main lacunæ, an anterior radial lacuna and a posterior axillary lacuna. Injections of indian ink showed that blood flows into the wing through the radial lacuna, then passes along the anterior margin, backwards across the breadth of the wing in the neighbourhood of the imaginal cross-vein, finally returning to the body cavity through the axillary lacuna. The direction of circulation is occasionally reversed. The proximal and distal regions of the wing are served by blood which diffuses outwards from the main backward stream in the cross-vein area. As Stehr points out, this region corresponds very closely with the axis of the future symmetry system. These relations are therefore quite consistent with the view that "pattern-forming" substances diffuse from the centre of the wing and particularly from a point near the anterior margin. They would also explain why the pattern bears little relation to the course of the main veins in the adult wing, since the disposition of the latter is markedly different from that of the pupal lacunar system.

Unlike the scale colour pattern, the arrangement of the scales themselves on the wing membrane may already be completely determined in the pupa. Some supporting evidence is advanced by K. Henke (*Biol. Zbl.*, 1946, **65**, 120). Moths that have been starved as larvæ have forewings which are small because there are fewer epidermal cells, although these are normal in size. The same ratio of epidermal cells to scales is preserved whatever the wing size. In the hind wing the ratio is different, but it also remains constant irrespective of the total size of the wing. Henke argues that this is a reflection of the orderly sequence of cell divisions undergone by the primordial cells, each of which yields a fixed number of epidermal, socket-forming and scale-forming cells. In the short-winged mutant of *Ephestia* (*kurzflügelig*) the cells are again of normal size, although fewer (H. Querner, *Nach. Akad. Wiss. Gött., Math.-Phys. Kl.*, 1947, 30). Since the gene affects only mitotic activity, the scale arrangement is normal. But the scale colour pattern, determined at a supra-cellular level, is disturbed. There are, for example, fewer dark scales in the cross-bands (K. Haardt, *Biol. Zbl.*, 1943, **63**, 478).

In his very thorough analysis of the genetic control of wing development in *Drosophila*, C. H. Waddington (*J. Genetics*, 1940, **41**, 75) has shown that besides mitotic activity mechanical processes, such as the degree of contraction of the wing from an inflated

condition, are important in determining wing shape. However, when the effect is slight, these processes are not easy to distinguish. A useful method employed by K. Henke, E. Schatz and H. Schwenk (*Nach. Akad. Wiss. Gött., Math.-Phys. Kl.*, 1946, 5) consists in plotting cell densities as contours on the wing surface. Temperature shocks at an early stage in pupal life produce wings that may be either short and fat or long and thin. As the maps of such wings show that the contour patterns are nearly normal, the characteristic shape probably arises through excessive mitotic activity along one or other of the two principal wing axes. In the *dumpy* mutants, on the other hand, the density contours are crowded together.

A more profound modification in wing shape is seen in those insects that possess rudimentary wings. The mechanism of reduction has been studied in the beetle *Ptinus tectus* (C. H. Waddington, *Proc. Zool. Soc. A*, 1942, **112**, 13) where the principal agency at work is the necrosis of cells throughout the wing epithelium. The same problem has been examined recently by H. Nüesch (*Arch. J. Klaus-Stiftung*, 1947, **22**, 221) in the Psychids *Fumea* and *Solenobia*. In these moths the male wings are of normal size while those of the grub-like females are exceedingly reduced. Wing development in the female commences at the same time as in the male, but the definitive growth stages are reached much later in larval life. As in *Ptinus*, however, rudimentation is brought about mainly by widespread autolytic degeneration of the epidermal cells in the pupa. In *Solenobia*, intersexes arise through crossing females of the tetraploid parthenogenetic race with males of the diploid bisexual race; they possess wings of intermediate size. Using this embryological evidence for an examination of the current theories of intersexual development, Nüesch can detect no sign of a " switch-over " (*Drehpunkt*) from the type of development characteristic of one sex to that of the other, as Goldschmidt had concluded for the *Lymantria* intersexes. On the contrary, the male and female determinants appear to act simultaneously, a weakened sexual differentiation resulting. This is the type of intersexual development described by Balzer in the Gephyrean *Bonellia*.

Locust Control from the Air.—The outbreak centres of many of the more important species of locusts are situated in remote and inaccessible country. Therefore, although ecologists have predicted that control measures would be most economical and effective if applied within these areas, this ideal has rarely been achieved. The object of a recent extensive series of investigations in Kenya and Tanganyika (D. L. Gunn, *Nature Lond.*, 1948, **161**, 342) has

been to decide on the practicability of using aircraft in attacking roosting and flying swarms.

Dusts containing DNOC (dinitro-*o*-cresol) were unsatisfactory against the Desert Locust (*Schistocerca gregaria*), as their effectiveness varied enormously with the atmospheric humidity and their delivery was also difficult to control. More promising results were obtained with DNOC sprays, either as an aerial curtain against flying swarms or as a ground spray against roosting locusts.

An important factor affecting the vulnerability of roosting swarms was the behaviour of the locusts themselves, which is described by D. L. Gunn, F. C. Perry *et al.* (*Anti-Locust Bull. No. 3*, 1948). In the Kenya Highlands, Desert Locusts roosted mainly in trees and bushes where they were comparatively protected against sprays delivered from the air. Shortly after sunrise the locusts began to descend from the trees and bask on the ground. Gradually the numbers of insects making short flights increased until the majority finally streamed away in one direction about two and a half hours after sunrise. It is concluded that the spray run should be timed to coincide with the period when most locusts were basking. The possibility of predicting the time of stream-away from the meteorological conditions is discussed.

While these studies were in progress the International Red Locust Control Organisation reported from Tanganyika that some swarms of this locust (*Nomodacris septemfasciata*) had escaped from the outbreak areas in the Rukwa Valley and were likely to menace the food-producing areas of Northern Rhodesia. The ensuing experimental campaign, in which aircraft with specially designed equipment were used, is described by D. L. Gunn, H. A. F. Lea *et al.* in a joint publication by the Anti-Locust Research Centre, London, and the Locust Control and Research Centre, Pretoria, 1948. The adult locusts were attacked with BHC (benzene hexachloride) and DNOC sprays. The latter, when suitably applied, proved highly effective and resulted in a complete mortality at a dosage of 1 gal./acre. The meteorological conditions and the situation of the swarms, which were roosting on grasses, were favourable, however. And it is suggested in the light of experience gained in this campaign that roosting locusts might be attacked more economically by equipment mounted on suitable trucks, while aircraft could be best used in destroying swarms in flight.

NOTES

First Arthur Stanley Eddington Memorial Lecture (**H. D.**)

"When Sir Arthur Eddington died," writes Professor C. E. Raven in the Introduction to this Lecture,* "we realised not only that a great mathematician and physicist, a great Cambridge man, a great Christian and a great Friend had been taken from us, but that here was a man whose work was vital to the present situation, and that as such we could best commemorate his memory by the traditional means of inaugurating a lectureship dealing with some aspect of his own particular contribution to thought." This idea had been given practical effect by the raising of a Fund under the management of a Board of four Trustees, appointed by the Royal Society, Trinity College, Cambridge, and the Society of Friends—three bodies which together cover the major part of the field of Eddington's wide interests. The Memorial Lectures, of which the booklet before us is the opening one, delivered on November 4, 1947, are to deal with some aspect of contemporary scientific thought considered in its bearing on the philosophy of religion or of ethics. It is hoped that they will thus help to maintain and further Eddington's concern for relating the scientific, the philosophical and the religious methods of seeking truth and will be a means of developing that insight into the unity underlying these different methods which was his characteristic aim.

Professor Ritchie has given a notable opening to what, it may be hoped, will become a memorable series of lectures, worthy of its high object. He deals with the fundamentals of Eddington's philosophy, and the Lecture should be of value to those who wish to grasp the significance of one of the most profound thinkers of modern times.

National Museum of Wales Annual Report, 1947–48 (**A. B.**)

Increasing public interest in our National Museums and Galleries since the war provides evidence of a wider appreciation of the

* *Reflections on the Philosophy of Sir Arthur Eddington*. By A. D. Ritchie. [Pp. x + 38.] (Cambridge: at the University Press, 1948. 2*s*. net.)

educational advantages of the national treasures, and of the special exhibitions which are arranged from time to time. The Annual Report of the National Museum of Wales for 1947–48 covers a period of unusual activity in that institution. Post-war rehabilitation and reorganisation have been further consolidated and the various Departments, devoted to Geology, Botany, Zoology, Archæology and Art, have each shown much progress.

Perhaps the most notable advance has been the opening in July last of the Welsh Folk Museum through the munificence of the Earl of Plymouth's gift of St. Fagan's Castle and grounds in 1946. This Elizabethan mansion is built within the curtain wall of a thirteenth-century fortress and is admirably suited to the display of period objects transferred from the existing Folk Life Collections. The interior has already been provided with some superb bed-hangings, tapestries and furniture, reflecting the arts and crafts of the time, and the Great Kitchen has been furnished with typical exhibits ranging from the sixteenth to eighteenth centuries. No doubt in time we may see the addition of agricultural implements, old-type vehicles and workshops with their tools arranged in an open-air setting in the grounds.

This important development in Museum provision will be watched with much interest. The need for Folk Museums illustrative of the domestic life of the people in the past has long been felt and good examples of the products of communal arts and crafts are becoming rare. It is to be hoped that the remarkable beginning now made in Wales will ultimately be followed by similar Museums in England and Scotland devoted to each country's national culture.

By a fortunate arrangement the Annual Conference of the Museums Association was held in the Museum during the year and upwards of 200 delegates visited the New Folk Museum. A further interesting development was the inauguration of a National Museums School Service involving the employment of a staff of officers, to be trained in the Museum, to carry on the visual educational work of which the burden has been borne hitherto by senior members of the Museum staff. The scheme is backed by the Ministry of Education and is suited particularly to Welsh needs.

Several temporary exhibitions were held, including the display of Princess Elizabeth's wedding gown, which was seen by over 100,000 persons. The largest exhibition was probably "Design Fair," arranged in April by the Council for Industrial Design. This type of exhibition is a valuable aid to industry at a difficult time in the country's recovery programme. It also brings publicity to a Museum, but as with the "Britain Can Make It" exhibition,

held in London in 1946, the provision of Museum accommodation for such a purpose is sometimes attended with risk to the development of the permanent collections. We are confident that in the present instance the risks, if any, were carefully considered and either eliminated or reduced to a minimum, thus carrying on the very excellent and efficient work which has been associated with the Directorship of Sir Cyril Fox, who retired on September 30, 1948, and was succeeded by Dr. D. Dilwyn John, of the British Museum (Natural History).

At a time when the National Museums are experiencing great difficulties in organisation and staffing it is welcome news to hear of the very good progress made in Wales.

Honey Poisoning in New Zealand (F. N. H.)

Honey that proves harmful or poisonous to human beings has been recorded from many countries in the past, including south-eastern Europe, North America, South Africa, Ceylon and Japan. Recently, particularly severe cases of honey poisoning have been recorded in New Zealand.

In Europe honey poisoning has been known from classical times, Xenophon's soldiers having suffered from it during the retreat of the Ten Thousand, the plant responsible being considered to have been *Rhododendron* (*R. ponticum*). This species is freely cultivated and naturalised in Britain. Hive bees do not as a rule pay much attention to the flowers, but they are much frequented by bumble bees, better able to reach the nectar with their longer tongues. Some years ago a doctor in Nottinghamshire had to treat boys suffering from vomiting, purging and abdominal pains. He discovered that they had been robbing bumble bees' nests and considered that honey derived from Rhododendrons may have been the cause of the trouble. This is one of the few suspected cases of honey poisoning in Britain.

In New Zealand the shrub known as "tu tu" (*Coriaria arborea*) is now known to be a source of poisonous honey. In 1945 a severe outbreak took place when honey (extracted honey) from one particular apiary near Pongakawa, North Island, caused poisoning by all who partook of it, some of the victims becoming serious hospital cases, suffering from vomiting, unconsciousness and abdominal pains, also temporary loss of memory in some instances. Most of the honey crop in question fortunately had not been consumed and was retrieved. It has formed the subject of valuable and interesting investigations in New Zealand ("A Recent Outbreak of Honey Poisoning," T. Palmer-Jones, M. D. Sutherland, C. R.

Paterson, W. F. Harris, D. W. Filmer. *N.Z. J. Sci. Tech.* **A.29**, No. 3 (1947), 107–43). Among the various test animals used, guinea-pigs were found to be very susceptible, rats slightly so, while pigs, sheep, rabbits, mice and bees were unaffected. A substance of formula $C_{15}H_{18}O_7$, for which the name *mellitoxin* has been suggested, was isolated and was considered to be the toxic principle or the main toxic principle present in the honey. It is one of the picrotoxin class of poisons. The same toxic principle was found to be present in honey dew secreted by a leaf hopper (*Scolypopa australis*) found on the plant. This is highly significant as hive bees often make use of honey dew if flower nectar is scarce. The pollen from the plant was considered to be non-toxic.

Investigations on Growing Pyrethrum in New Zealand * (F. T.)

The flowers of pyrethrum (*Chrysanthemum cinerariifolium*) have been used as an insecticide for a very long time. Gnadinger reports that this species superseded other varieties for this purpose as long ago as 1840. Since little was known about the nature of its active principles, a good deal of superstition surrounded its use, and some entirely erroneous beliefs about its cultivation, harvesting and mode of action were prevalent. The researches of Staudinger and Ruzicka heralded in a new period and from 1924 onwards research on the chemistry of its active principles, means of evaluation, cultivation, selection, methods of compounding, use and mode of action has literally filled volumes. The work continues for three very good reasons; the active principles are characterised (1) by extremely high potency, (2) they are harmless to man, (3) their speed of action surpasses that of all other insecticides.

Although great headway towards the synthesis of the pyrethrins and cinerins has been made, these compounds are complex and are likely to present for some time ahead difficulties to their large-scale production. The flowers can be grown relatively easily and are hardy, but, in countries where labour is dear, the cost of harvesting constitutes a limiting factor to production. This accounts for the fact that the main areas of cultivation have been countries like Kenya and Japan where low-priced labour has been up to now relatively abundant. But it also accounts for the type of study shown in the two papers under review. Mechanical harvesting

* II. "Selection of Clones: Their Variation in Habit of Growth, Susceptibility to Root-rot, Yield and Pyrethrin Content of Flowers." By E. E. Chamberlain and P. J. Clark. III. "Factors influencing Pyrethrin Content of Flowers." By P. J. Clark, E. E. Chamberlain and C. H. Proctor. (*N.Z. J. Sci. Tech.*, **A.29**, No. 4, (1947), 215, 223.)

seems a *sine qua non* if countries with a high standard of living, such as New Zealand, England and the United States, are to be able to produce pyrethrum flowers on an economic footing. A few experiments upon seedlings quickly demonstrate the variability in habits of growth of this plant and in the yield and quality of its flowers. High pyrethrin content per acre, freedom from foot-rot, the right habit of growth and one suitable for mechanical harvesting are the characteristics which research workers have been seeking in recent years in pyrethrum, since once seedling plants with these properties are obtained they can be propagated readily by root cuttings and desirable clones thus established. These have been the specific aims of the authors of this group of papers emanating from New Zealand.

Selections out of 39 clonal lines of pyrethrum were made and two of these were chosen, largely because of freedom from root-rot, one of the limiting factors to production on certain types of soil; but these plants, although showing a difference in habits of growth such as compactness and length of flower stem, possessed a high percentage pyrethrin content in both cases. The less-compact clones tend to lodge and thus give rise to dirty flowers and probably to fungus trouble and would present difficulties to mechanical harvesting. The ideal pyrethrum plant would combine some of the properties of each of the two main clones selected, and it is greatly to be hoped that they will be kept in being for purposes of further crossing and selection.

The authors of these papers confirm the findings of other workers on such important matters as the right time of harvesting the flowers, the increase in the pyrethrin content up to the fully open stage; in addition, a study of artificial drying and the effect of the temperature and the time employed is dealt with. It is regrettable, however, that so useful a piece of work should not have included a critical statistical analysis of the results obtained, and if further work on these lines is envisaged—and it would be warranted—it should be planned from the start on a statistical basis.

"La Revue de Géographie Humaine et d'Ethnologie" * (L. D. S.)

This new French journal, under the editorship of Pierre Deffontaines and André Leroi-Gourhan, is planned as a quarterly, but the first two numbers (January–March and April–June, 1948) are separately priced and no subscriptions are at present accepted. It is greatly to be hoped that the venture will prove successful,

* No. 2, April–June, 1948. [Pp. 128, illustrated.] (Paris: Gallimard. Frs. 320.–.)

as the standard is very high both in matter and production. In the second number scholarly, well-documented articles, adequately illustrated by maps, diagrams and pictures in the text, range from plant geography in North America to recent population movements in Europe, and cultural geography of Africa. Nearly half the volume is devoted to a chronicle of proceedings of scientific meetings, a résumé of discussions of current problems, varied information, and reviews. The new journal covers a wide field, and is most effective.

International Book Token Scheme

Arrangements have now been completed for the sale in this country of book coupons under the Unesco International Book Coupon scheme. As the amount of dollars available is limited, coupons will be allocated on a priority basis, and the needs of educational, scientific and cultural institutions serving a large public will be given first consideration. The coupons are held by the distributing agency in the United Kingdom, Book Tokens Limited, of 28–30 Little Russell Street, London, W.C.1, from whom full particulars of the scheme may be obtained.

The purpose of this Unesco scheme is to overcome foreign exchange difficulties and so enable appropriate organisations and individuals in one country to buy publications on education, science and culture from another. In particular, it will make possible the purchase of suitable publications from "hard" currency countries such as the United States. Purchases will be made in the national currency of the countries concerned and Unesco will supply the necessary dollars.

Prevention of Deterioration Abstracts

The National Research Council of the National Academy of Sciences (Prevention of Deterioration Center, Room 204), 2101 Constitution Avenue, Washington, D.C., offers the "Prevention of Deterioration Abstracts" on a yearly subscription basis. These Abstracts are classified under the following headings: Biological Agents; Electrical and Electronic Equipment; Fungicides and Other Toxic Compounds; Lacquers, Paints and Varnishes; Leather; Lubricants; Metals; Miscellaneous; Optical Instruments and Photographic Equipment; Packaging and Storage; Plastics, Resins, Rubbers, and Waxes; Textiles and Cordage; Wood and Paper. Cross-references are included in each issue; author and subject indexes are compiled at the conclusion of each volume. Material for the Abstracts is obtained from journal articles, patents,

and unpublished reports from government, university, and industrial research groups in the U.S.A. and other countries.

Approximately 2000 pages are published a year, in two volumes of six issues each. The individual abstracts are in loose-leaf form so that they may be arranged in any manner desired by the individual receiving them. Comments are added to many of the abstracts to correlate relevant information, to evaluate reports, or to make suggestions for further research.

The yearly subscription rate, which includes two sturdy binders and index guides, is at present $37.50. The rate will be $50.00 for requests received after July 1, 1949. All subscriptions run from July 1 to June 30. Back issues are available from April 1946, when the series was started.

An " Advance List," a monthly bibliography of all the reports received in this field by the Prevention of Deterioration Center, is also available for an additional $10.00 per year.

Miscellanea

The New Year Honours List contained the names of the following scientists and others associated with scientific work : *Baron* : Sir John Boyd Orr, lately director-general of the Food and Agriculture Organisation of the United Nations. *Privy Councillor* : Lord Hailey, chairman of the Colonial Research Committee. *G.C.B.* : Sir Henry Tizard, chairman of the Defence Research Policy Committee, Ministry of Defence. *G.B.E.* : Sir Wilson Jameson, chief medical officer, Ministry of Health and Ministry of Education. *D.B.E.* : Miss Myra Curtis, principal of Newnham College, Cambridge, and lately chairman of the committee on children deprived of a normal home life. *Knights* : Prof. J. D. Beazley, professor of classical archæology, University of Oxford ; Prof. H. Cohen, professor of medicine, University of Liverpool ; A. J. Gill, assistant director-general (engineering), and engineer-in-chief since 1947, of the General Post Office ; Dr. H. L. Guy, chairman of the Mechanical Engineering Research Organisation, Department of Scientific and Industrial Research ; Dr. W. G. Ogg, director of Rothamsted Experimental Station ; Dr. R. E. Priestley, vice-chancellor of the University of Birmingham ; Dr. J. L. Simonsen, director of research, Colonial Products Research Committee ; Prof. S. A. Smith, dean of the Faculty of Medicine and regius professor of forensic medicine, University of Edinburgh ; W. L. Taylor, lately director-general of the Forestry Commission ; Capt. F. O'B. Wilson, chairman of the Board of Agriculture, Kenya. *C.B.* : Air Commodore A. H. Robson, director of educational services, Air Ministry. *C.M.G.* :

V. Boyle, superintending inspector, Animal Health Division, Ministry of Agriculture; Brigadier M. Hotine, director of Colonial Surveys, and adviser on surveys to the Secretary of State for the Colonies; Dr. J. M. Stewart, formerly vice-chancellor of the University of Adelaide. *C.B.E.*: B. C. Aston, of Wellington, New Zealand, for services to agriculture and botany; O. V. S. Bulleid, chief mechanical engineer, Southern Region, Railway Executive; Prof. J. B. Cleland, formerly professor of pathology, University of Adelaide; F. S. Collier, chief conservator of forests, Nigeria; Colonel W. C. Devereux, managing director of Aluminium, Ltd., and chairman of International Alloys, Ltd.; I. G. Evans, assistant secretary, Department of Scientific and Industrial Research; Dr. J. Hammond, reader in agricultural physiology, University of Cambridge; J. McDonald, director of agriculture, Cyprus; Prof. F. H. Newman, professor of physics, University College of the South-West of England, and chairman of Exeter Joint Recruiting Board; Miss M. H. Read, head of the Colonial Department, Institute of Education, University of London; Lieut.-Col. W. Campbell Smith, deputy chief scientific officer and keeper of mineralogy in the British Museum (Natural History); Dr. O. J. Voelcker, director of the West African Cacao Research Institute; Prof. W. Wardlaw, professor of physical chemistry, University of London (Birkbeck College), scientific adviser, Appointments Department, Ministry of Labour; R. T. B. Wynn, assistant chief engineer, B.B.C.

At the anniversary meeting of the Royal Society on November 30, the following were elected as officers for the ensuing year: *President*, Sir Robert Robinson; *Treasurer*, Sir Thomas Merton; *Secretaries*, Sir Edward Salisbury and Prof. D. Brunt; *Foreign Secretary*, Prof. E. D. Adrian.

The two Royal Medals of the Royal Society for 1948 were awarded to Prof. Harold Jeffreys, for his work in geophysics and his important contributions to the astronomy of the solar system, and to Prof. James Gray, for his researches in cytology, ciliary movement, and particularly his anatomical and experimental studies of animal posture and locomotion. The following medals were also awarded: Copley Medal to Prof. A. V. Hill, for his researches on myothermal problems and on biophysical phenomena in nerve and other tissues; Rumford Medal to Prof. F. E. Simon, for his contributions to the attainment of low temperatures and to the study of the properties of substances at temperatures near the absolute zero; Davy Medal to Prof. E. L. Hirst, for his work in the determination of the structure of sugars, starches, plant gums and especially

of vitamin C; Darwin Medal to Prof. R. A. Fisher, for his contributions to the theory of natural selection, the concept of the gene complex and the evolution of dominance; Hughes Medal to Sir Robert Watson-Watt, for his work in atmospheric physics and in the development of radar.

The Nobel Prize for Physics for 1948 was awarded to Prof. P. M. S. Blackett, F.R.S., the Nobel Prize for Chemistry to Prof. Arne Tiselius, and the Nobel Prize for Medicine to Dr. Paul Müller.

Sir Harold Spencer Jones, Astronomer Royal, has been awarded the Catherine Wolfe Bruce Gold Medal for 1949 of the Astronomical Society of the Pacific, for distinguished services to astronomy.

Sir Edward Appleton, secretary of the Department of Scientific and Industrial Research, has been appointed principal and vice-chancellor of the University of Edinburgh. He has also been awarded the Valdemar Poulson Gold Medal of the Danish Academy of Technical Sciences, for his outstanding contributions to radio technics and particularly for remarkable achievement in research on the ionosphere.

Dr. Jaime Torres Bodet, Mexican Foreign Minister, has become director-general of Unesco in succession to Dr. Julian Huxley.

Mr. G. S. Carsdale has been appointed superintendent of the Zoological Society's Gardens in Regent's Park, London, in succession to Dr. G. M. Vevers.

We have noted with great regret the announcements of the death of the following scientific workers: Dr. S. C. Bradford, formerly chief librarian of the Science Library, South Kensington; Mr. J. H. Coste, formerly chemist-in-chief to the London County Council; Dr. F. H. S. Curd, of Imperial Chemical Industries, Ltd.; Prof. W. J. Dann, professor of nutrition at the Duke University School of Medicine, Durham, North Carolina; Dr. L. Doljanski, head of the Department of Experimental Pathology, the Hebrew University, Jerusalem; Mr. R. Murdin Drake, O.B.E., joint manager of the Association of British Chemical Manufacturers; Mr. R. Elmhirst, director of the Scottish Marine Biological Laboratory at Millport; Prof. L. Farkas, professor of physical chemistry in the Hebrew University, Jerusalem; Sir John Fryer, K.B.E., F.R.S., secretary of the Agricultural Research Council; Mr. C. A. Hill, founder of the British Drug Houses, Ltd.; Prof. Johan Hjort, For. Mem. R.S., formerly Director of Norwegian Fisheries, and later professor of zoology in the University of Oslo; the Right Hon. J. H. Hofmeyr, chancellor of the University of the Witwatersrand; Mr. J. E. Kingsbury, a founder of Standard Telephones and Cables, Ltd.;

Mr. Frank Nasmith, a founder member and president during 1938–39 of the Textile Institute; Mr. T. Petch, formerly Government mycologist, Ceylon, and later director of the Ceylon Tea Research Institute; Mr. E. Platt, geologist; Dr. M. C. Rayner (Mrs. W. Neilson Jones), formerly head of the Botany Department, University College, Reading; Dr. G. Scott Robertson, permanent secretary of the Ministry of Agriculture, Northern Ireland; Prof. G. Roussy, formerly rector of the Sorbonne and director of the Cancer Institute of the Faculty of Medicine, Paris; Dr. Marjory Stephenson, M.B.E., F.R.S., Medical Research Council Unit for Chemical Microbiology; Dr. H. J. van der Bijl, F.R.S., chancellor of the University of Pretoria; Prof. J. H. M. Wedderburn, F.R.S., professor emeritus of mathematics in Princeton University; Dr. C. M. Wenyon, C.M.G., F.R.S., formerly director-in-chief, Wellcome Research Institution.

We have received from Dr. S. Prát a list, dated 1948, of the living cultures of algæ, liverworts and mosses which are maintained in the Plant Physiology Institute of the Charles University, Prague. The list includes 7 Cyanophyceæ, about 100 species of green algæ, over 60 species of liverworts and over 50 species of mosses. It may be hoped that these collections of living cultures will be maintained and developed, so as to be able to serve the needs of research workers throughout the world.

All users of chemicals will welcome the 1949 edition of *British Chemicals and their Manufacturers*, which has recently been published by the Association of British Chemical Manufacturers. This handy volume will be invaluable not only in industry, but also in University laboratories. The contents are divided as follows: Grouping of chemical industries; directory of members of the A.B.C.M.; classified list of British chemicals; classified list of indicators and microscopic stains; list of proprietary and trade names; list of proprietary and trade marks. Over half of the book is taken up by the list of chemicals, indicators and microscopic stains, and the makers of each substance are given. Copies are supplied gratis to anyone who may need the Directory in the course of his work.

We have received from H.M. Stationery Office a catalogue of publications of the Department of Scientific and Industrial Research (*Government Publications, Sectional List, No. 3*). This is revised to August 31, 1948. Applications for copies should be sent to the Director of Publications (Section SP), H.M.S.O., 429 Oxford Street, London, W.1.

The causes of many of the defects in modern concrete roads are described in a very comprehensive survey, *The Effect of the Soil Foundation on the Road Surface*, which has recently been published for the D.S.I.R. by H.M. Stationery Office. Many photographs, showing types of road failure that have occurred in the past, are included in the report.

In the press release from the D.S.I.R. for this paper it was pointed out that, judging from the writings of Telford and McAdam, the pioneer road engineers were very much aware of the importance of stable road foundations and took great care to study the conditions of the subsoil and to provide adequate drainage. The subsequent introduction of high-grade surfacings tended to obscure the importance of the soil foundation, particularly as many of the new roads were placed on top of old roads, which formed perfect foundations. It was only when, about 25 years ago, many miles of new road were built on virgin ground that the importance of the subsoil again became apparent.

The soil foundation is usually considered as consisting of two separate parts, the soil immediately beneath the road surface, termed the subgrade, which is highly stressed by the passing traffic, as well as by the road surface itself, and the lower layers, which are relatively unstressed by traffic loads, but have to support the subgrade and the road structure. In deciding the type of road structure to build, due allowance must be made for the strength of the particular subgrade, which may be a soft clay or a stable gravel or sand. Tests which have been devised to measure the shear and bearing strength of different kinds of soil are described in the paper. It is also important that due allowance should be made for changes in the strength of the subgrade in course of time, and it is here that engineers differ widely in their views. While many assume that little deterioration need occur, some base their design on the assumptions that the subgrade will deteriorate to the worst possible conditions. As an example of the latter, a test which is used in the U.S.A. measures the strength of the soil specimen after it has been soaked in water for several days.

Reduction in the supporting power of the subgrade has been found to arise from three main causes: the action of traffic, the ingress of moisture and the action of frost. The action of traffic is thought to cause a compacting effect, and it has been found in the U.S.A. that the vibration of aircraft was particularly effective in compacting the sand under runways built on sandy subgrades. Investigations of road failure due to ingress of moisture have shown that the most common mode of entry is through leaky joints and

cracks in concrete roads, and it has become clear that the road surface should act at all times as a waterproof covering to the subgrade. As regards frost, the experiences of the severe 1946–47 winter showed among other things that chalk, limestone and brick suffered more damage than other kinds of base, and that the thickness of roads constructed on frost-susceptible subgrades should be dependent on the depth of frost penetration when this exceeds the thickness required for stability to traffic.

J. P. Stephenson has prepared for Unesco an illustrated booklet called *Suggestions for Science Teachers in Devastated Countries*. The book is a very practical help for those who are forced to teach science without a cupboard full of expensive apparatus at their disposal. The book is being distributed free by Unesco to schools in Greece, Poland, Czechoslovakia, Austria, Hungary, Italy, China and the Philippines.

Chapter 4, which occupies well over half the book, describes how to construct many simple pieces of apparatus from everyday odds and ends. A model turbine is made from a short piece of glass tubing, the Davy lamp is demonstrated with a candle and a small piece of gauze, a piece of corrugated rubber tubing from an old gas mask serves to make a model aneroid barometer, while small accumulators are constructed using lead stripped from some electric cable. Suggestions are given in all the principal branches of Physics as well as in Chemistry, Biology and Astronomy.

The remainder of the book includes an account of the latest materials which are now available for use in the laboratory, a discussion on the use of visual aids in teaching and an account of other up-to-date methods of running a science class. The " Project " method, which is very popular in the U.S.A. and Scandinavia, is described in detail ; possible subjects are suggested and the results of one project which was tackled by boys of 13–14 years of age are described. The subject was " Air and Burning " and arose from a discussion during a Chemistry lesson. The idea was enthusiastically taken up by the class, who split up into nine groups to study different aspects of the subject, the work being mainly done in school after hours or at home. Great interest was aroused by the group which set out to make matches, and the boys as a whole had to be restrained from turning the project into one on fireworks. Information poured in from local libraries and other sources, and the project ended in a really interesting exhibition. While the use of libraries and visits to factories, etc., are a great help in developing any project, this method of teaching can still be

adopted on a less ambitious scale when these facilities are not available. Of more importance is the ability of the teacher to pick the right moment to suggest a project and then to be so enthusiastic about it himself that the interest of the class does not have a chance to flag.

When it was realised by the Bell Laboratories, shortly before the last war, that the potential supply of stable high-value wire-wound resistors would be insufficient to meet requirements, they turned their attentions to the development of an alternative which could be made from non-critical materials. The answer was the deposited carbon resistor, and an account of this development is given by A. C. Pfister in the *Bell Laboratories Record* for October 1948.

As the name suggests, this type of resistor is made by depositing a thin film of carbon on to a suitable core, the material chosen for the core being one of the ceramics. The carbon is obtained by the thermal decomposition of gaseous hydro-carbons at very high temperatures, a process very similar to that in which undesired carbon is deposited in internal combustion engines. This type of carbon had previously been used in the Bell Laboratories for microphones and also for making deposited carbon resistors for high-frequency work. This latter type of resistor, which was made in Europe prior to the war, is particularly well suited for work at high frequencies, as there is no skin effect, small reactance and an effective resistance which is independent of frequency.

The resistance of the deposited film is normally of the order of a few thousand ohms. For higher values in the megohm range a helical groove is cut through the carbon film, making in effect a carbon ribbon wound round a ceramic core. It was when attempts were being made to produce resistors in this higher range that the importance of the state of the ceramic surface became apparent. It was found that the resistance stability, the temperature coefficient and the random noise in excess of thermal noise were directly dependent on surface imperfections. A special type of ceramic had to be developed which had the required surface perfection and to which the carbon would adhere. It was soon learned that the surface, besides being physically perfect, had to be kept scrupulously clean, and special arrangements were made for handling the ceramic cores after their final firing.

It is indicative of the successful solution of the problem of surface cleanliness and perfection that, with a carbon film only a few millionths of an inch thick, the value of the resistance obtained agrees with the computed value to within the accuracy of measure-

ment. After the carbon has been deposited and the helical groove has been cut electrodes are fitted and a baking varnish applied. Resistors of this type were supplied to an initial accuracy of within 1 per cent. and in certain sealed apparatus assemblies were found to be stable to within 1 per cent. per year.

With the immediate requirement fulfilled, the stability was further improved by sealing the resistor element inside a glass tube, the variations in resistance being reduced to as low a value as 0·005 per cent. per year. Comparison of these glass-sealed carbon resistors with the best obtainable wire-wound resistors showed that the stability of a 10-megohm carbon resistor is better than that of a 2-megohm wire-wound resistor. However, although the carbon resistor can dissipate 10–20 watts of power without permanent change, its temperature coefficient is larger than that of the wire-wound type.

A very convenient method of representing a crystal's structure is described in the same issue of the *Bell Laboratories Record.* A pair of drawings are made, each showing a three-dimensional diagram of the crystal's structure. When these drawings are viewed through a stereoscope, as clear an impression of the crystals structure is obtained as with a physical model, the only disadvantage being that to obtain other views of a particular crystal further drawings have to be prepared.

In the drawings different atoms are represented by circles of different size, the circles being joined by straight lines. The original drawing is made on a piece of cellulose acetate which is then used to make numbers of contact prints. Any three-dimensional data can be stored in this way, which has the advantage of being easy to handle and requiring little storage space.

Ever since ancient Egyptian times the use of solder has been one of the most convenient methods of joining two pieces of metal together, and nowadays it is being used by the amateur and big industrialist alike. A book " intended to help in solving the practical difficulties which may arise in soldering operations," called *Notes on Soldering*, was issued free by the Tin Research Institute at the beginning of 1948 and contains useful information on all branches of the subject.

In the introduction, the nature of a soldered joint is discussed. Examination under a microscope reveals that at the joint the metal surface and the tin in the solder combine to form an intermetallic compound phase. This phase continues to grow if the joint is maintained at the soldering temperature and, as noted later in the book, the idea that it is advantageous to let molten solder

soak into a joint is incorrect. What in fact happens is that the intermetallic phase grows into large crystals which tend to make the joint brittle.

A section is devoted to a description of the basic principles of soldering and contains a comprehensive account of the different fluxes which can be used, the metals for which they are most suitable and much other useful information. Another section describes laboratory tests which have been devised to measure the properties of different solders and fluxes when used with different metals. In one of these tests a pellet of solder is placed on a 1-inch square of metal and melted by means of a special soldering iron. The area over which the pellet spreads, measured by means of a planimeter, indicates the spreading power of the solder under the conditions of the experiment. To measure the ability of solder to flow into narrow spaces, a very simple method has been suggested by Schumacher and others. A pair of wires are twisted together and dipped into the molten solder. After 15 seconds the wires are withdrawn and the height to which the solder has climbed is measured. It is interesting to read that even if the solder only contains 15 per cent. of tin (the balance being lead), the composition of that part of the solder which rises to the greatest height contains much more tin and is almost of eutectic composition (63 per cent. tin). This is due to the constituents of higher melting point freezing out as they reach the cooler upper levels.

Solder has never been used so much as in the last few years, and many new techniques have been developed. For the production of can bodies for canned food a fully mechanical process is used. The edges of the metal are covered with flux and bent to form a locked seam. This seam is then drawn over a slowly rotating horizontal roller, the lower half of which is dipping into a bath of molten solder. Solder is thus brought into contact with the prefluxed seam and the joint is made, any surplus solder being automatically removed by means of a buff. In the motor-car industry solder is used to smooth over small dents and imperfections in the body, the solder being applied by a gas torch or a special spraying process.

The author has covered many more aspects of the subject than it is possible to describe here, including the art of making joints in lead pipes, the soldering of special metals and an account of the properties of solders and soldered joints. The book is very readable, contains many photographs and illustrations, and should prove a useful guide and source of information for those interested in the use of solder.

ESSAY REVIEW

ISOTOPES AND THE BIOLOGIST. By G. Popják, M.D., National Institute for Medical Research, Hampstead. Being a Review of **Radioactive Indicators: Their Application in Biochemistry, Animal Physiology and Pathology,** by George Hevesy. [Pp. xviii + 556, with 97 figures and chart in pocket.] (New York and London: Interscience Publishers, Inc., 1948. 60*s*. net.)

The scientific world owes much to Professor Hevesy for initiating the use of radioactive isotopes in the study of biological phenomena. In 1923 and 1927 Hevesy and his co-workers reported the measurement of uptake of lead by plants, and the absorption and excretion of lead and bismuth by animals using the naturally radioactive isotopes of lead, ThB (Pb^{212}), and RaD (Pb^{210}) and of bismuth, RaE (Bi^{210}). It is significant that at that time isotopes of the biologically important elements were not available and it must have been the intuitive foresight of the great scientist which made those investigations seem worth while. Later, after the discovery and concentration of deuterium, it was again Hevesy who in 1934 applied heavy water for the first time to study water metabolism in man. Hevesy's contributions to the study of various problems of phosphorus metabolism with the aid of P^{32} are also very considerable. It was, therefore, pleasurable news to learn that Professor Hevesy was writing a book on the application of radioactive isotopes. The publication of this volume fully realises our expectations.

Recently there appeared a number of books dealing with the same subject, *e.g. Radioactive Tracers in Biology: An Introduction to Tracer Methodology*, by M. D. Kamen, Academic Press, New York, 1947; *A Symposium on the Use of Isotopes in Biology and Medicine*, University of Wisconsin Press, 1948; *Advances in Biological and Medical Physics*, Vol. I, edited by J. H. Lawrence and J. G. Hamilton, Academic Press, New York, 1948; and *Radioactivity and Nuclear Physics*, by J. M. Cork, D. Van Nostrand Co., New York, 1947; the last publication being almost entirely devoted to physics. Hevesy's book differs from all these in several aspects.

We may enquire at this point what should be the aims of books

of this type in the present state of development of isotope application. Until recently there were no concise accounts of isotope techniques available to the uninitiated biologist who wished to start in this field, and information had to be gathered from articles scattered in journals. To employ isotopes intelligently for the study of biological problems, it is necessary that the biologist should be familiar with at least the basic principles of nuclear physics, with the special methods of isotope application, with methods of assay, and further, in respect to radioactive isotopes, with the health hazards involved in handling the latter. These demands put the authors to a severe task; not only are they expected to show, with practical examples, the application of isotopes, but also to give instruction in physics to the biologist in easily understandable language.

The publications cited above fall mainly into this category, and the scientific investigator will find all the theoretical and practical instructions relating to isotope work in one or the other.

There is another course, however, open to the authors, and this arises from the fact that knowledge acquired by the use of isotopes is in many aspects so revolutionary that, if recent progress in many of the biological fields is to be presented, this amounts mainly to a review of researches carried out with isotope techniques.

Hevesy's book belongs to this group and in it he has provided us with one of the most comprehensive surveys of results so far obtained in biochemistry, animal physiology and pathology with the aid of naturally and artificially radioactive isotopes. This is not to say that he has neglected entirely the practical side of tracer methods, but this appears only as a subsidiary matter of secondary importance in the volume. The first 81 pages contain chapters on, what might be called, tracer methodology, production of radioactive indicators, radioactive isotopes of possible interest in tracer work, and determination of radioactivity. Of these 81 pages, 20 are, however, occupied by the catalogue of radioactive isotopes supplied by the U.S. Atomic Energy Commission. To show the difference in emphasis in the two types of books, it will suffice to say that, for example, in the volume edited by Lawrence and Hamilton 32 pages are devoted to the nature and production of artificial radioactivity, 70 pages to fundamentals of radioactivity and instrumentation, and 62 to discussion of health hazards and protection against radiation, while the latter problem is hardly even mentioned in Hevesy's book. Hevesy assumes that his reader has already a basic knowledge of nuclear physics, without which the first chapter of 20 pages on the production of radioactive isotopes is hardly

intelligible. This is, however, not meant to be an adverse criticism, but merely to point out that, if the reader wishes to obtain primarily instructions in basic nuclear physics and in tracer methods, he should consult Cork, Kamen, Lawrence and Hamilton, or the publication of the Wisconsin University, but if he is desirous of having a comprehensive review of results obtained with radioactive tracers he will prefer Hevesy.

The body of Hevesy's book is contained in Chapters V–XII, where the results of various types of investigations to which radioactive isotopes have been applied are reviewed, *e.g.* chemical analysis; absorption, distribution and excretion of elements; permeability and turnover studies, etc. Within sub-divisions of each chapter the applications of the different elements to the one particular problem are discussed. In this respect Hevesy's presentation, aiming at a survey of results, differs again from, for example, Kamen's, who—with the emphasis on methods—shows in individual chapters the application of one isotope to several problems, together with a full discussion of the properties of the isotope in question.

Fundamentally there is no difference between the application of the stable and of the radioactive isotopes to biological problems; both types of indicators are merely tools for obtaining information. If, therefore, an author's choice is to give a survey of new knowledge acquired with tracer methods then—in the reviewer's opinion—the results obtained with the stable isotopes (D, C^{13}, N^{15}) cannot be omitted. It is to be remembered that real progress in tracer investigations dates from 1935, when Schoenheimer and Rittenberg announced for the first time the preparation of organic substances labelled with deuterium. Their subsequent investigations into fat, amino-acid and protein metabolism will always remain the classics of tracer research. Apart from an occasional reference, there is no place in Hevesy's work for the discussion of the very important results obtained with the stable isotopes. The author's remarks in the preface indicate that he is aware of this gap caused by his choice of topic. This seems to be the only shortcoming of an otherwise excellent book, well written and amply documented with tables and figures taken from the original publications. It seems a trivial matter to mention now that with more careful proof-reading some glaring misprints (like " chronic systiomastitis " for " chronic cystic mastitis "; " thyrosine " for " tyrosine "; " intestatial " for " interstitial "; " interperitoneal " for " intraperitonal " and others) might have been avoided. Nevertheless, the publishers are to be commended for the clear type used in printing and for the excellent layout of the whole volume.

REVIEWS

MATHEMATICS

Introduction to the Algebraic Geometry of a Plane. By J. W. ARCHBOLD, M.A. [Pp. xiv + 300, with 108 figures.] (London: Edward Arnold & Co., 1948. 25*s.* net.)

THIS is an admirable and much needed book. It provides the student with an account of all the plane geometry that he needs for the average honours course and, indeed, provides an excellent scheme for such a course. In the reviewer's opinion, it is the only existing single book that does this.

The book is in the main on general projective geometry of the plane. The author approaches it by generalisation of the euclidean plane. His approach has much to recommend it by permitting the student to use what he already knows, by giving him the feeling which he likes to have, whatever its justification, that he is doing *geometry* and not merely algebra clothed in geometrical terms, and by making it appear natural at the appropriate stage to revert to such topics as the metrical theory of conics and the metrical forms of collineations. Both the generalisations and specialisations are all the more readily dealt with by virtue of the early introduction of simple ideas on *groups* and *fields*.

The first three chapters deal with *foundations, projective transformations and linear geometry, projective theory of conics*. Then comes the chapter on the *metrical theory of conics*, followed by one on *further properties of conics* (harmonic envelope, outpolar and inpolar conics, etc.). Such a delayed treatment of " ordinary conics " should be no surprise to the student who is probably at the same time having an analogous experience in analysis, being required to contend with foundations, convergence, differentiability, and so on, before being allowed to proceed with " ordinary calculus."

There is then a substantial chapter on *collineations* leading up to the study of projective invariants. The reviewer knows of no comparably accessible account of this part of the work (at this level) which really comprehends the whole of projective algebraic geometry. The final chapter on *rational curves* is an excellent, though somewhat intensive, introduction to the subject. It is a distinctive feature of the book and is clearly the part intended for the student with special geometrical interests.

In spite of the multitude of ideas to which the reader is introduced, there are no loose ends. The essential coherence of the treatment is emphasised by the commendable expedient of concluding each chapter with a review showing clearly how all the strands come together. Perhaps the most special merit of Mr. Archbold's presentation is that it is calculated to impart a lot of genuine geometrical knowledge without requiring the reader to

possess any particular geometrical "kink." In a few years teachers of geometry will be wondering how they ever got along without this book.

W. H. McCrea.

Applied Differential Equations. By F. E. Rolton, M.A., D.Sc. [Pp. viii + 264, with 12 figures.] (London and Glasgow: Blackie & Son, Ltd., 1948. 20*s*. net.)

It is fortunately true, as the author states in his preface, that there have been moves to reform the teaching of mathematics to science students. The importance of an appreciation and awareness of mathematical technique and its possibilities has now been so widely realised that there is a strong urge for adequate provision of suitable mathematics courses for science and engineering students. This book makes a useful contribution to this end in providing an introductory course on differential equations for just such students.

For this purpose a chapter on Numerical Methods of Solution and one on the Method of Isoclinals have been included. Both are very welcome additions and receive an adequate and clear introductory treatment.

The method of presentation, the selection of material and the assignment of emphasis to the various subjects which are discussed—first order equations, linear equations with constant and with variable coefficients, simultaneous equations, Fourier series, partial differential equations, the method of isoclinals, numerical methods and equations in three variables—are well suited to the author's purpose. Thus it is particularly satisfactory to find the physically very important concept of the exact differential introduced at an early stage and discussed gradually and carefully with numerical illustration and relation to mechanics and thermodynamics.

A large number of well-chosen examples are included which should provide useful practice for the student.

The book is clearly written and can be unreservedly recommended as an introductory text for students specialising in physics, chemistry or engineering.

H. S. W. M.

Elements of Nomography. By R. D. Douglass, Ph.D., Sc.D., and D. P. Adams, M.A. [Pp. x + 209, with 111 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. $3.50.)

The authors state in their preface: "A few simple types of chart with correspondingly elementary theory, can and do make at the present time the major contribution of the subject to the engineering profession, industry, business and other pursuits."

The higher theory of nomography is not discussed in this book, which concentrates on a detailed and practical exploitation of the possibilities of the alignment diagram.

In the first chapters practical points concerning the reading and adjustment of uniform and non-uniform scales are discussed and illustrated with examples. Six simple types of alignment diagrams are then developed, *viz.*: three parallel lines equidistant (Type I) and not equidistant (Type II); two parallel lines and a slanting line (Type III); three lines passing through a point at angles 60°, 60°, 120° (Type IV) and at angles $\theta°$, $90° - \theta°$, 90° (Type V); and a circular chart (Type VI). The choice of Type, the size

of scales, the fitting of the scale-ranges into the available space and other practical points are stressed. Examples, mainly from chemical engineering, illustrate the procedures. The absorption of mathematical functions of *one* of the scales as a scale transformation is well brought out.

The combination of the simple Types to form "compound" charts is the subject of the last chapters; too little use is made here of arrangements in consecutive quadrants. Some detailed diagrams for the determination of roots of polynomials are drawn. (Note misprint in Fig. 50*b*—for V = U/W read V = W/U.)

H. O. Hartley.

The Differential Analyser. By J. Crank, M.Sc. [Pp. viii + 137, with 4 plates and 31 figures.] (London, New York, Toronto: Longmans, Green & Co., Ltd., 1947. 10*s*. 6*d*. net.)

The Differential Analyser is a calculating machine designed to solve differential equations. Based on the "integrating disk" first conceived by Kelvin, and an equally important amplifying mechanism (the torque amplifier) invented by Nieman, it was first designed and constructed in the United States by Dr. V. Bush. Pioneer work in this country was done by Prof. Hartree, who built the first model at Cambridge from Meccano parts and later designed larger analysers both at Manchester and Cambridge Universities. Quite a number of differential analysers, large and small models, are now in existence, the largest being the tape-controlled and electrically interconnected "giant" at the Massachusetts Institute of Technology in charge of Prof. Caldwell.

Apart from giving historical information and constructional details, the present booklet has two purposes, both briefly but admirably executed:

(*a*) To provide instructions and guidance to those actually running a differential analyser or planning work for it.

(*b*) To give some idea to the potential user as to the type of problem that can be tackled on the machine.

If a criticism may be made (*a*) has perhaps been given undue preference.

(*a*) The special technique of adapting differential equations to the analyser, entirely different from the analytical approach, is well brought out. The principles are fittingly illustrated by important examples. Naturally the choice of methods is partly dictated by the number of units, tables, etc., available on the Cambridge machines, on which the author did most of his work.

(*b*) This is mainly conveyed by the discussion of physical examples; notable among these is the "boundary layer equation." An interesting feature is the discussion of partial differential equations, which is reduced to a system of ordinary differential equations for which the analyser was primarily designed. The discussion of difficulties, such as, for instance, arise with highly oscillatory problems, might have been given more prominence.

H. O. Hartley.

Sequential Analysis. By Abraham Wald. [Pp. xii + 212, with 16 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 24*s*. net.)

The incentive for the development of this new statistical technique, known as "Sequential Analysis" or more particularly "Sequential Sampling," has

come from the war requirements of industrial quality control of mass-produced articles. Whilst in this country the theoretical technique was developed by G. A. Barnard and his colleagues working at the Ministry of Supply, the method was independently and more extensively developed in the United States, notably by the author of this book, which is mainly based on published work by him and his collaborators at Columbia University.

From the point of view of sampling inspection the difference between the classical method and sequential sampling is this: Whilst with the former the quality of a batch is judged from the number of defectives found in an inspected sample of a fixed size, with the latter sampling proceeds and a "current account" of the defectives is kept until a verdict of the required precision can be made, when sampling stops and the batch is either rejected or accepted. The procedure usually results in considerable saving of inspection.

The mathematical method of determining the "target-values" against which the current number of defectives must be judged is the author's simplified construction of the sequential test criterion (the probability ratio test). This is the cumulative "likelihood ratio," computed on the required definitions of "acceptable" and "faulty," as well as on the levels of consumers' and producers' risk. The keynote of the book is that the criterion so constructed is automatically the "optimum."

This idea is further exploited and developed to cover sequential testing outside the realm of industrial quality control. Notably among these are: The sequential normal deviate- and t-tests, the sequential comparison of two percentages as well as of a sample- and population standard deviation. Finally, a sequential theory of estimation is developed.

Although the author states that "some concessions are made to generality and occasionally even to rigour," the style of the book is basically that of a mathematician writing for mathematical statisticians and, as the first comprehensive treatise on this new statistical technique, will be indispensable for them. However, those not well acquainted with mathematical argument will find the book "heavy going."

H. O. HARTLEY.

Selected Techniques of Statistical Analysis for Scientific and Industrial Research and Production and Management Engineering. By the Statistical Research Group, Columbia University. [Pp. xiv + 473, with 45 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 36s. net.)

THIS book gives an account of the war work carried out by the Statistical Research Group of Columbia University, under the direction of the Applied Mathematics Panel administering contracts of the U.S. Office of Scientific Research and Development.

Under the editorship of C. Eisenhart, M. W. Hastay and W. A. Wallis (Director of Research of the Group), numerous distinguished statisticians of the group have contributed, and ideas of others have been utilised. Generous acknowledgment of methods due to British statisticians is made.

By its nature the book is not built on a logical structure and theme, but is rather comparable to an "edited folder" of statistical memoranda. A certain amount of cohesion has been introduced by the editors, and an extensive index will be of some help to the reader who is anxious to have some guidance in his particular problem from the solutions to the selected

problems treated in the book. He will, however, find a search worth while and will discover many good and ingenious ideas in the book.

Without prejudicing the good work in the other chapters we may single out the following: Chapter 3, "Multivariate Quality Control," by H. Hotelling (a method of combining several measurement-characteristics of an article under inspection into an "index" most suited to give warning of lack of control. This is followed by multivariate analysis of airtests of bomb-sights); Chapter 4, "Effects of Rounding or Grouping Data," by C. Eisenhart (the effect of measuring to a limited but fixed accuracy on current statistics and tests; one misses the British results for Range); Chapter 10, "Experiments in which the Independent Variable is known only in Terms of a Probability Distribution," by the Editors, based on work by J. Wolfowitz (gunbarrel test at pressures, not known exactly but known to follow a certain frequency law, required to determine number of tests appropriate to prescribed risk levels); Chapter 12, "Abandoning an Experiment prior to Completion," by the Editors, based on work by L. J. Savage, K. J. Arnold and M. A. Girshick.

Note also the extensive new tables of errors of the second kind in the variance ratio test with random set up (Chapter 8).

H. O. Hartley.

Proceedings of a Symposium on Large-scale Digital Calculating Machinery, 1947. [Pp. xxix + 302, with 99 figures.] (Cambridge, Mass.: Harvard University Press; London: Oxford University Press, 1948. 55*s*. net.)

This book was compiled from the manuscripts of the papers presented by thirty-four speakers at a Symposium on Large-scale Computing Machinery at Harvard in January 1947. The papers cover almost all aspects of the subject and give a very fair picture of the state of development in the U.S.A. at that time, which should be of great interest to workers in the same fields in this country. The subjects treated fall roughly into three main groups dealing with existing machines, machines and components in the development stage, and numerical methods in relation to large-scale computing machinery.

Inevitably the most satisfying are the papers on existing calculating machines. The best of these is that dealing with the Bell Telephone Computing System, which contrives to give, in quite a brief account, all the main features of the logical structure of the machine and the process of adapting a problem for computation on it. Almost equally good is that dealing with the Harvard Mark I Computer. The paper on the Eniac is a disappointment; the attempt to compress too much detail into a short account has produced a catalogue effect.

The second group is concerned mainly with brief descriptions of proposed methods of high-speed storage and suggestions for input and output mechanisms. Papers by K. Sharpless on mercury delay line storage and by J. Rajchman on the Selectron give, within the limits set by the writers, a reasonably complete sketch of these two methods of storage, but that by J. Forrester on high-speed electrostatic storage is of a remarkably tentative character.

The third group, on computational techniques, should interest a wider class of readers than the first two. Of particular interest is the paper by H. Rademacher on the accumulation of errors in computing processes. It

deals with simple quadrature formulæ and an elementary process for integrating ordinary differential equations, and gives estimates for the errors due both to the approximate nature of the finite difference equations and to the accumulation of rounding off errors. This paper represents one of the first attempts to deal with a problem which is becoming of increasing importance with the development of electronic computers. Surprisingly little attention is given to the process of programming problems for electronic computers; it is treated in a brief paper by J. Mauchly which does little more than state the main principles which are involved. The book is well illustrated and the standard of presentation is very high.

J. H. WILKINSON.

Theory of Probability. By HAROLD JEFFREYS, M.A., D.Sc., F.R.S. Second edition. [Pp. viii + 411.] (Oxford: at the Clarendon Press, 1948. 30s. net.)

THIS second edition of Prof. Jeffreys' book makes a welcome appearance; through its issue in 1939, the first edition was missed by many would-be readers. The volume is not a general textbook on the Theory of Probability, but is primarily devoted to the development and defence of the author's own methods of drawing inferences from observational data. It is impossible within the permitted compass of a review either to do justice to the structure that the author has built up or to discuss why he is at variance with the approach followed today by other mathematical statisticians. The record of a few of one reader's many impressions must suffice.

As is well known, Jeffreys starts with the idea that the degree of confidence in a proposition, a quantity varying with the nature and extent of the evidence, is a primitive notion with which it is possible to associate a numerical scale. Starting from this notion, he constructs a formal system of inductive inference, thence developing a mathematical theory of statistics which is capable of practical application and runs parallel to the generally accepted theory of his contemporaries. This construction is possible provided that, when we have no initial knowledge whatsoever bearing on the probability of a proposition (or the relative probability of several propositions), we can start the ball rolling by adopting certain formal rules for assessing this unknown. Using this formal "prior probability," as data bearing on the proposition accumulate, it is possible to express numerically the manner in which our degree of belief is modified by additional observations. Thus, with the help of considerable mathematics, there can be seen emerging the attractive concept of a numerical measure which changes progressively as we learn from experience.

But the method of formal construction is such that the resulting measures of probability are not necessarily directly related to the frequency of occurrence of events, whether observed or expected. From Jeffreys' point of view this is no disadvantage; nevertheless, there is here a question to be faced. Is the way in which he has linked a numerical measure with the primitive notion of degree of belief necessarily the most helpful to the human mind? For example, the purpose of his K ratios is, as he says, to grade the decisiveness of the evidence. But when he describes a value, $K = 10^{-1}$, as representing odds of 10 to 1 against a hypothesis, to interpret these figures may not the common man need to call up the idea of one event happening 10 times more frequently than another? Thus, after all, may not relative frequency of

occurrence be the primitive notion with which the human mind seeks to associate a numerical measure of probability?

Prof. Jeffreys, as most writers who engage in controversy, has suffered from criticism, some of which may have been unfair. Still, it is a pity that in supporting his thesis he sometimes falls into the bad habit of sketching his opponents' case by means of a caricature which they must find hard to recognize. As a result, the book contributes less than it might to the clearer understanding of the points at issue. But he has given us more than the presentation of one side of a controversial subject; in developing his theory he has thought deeply on many problems of scientific inference. Much that he writes, particularly on the interpretation of physical experiments and the methods of critical discrimination between physical hypotheses, is of very considerable interest to the scientist, whether a statistician or not. The value of these comments lies in the fact that the broad principles of scientific reasoning have a validity more fundamental than the particular probability tools which we choose to adopt as an aid to judgment.

E. S. P.

PHYSICS

General Physics. By A. E. E. McKenzie, M.A. [Pp. viii + 499, with 369 figures.] (Cambridge: at the University Press, 1948. 8*s*. 6*d*. net.)

As stated by the author in his preface, this volume is an abridged, omnibus version of his School Certificate series of physics textbooks, and is intended to provide instruction in physics for pupils taking General Science or Physics-with-Chemistry courses below the sixth form. That Mr. McKenzie has but partially achieved this object is only too evident from a study of the 499 pages of the book, for the ruthless excision required in collapsing the 1120 pages of his School Certificate series has destroyed much of their original general excellence, and what is left is a somewhat incomplete and unbalanced survey of the subject.

The presentation of the subject matter for pupils taking a general course up to School Certificate requires a careful blending of the descriptive, experimental, and quantitative aspects of the subject, and in Mr. McKenzie's book it is mainly the first of those ingredients that is present. The experimental work is sketchy, and, in the reviewer's opinion, quite inadequate for the type of course in mind, whilst it is unlikely that a candidate could reasonably be expected to attempt from the text the solution of many of the otherwise excellent examples collected at the end of each chapter.

The early sections of the book are well presented, particularly that dealing with the pressure of fluids, to which 63 pages are devoted. Too much space, however, has been given to this topic, and subsequent sections, such as dynamics and electrostatics, have suffered in consequence, whilst many topics, notably the reflection of light at curved surfaces, and the gas laws (other than Boyle's law), find no place at all in the text. In the rather brief section dealing with calorimetry, the student is first confronted with Darling's fuel calorimeter with which the concept of water equivalent is used without previous definition or explanation. Graphical work finds little place in the book either as a means of treating experimental results or to show the relation between two quantities. In the electricity section the symbol i is used throughout for current in amperes.

Among the good features in the book is the treatment of heat and energy given in Chapter 16, which also contains a stimulating descriptive account of heat engines. There are excellent references to the spirit and aims of science, and throughout the historical notes are expertly selected and presented. The text is liberally supplied with many well-chosen photographic illustrations and excellently drawn line diagrams (although in Fig. 219 the diameter of the eyepiece of the astronomical telescope is shown larger than that of the objective). The book is of handsome appearance, being well printed on good paper and attractively bound.

F. T.

Higher Physics. By E. Nightingale, M.Sc., A.R.C.O. [Pp. xvi + 808, with 746 figures.] (London: G. Bell & Sons, Ltd., 1948. 27*s*. 6*d*. net. Also in separate parts: Pt. I, *Mechanics and Properties of Matter*, 7*s*. 6*d*. net; Pt. II, *Heat*, 7*s*. 6*d*. net; Pt. III, *Light and Sound*, 10*s*. net; Pt. IV, *Electricity*, 12*s*. 6*d*. net.)

This is a notable contribution to Intermediate textbooks of the compendium type, and is primarily intended for sixth-form students in schools, although its usefulness could well be extended to University students in the earlier stages of their courses. It is a vital book, written in the clear style one has learned to associate with the author, and contains an amazing amount of material, most efficiently condensed, which should prove invaluable to physics masters in charge of sixth-form courses.

It is doubtful, however, whether the student will derive equal benefit from this volume. It is the reviewer's experience that students are much happier when working with separate books dealing with the various divisions of the subject, where the argument can be more fully developed, and which contain more explanatory discussion than is possible in the omnibus type of textbook. Inevitably the subject matter has to be telescoped and greatly condensed in order to deal with the very wide range of the whole subject. In consequence it is often found that the various subdivisions of the subject do not receive equal justice, and frequently certain material either gets scant attention or is completely omitted.

Thus, in the present volume, the section on Sound is most inadequate for the purposes of the sixth-form student, and the Statics section deals with isolated topics rather than presenting a graded course. The section on Geometrical Optics is patchy, and contains no work on curved mirrors. Among other topics receiving little or no attention are: optical pyrometers, mechanical methods for J, secondary cells, hygrometry, and refraction through prisms.

The economies of space thus effected are put to good use, however, in dealing with such additional topics as aerodynamics, Heyl's work on gravitation, Reynolds' work on the flow of liquids, etc. There is, too, an interesting chapter on Nuclear Physics. Units are systematically defined, and the graphical treatment of experimental results receives due emphasis. The grace and power of the method of dimensions is revealed by its extensive application to numerous topics throughout the book. The text contains many worked examples and there is a useful collection of examination questions at the end of each chapter.

The book is well illustrated, and superbly turned out on fine-quality paper, but on account of the high published price it is doubtful if the book

could be economically used as an additional textbook for sixth-form students, although certainly such students should have access to a copy on the library shelves.

F. T.

Heat Conduction. By L. R. INGERSOLL, O. J. ZOBEL and A. C. INGERSOLL. International Series in Pure and Applied Physics. [Pp. xii + 278, with 43 figures.] (New York: McGraw-Hill Book Co., Inc., 1948. 24*s*. net.)

THIS book is the successor of a well-known textbook, which has been revised with the help of a third author, and it also embodies much new matter. It is characterised throughout by its lucid treatment and the many practical examples with which it is illustrated. Only a moderate knowledge of mathematics is assumed and the more abstruse sections are approached in easy stages. Consequently it should prove of great value to physicists, engineers or others interested in the subject. Further, with its extensive bibliography, it should provide a good starting-point for more advanced study.

Of the new material, the most important is perhaps the chapter on approximate methods (No. 11). This deals with the processes, graphical, arithmetical or electrical (based on the analogy of the flow of heat and electricity), now available for treating problems both of the steady and unsteady states of heat flow. Certainly the results which can be obtained by simple means in practical problems, many of which would defy strict mathematical analysis, are remarkable. Particularly noteworthy in this connection are Schmidt's method and Emmons's application of Southwell's relaxation principle. Another valuable section is that dealing with the theory of permanent sources and the applications thereof.

A single criticism may be made. As in the case of many textbooks, not intended to be exhaustive, the authors have no doubt found it difficult to fix the precise limits of subject matter to be covered, but there is one direction in which, it is suggested, a slight extension might have been of advantage. The subject of conjugate functions is touched on, but nothing is said of conformal transformation. The application of this two-dimensional method for steady flow to shapes not presenting more than two right-angle bends would involve only the use of circular and hyperbolic functions, and might perhaps be considered as coming within the scope of the book. Thus the problem discussed in Chapter 4.1 could readily be treated by this method, which has already been applied to the cases illustrated in Figs. 11.1 and 11.3, and to a number of other similar cases.

F. H. S.

Wave Mechanics and its Application. By N. F. MOTT and I. N. SNEDDON. [Pp. xii + 394, with 68 figures.] (Oxford: at the Clarendon Press, 1948. 30*s*. net.)

A NEW book on any branch of Quantum Theory is always eagerly awaited. Here comes what must surely serve for some years as the standard English textbook on Wave Mechanics. Prof. Mott is already well known for the readability and liveliness of his earlier books: this new one, in collaboration with Dr. Sneddon, of Glasgow, is no exception.

What stands out most clearly is the wide scope of topics covered—or, more properly, introduced to the reader. The book itself is written for

students just beginning a study of Wave Mechanics, so that the first steps in converting the ideas of De Broglie waves into a mathematical system are described. But during the last 10–15 years a large variety of new material has become available, typical of the growth and development of this subject. (Wave mechanics is only 22 years old, even now, though it seems older by virtue of its many astonishing successes.) Mott and Sneddon find room for brief introductions to everything of significance discovered in these latter two decades. Here, for the first time, a student can find between the covers of one volume of 400 pages, not only all the standard work on harmonic oscillators, hydrogen atoms and perturbation theory, but also something about molecular structure, momentum distribution, radiation damping, structure of the deuteron, van der Waals forces, motion of electrons in crystals, and an excellent account of the Dirac theory of the electron. Emphasis is on Wave Mechanics as a tool useful in almost every problem of modern physics. The more esoteric pleasures of matrix mechanics find a somewhat thin recognition in one chapter at the end. The purist may cavil and say that symbolic methods have an elegance and æsthetic appeal quite beyond that of the Schrödinger equation. Let them do so—the student, whether physicist, chemist or mathematician, who is beginning research and wants to know what Quantum Theory can say by way of application to a hundred and one problems of atomic and molecular character, will find here the sort of stuff that he wants, and he will be glad.

The book is well got up, as befits the Clarendon Press. It is not without minor blemishes and misprints, which a second edition will no doubt remove. These do not, however, in any way detract from the judgment that this is a very welcome and timely addition to the literature of its subject.

C. A. COULSON.

Some Aspects of the Luminescence of Solids. By F. A. KRÖGER. [Pp. xii + 309, with 74 figures.] (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. 30s. net.)

ALMOST exactly ten years ago a report appeared in this journal of a conference on luminescence held by the Faraday Society at Oxford. Looking back at that report, and then again at the present position, it appears at first that very little advance has been made in the satisfactory explanation of luminescent processes in solids. This is not from any lack of theorising, for the zone theory of solids offers tempting opportunities for plausible surmise which have not been neglected. Nor, as this book shows, is it due to lack of new experimental data. It is rather, one feels, that a new welding together of theory and experiment is required in the light of the vast amount of work done since 1938 and the many and various interpretations that have been made. For the most part the work here described consolidates the rather general ideas suggested at the 1938 Conference. The long chapter on the manganese activated solids, for example, confirms the suggestion of Randall and others that the emission is here associated with manganese ions or complexes (though the work on the differences between the effects of divalent and tetravalent manganese is new).

The work on tungstates and molybdates affords valuable evidence for the responsibility of tungstate and molybdate ions and important evidence for the transfer of excitation energy through considerable regions of the host lattice and for the action of killers.

The short chapter on uranium as an activator suggests differences between uranate and uranyl absorption and emission: that on titanium activated solids is less definite in its indications.

The chapter that will probably excite most interest from theoreticians is that on the temperature dependence of luminous efficiency. Here, after describing extensive investigations, Dr. Kröger discusses the mechanism of quenching, and, whether one agrees with his conclusions or not (his dismissal of the Möglich Rompe theory of radiationless transitions is not quite convincing), it will not be denied that he has done valuable service in fixing attention on a fundamental problem. For it becomes clear that, oddly enough, it is not so much the luminescence of crystals that has to be explained, but why all crystals do not show this effect under suitable stimulation, if indeed they do not. The interaction of absorbed energy and lattice energy appears a fundamental issue not only for luminescence but for other properties of the solid state. The attention given to the Schon-Klasens-Wise ideas concerning the part played by positive holes is deserved and valuable.

The extraordinarily extensive lists of luminescent solids and of references at the end of the book will earn the gratitude of all interested in this subject, though one could have wished that Dr. Kröger had been less restrained in indicating the doubtfulness of some of the specifications and that the descriptions of the experimental methods, relegated to a short appendix, had been more adequate.

J. E.

Hyperfine Structure in Line Spectra and Nuclear Spin. By S. TOLANSKY, D.Sc., Ph.D., D.I.C. Second Edition. [Pp. viii + 120, with 26 figures.] (London: Methuen & Co., Ltd., 1948. 6*s*. net.)

THIS useful little book, first published in 1935, has now been brought up to date, or as nearly so as may be, having regard to the long delay in publication imposed by present difficulties. Prof. Tolansky has made a very thorough revision of the text, which has involved the virtual rewriting of about half the book. The most valuable additions are probably those concerned with isotope effects and nuclear spins, in both of which connections important new results are now available. The revised Table of Nuclear Spins and Moments includes determinations up to 1946, but was evidently compiled before the interesting values for Lu and Pr were obtained. Some room for new matter is found by abbreviating the discussion of the origin of nuclear spins and magnetic moments and relegating it to an appendix.

A new chapter deals with nuclear electrical quadrupole moments. Its insertion is fully justified by the growing importance of the subject, but one is left with the impression that brevity has only been achieved at some sacrifice of clarity.

Apart from a few minor errors, there is one slip which should be corrected in a future edition. In Chapter VIII, it is stated that the isotope displacements of band heads (*e.g.* $B^{10}O$ and $B^{11}O$) are due to differences in the moments of inertia of the two molecules, whereas of course they are in fact simple mass effects on the vibration frequencies.

Every student of spectroscopy and nuclear physics, as well as many others, should find this book of interest and value, and none should find its price prohibitive.

W. E. C.

Multiple-Beam Interferometry of Surfaces and Films. By S. TOLANSKY. Monographs on the Physics and Chemistry of Materials. [Pp. viii + 187, with 113 figures.] (Oxford : at the Clarendon Press, 1948. 18*s*. net.)

THIS is the first volume in a new series "intended to summarise the recent results of academic or long-range research in materials and allied subjects, in a form that should be useful to physicists in Universities and in Government and industrial laboratories."

Since 1942 Tolansky has been refining the classical techniques of interference, and the present volume deals with the application of the old methods, and of his modifications of them, to the study of the topography of surfaces of solids. A chapter is included on the production of plastic replicas, again with the purpose of studying surface topography but at second hand. There is also a chapter devoted to the subject of interference-filters.

If a parallel beam of monochromatic light falls on a wedge bounded by transparent surfaces Fizeau fringes are formed, which are loci of equal thickness. The source must be restricted in size, unless the film is very thin. If the transparent surfaces are partly silvered, multiple beam fringes are formed, which are very sharp, and the high resolution afforded altogether transforms the examination of crystal surfaces. In practice, one transparent surface is provided by the face of a good optical flat, while the other is the crystal surface under examination. Results are reported of observations made by this method on quartz, mica, selenite and calcite : cleavage surfaces of these substances yield interesting information. For example, in mica, steps are observed, with large optically uniform areas between them. The sharpness of the fringes allows measurement of the steps to an error of 3 A°., and they are found to be small multiples of 20 A°., *i.e.* simple multiples of whole "molecules."

To study a surface completely by the above means would require a number of photographs, providing complex data to be related. To obviate this, Tolansky has introduced a special technique which he calls crossed Fizeau fringes. The surface to be studied is placed opposite the flat, forming a wedge, and the above described multiple beam Fizeau fringes are photographed. The wedge-angle is now reduced until the surfaces are sensibly parallel, bearing in mind the irregularities of the crystal surface, the colour-filters are removed and a photograph of the fringes is again taken on the same plate as before, so that an overprinting results. The first exposure gives a series of sharp fringes, and the second one yields a complete picture of the surface. An extension of the method is to overprint more than one set of narrow-fringes—*e.g.* a second set approximately at right-angles to the first, so that the picture of the surface is covered by a fringe grid. In this way, for example, it was shown that the triangles found on diamond surfaces are pits formed by growth of the surrounding material, the sides of the pit being formed by the arrest of growth.

In favourable circumstances the optical flat can be dispensed with ; *e.g.* mica sheets can be studied by partly silvering both faces.

A further development is the use of what the author calls "fringes of equal chromatic order"—white-light fringes, such that each point on a given fringe corresponds to a path-length within the film containing the same number of waves. The fringes are made visible by examining the transmitted light with a spectroscope. The advantage of this method over the

Fizeau method is that hills in a surface are readily distinguished from valleys, and another is that the precision of measurement is not affected by local variation in dispersion.

The use of interference by reflection is described for dealing with metals and other opaque solids.

An omission in the chapter on interference filters is any reference to the work of Geffcken, of Schott's. The book is a stimulating one, and gives the impression that the author has tried to make clear all the possibilities.

A. C. M.

Progress in the Theory of the Physical Properties of Glass. By Dr. J. M. Stevels. [Pp. xii + 104, with 26 figures.] (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. 10*s*. net.)

This is No. 20 of the monographs on the Progress of Research in Holland during the war; it maintains the very high standard of the series. The first chapter is a satisfactory, concise statement of current views on the arrangement and function of the various constituent atoms in silicate glasses. This introductory chapter will be interesting to the general reader; thereafter the book will appeal only to those who are actively concerned with the same or closely related fields. Such workers will read this monograph with great interest, for it is obviously an important addition to the literature on the theory of the physical properties of glasses.

The subjects discussed in the main body of the book are: Chapter II, The Density of Glasses; Chapter III, The Electrical Conductivity of Glasses; Chapter IV, Dielectric Losses; Chapter V, Molecular Refraction.

Empirical formulæ for calculating the physical properties of glasses from their chemical composition have been in use for some time; they date from the time of the pioneer work of Winkleman and Schott. Stevels deals with the subject from this angle, but his approach is novel in that it is founded on the information on the arrangements and functions of the various atoms in glasses obtained from X-ray diffraction studies over the last fifteen years or so. He is successful not only in deriving new empirical formulæ, but also in introducing new and stimulating ideas in the correlation of physical properties with the structure of glasses.

The text is well provided with references to original papers.

R. W. Douglas.

About Cosmic Rays. By John G. Wilson, M.A., Ph.D., F.Inst.P. Sigma Introduction to Science 14. [Pp. 144, with 8 plates and 26 figures.] (London: Sigma Books, Ltd., 1948. 8*s*. 6*d*. net.)

The Sigma books are intended for the intelligent lay reader, but, as explained on the dust cover, Dr. Wilson's book is of a more advanced character and therefore it may be rather more difficult for the non-scientific person. However, this is compensated, in part at least, by the extremely lucid treatment and admirable arrangement of his material. One of the assets of the book is the helpful illustrative diagrams, where care has been taken to ensure that they do make for an easier understanding of the text. Although the book is of the popular style, the author does not shirk treating the theoretical problems associated with cosmic rays.

This is a book which physicists, as well as others, will find very informative

and one that can unreservedly be recommended as a very readable account of what has now become a complex subject. One small discordant note may be introduced; the reviewer would like to utter a protest against the phrase "electron-proton build-up," lest ere long we find ourselves reading of "a nucleon set-up" or "a line-up of lenses."

W. E. DUNCANSON.

Radio Receivers and Transmitters. By S. W. AMOS, B.Sc., A.M.Brit.I.R.E., Grad.I.E.E., and F. W. KELLAWAY, B.Sc. Second edition. [Pp. xii + 356, with 8 plates and 210 figures.] (London: Chapman & Hall, Ltd., 1948. 25s. net.)

THE authors' claim that this book is unique in providing a bridge between "pure science and applied radio" is to some extent justified, and probably accounts for its reaching this second edition. Many amendments and additions have been made to the first edition, but the authors' belief that nearly all the errors have here been corrected is much too optimistic.

Chapter and section headings include all the main principles and equipment associated with orthodox radio receivers, transmitters and propagation, but the arrangement of subject material fails to show the "coherent and orderly fashion" that is claimed for it. Typical of the disorder is a section on pressure and velocity microphones intruding uncomfortably between one on modulated R.F. amplifiers and one on transmitter feeder lines. The Appendices consist of six arbitrarily chosen mathematical embellishments and one note on Dimensions that is in flat contradiction to a statement about capacitance on p. 42.

Within each section the choice of subject matter is very unbalanced, showing a deplorable tendency to gloss over difficulties. Much of the treatment of aerials and radiation, for example, is a glib quotation of unexplained formulæ, with the customary confused mixture of units as an added tribulation to the serious student. Space devoted to matters like the spark transmitter and the "singing arc" could have been used much more profitably. Three times as many pages are devoted to the rarely used leaky grid detector as to the ubiquitous diode detector, and no hint is given about reservoir condenser action in the latter.

Tacit simplifications and obvious errors are to be found in many chapters, a 90° phase error on p. 75 and a decibel expression involving complex quantities (rationalised simply by squaring!) on p. 64 being typical examples. These make the book unsuitable for the advanced student; it avowedly assumes prior knowledge of mathematics, electricity, and radio, and is therefore not suitable for the beginner. It can be of value only to the more mature student seeking either a different viewpoint for some of his already established knowledge or an exercise in critical reading.

H. MARRIOTT.

Ultrahigh Frequency Transmission and Radiation. By NATHAN MARCHAND. [Pp. x + 322, with 140 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 27s. net.)

THE greater part of this book is devoted to an elementary exposition of Maxwell's equations (using vector analysis and rationalised MKS–C units), together with a detailed derivation of certain technically important solutions, applicable mainly to aerials and wave guides. Conventional transmission

line theory is also included, and a final chapter deals with breaks in transmission line shields, balance-to-unbalance transformers, etc.

The theoretical analysis is straightforward, though sometimes cumbersome, and not always convincing in its treatment of signs. The physical meaning of each of the fundamental equations of electromagnetic theory is clearly explained, however.

Several vague and misleading arguments are encountered, for example the erroneous explanation of proximity effect in a two-wire line as due to attraction between the two currents. Moreover, the author adopts a somewhat uncritical style throughout, for instance, the statement that, for various possible definitions of the characteristic impedance of a wave guide, ". . . the results obtained by all of them are *quite good* " (reviewer's italics). Similarly naïve is the remark that, by adding to the resistance of the wire a term corresponding to radiation, transmission line theory may be applied to aerials and "correct results obtained." Notation, though generally satisfactory, is not always good, as when ρ is used both for charge density and radial distance in the same calculation and when TE_{mn} modes are found in rectangular guides, but TE_{nm} modes appear in circular guides, an unnecessary and confusing change. Equation 7.55 on p. 220 should have a factor $\sin\left(\frac{m\pi}{z_0}.z\right)$.

The book is well illustrated and produced, and contains a very conveniently arranged set of mathematical tables of elementary functions.

A. L. CULLEN.

Microwave Transmission Circuits. Edited by GEORGE L. RAGAN. [Pp. xvi + 725, with 624 figures.] (New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. 51*s*. net.)

THIS book is the ninth volume of the M.I.T. Radiation Laboratory series, and deals primarily with the design of components for microwave transmission circuits. It may be regarded as complementary to the eighth volume, *Principles of Microwave Circuits*, which is mainly concerned with the theory.

Transmission of microwave power is commonly effected by means of waveguides, but coaxial lines are useful at the longer wavelength end of the microwave band. Accordingly both waveguide and coaxial line components are discussed, both versions of a given component being described in adjacent sections. The components themselves are then grouped according to function without undue emphasis on the particular form of transmission line employed.

The first two chapters deal with principles, and cover such topics as guided wave theory, circle diagrams, impedance matching, etc. The third chapter gives a valuable discussion of materials and construction techniques. Next follows a chapter on the practical forms of waveguide and coaxial line, couplings and connectors, corners, bends, matching devices, etc., concluding with a discussion of voltage breakdown under high-power pulse conditions. Flexible waveguides and coaxial cables are dealt with next, but the data is largely relevant to American manufacturers' products, and so will be less useful in this country. Chapter VI describes reflectionless transitions from one form of transmission line to another; Chapter VII is concerned with rotary joints; and Chapter VIII with tuners, power dividers, and switches.

The last two chapters, which represent about one-fifth of the whole book, make a remarkable attack on the difficult problem of microwave filter design.

This work takes conventional filter theory as its starting point, and the relevant material is fairly completely covered in the first of these two chapters. The final chapter applies the theory to the design of microwave filters, and gives a number of practical examples of the construction of such filters, together with typical performance curves, etc.

The book is well worthy of its place in this excellent series, and can be strongly recommended to all who have to deal with microwave apparatus.

A. L. CULLEN.

Protective Methods for A.C. Power Circuits and Machines. By H. M. MARKLEW, M.Sc.(Eng.), A.M.I.E.E., and J. A. COLLIER, B.Sc.(Eng.), A.M.I.E.E. [Pp. x + 142, with 98 figures.] (London and Glasgow: Blackie & Son, Ltd., 1948. 10s. net.)

THIS book has been written to fill a gap which exists between the treatment of protective gear in general and advanced texts. The plan of presentation is sound, starting with a review of fundamentals, relay types and current and voltage transformers, then proceeding to the application of the fundamental principles to various circuits and concluding with chapters on fuses and fire protection.

The authors have covered an extremely wide range of specialised systems and due to space limitations, compelling brevity, have occasionally been ambiguous, but in general descriptions are given with lucidity and simple directness. Mathematics have been kept to a minimum. The major criticisms are: more attention could have been given to fundamentals and less to detailed descriptions of particular systems; a more extensive treatment of current transformers is very desirable, as in the majority of applications these govern the performance more than trick circuits and relay characteristics; overload and earth fault time discrimination systems should have been treated much more extensively, as they are used on probably 90 per cent. of all circuits. To some extent these criticisms are met by the inclusion of a good bibliography. It is a pity that greater care was not exercised in the proof stage, as minor editorial errors abound; once these are corrected, it is felt that this book will fill a long-felt want, and it should act as an excellent introduction to more advanced texts. It can be recommended as being suited particularly to the needs of students and also system engineers requiring some familiarity with the performance of protective gear.

J. R. MORTLOCK.

Fundamentals of Soil Mechanics. By DONALD W. TAYLOR. [Pp. xii + 700, with 239 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 36s. net.)

Soil Mechanics in Engineering Practice. By KARL TERZAGHI and RALPH B. PECK. [Pp. xviii + 566, with 218 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 33s. net.)

THE literature of soil mechanics has been considerably enriched by the publication of these American books. In a sense, they are complementary; the book by Taylor is concerned purely with the principles of the subject, while that by Terzaghi and Peck is devoted primarily to the application of present knowledge of the properties of soil to the design and construction of foundations and earthworks. From both the text and the lists of references

it is clear that scant attention has been paid in both books to developments on this side of the Atlantic, and, while this in no way invalidates most of the information given, the reviewer has noted cases where data quoted would be inapplicable in this country. As an instance of this, research in this country has shown that the information given on page 375 of the book of Terzaghi and Peck on the compaction of soil with sheepsfoot rollers would be quite unsuitable for British soils and climatic conditions.

Although one book is concerned with theoretical soil mechanics and the other with the practical aspects of the subject, their scope is essentially the same, the principal matters dealt with being the design of footing, raft, pile and pier foundations for buildings, bridges and other structures, the design of retaining walls for earthworks, the design and construction of earth dams, embankments and cuttings, the control of ground water and site exploration. Road and airfield engineers will regret the paucity of information in both books on soil compaction, soil stabilisation, pavement design and the control of moisture conditions in road and airfield subgrades.

It is possible to detect a difference in the attitude of the authors as to the best procedure of applying principles of soil mechanics in practice. Terzaghi and Peck maintain (and few engineers would disagree with them) that undue attention has been given in the past to the very few problems that can be solved with accuracy (the attention given to the theory of circular slips in this country may be instanced). This leads them to the conclusion that in most cases only an approximate forecast is possible and that the careful laboratory testing of soil specimens is often unnecessary. Taylor, while noting the limitations imposed by the variability of soil strata, contends that the results of a soil investigation will enable a correct forecast to be made of the effect of some of the factors bearing on a problem, and that this will be of much value in the hands of an experienced engineer, even though other factors cannot be adequately taken into account.

The book by Terzaghi and Peck is divided into three parts. Part C, the main section, describes methods of investigating soil engineering problems; Parts A and B, which are ancillary to Part C, deal respectively with the physical properties of soil and theoretical soil mechanics.

The lay-out of Taylor's book calls for no comment; the reviewer was particularly impressed by the excellence of the chapters on flow nets, shear testing and shearing resistance of clays.

Both books should find a considerable body of readers in this country. Taylor's book must surely become the standard textbook for university students of soil mechanics. It will also be invaluable to research workers and engineers with a special interest in the subject. The book by Terzaghi and Peck will be welcomed by consultants and testing engineers and will undoubtedly prove of great value and interest to all engaged in civil engineering construction.

W. H. G.

CHEMISTRY

British Chemical Nomenclature. By A. D. Mitchell, D.Sc., F.R.I.C. [Pp. viii + 156.] (London: Edward Arnold & Co., 1948. 21*s*. net.)

All chemists realise the necessity of a standard system of documentation or nomenclature for inorganic and organic chemistry, so that chemical compounds shall be adequately described by names indicating unequivocally the

constitution of each compound, and indexed in a manner which shall make reference to previous investigations easy and accurate.

The rules for nomenclature of inorganic and organic chemical compounds are formulated by the two respective committees of the International Union of Chemistry. International organisation of organic chemical nomenclature began at the historic Geneva Conference (1892); but the inadequacy of the "Geneva Rules" was realised long before the modern organisation of the subject was set up. The present rules which now form the basis of chemical nomenclature and of indexing, followed by the Chemical Society in this country and by the American Chemical Society, essentially result from international conferences held from 1930 to the present time. Critical publications of the existing difficulties and anomalies in chemical nomenclature and in indexing have been markedly more numerous in America than in this country. The late Dr. Clarence Smith, Editor of the Chemical Society's journal, was for many years a recognised authority on chemical nomenclature, and in 1936 published a lecture in which the conventions and rules of nomenclature used in the journal of the Chemical Society were succinctly described. This lecture has remained the only authoritative publication on the subject in this country.

It has fallen to Dr. Mitchell to write a more complete treatise, and he has done it admirably, and produced an authoritative work on chemical nomenclature. With his long and unusually wide experience, no chemist in this country is so well qualified to advise on both inorganic and organic chemical nomenclature and to make critical comparisons, with informative examples, of American and English methods of treatment of the subject.

Owing to the more numerous types of organic than of inorganic compounds, the greater part of the book had to be devoted to organic chemical nomenclature.

The rules recently adopted after consultation between British and American chemists and biochemists for the nomenclature of amino acids are dealt with in detail. These, together with the specialised nomenclature now applied to the carbohydrates and their derivatives, constitute the most important recent advances in organic nomenclature, and these improvements are explained and discussed with adequate illustrations in a clear and informative manner.

Dr. Mitchell's treatment of Cyclic Systems is perhaps the outstanding part of this important contribution to the literature of organic chemical nomenclature. It is not a catalogue or "Index," but it is comprehensive and, as far as your reviewer is aware, there is no important type of cyclic compound of which the most acceptable method of numbering is not adequately treated. In this section, as elsewhere, Dr. Mitchell does not hesitate to indicate inconsistencies in some methods of numbering cyclic systems which persist ten years after the publication of Prof. Patterson's comprehensive "Ring Index."

The continued, and far too common, use of trivial names for compounds of known constitution is to be deplored, and all chemists must agree that these trivial names should be discarded and replaced by the systematic names. Many such compounds whose trivial names are still in common use are given with their systematic description in nine useful lists, that of the amino acids being set out in a particularly ingenious way. This also applies to Dr. Mitchell's method of setting out concisely the constitution of the simple sugars. These lists, together with that of radicles now suggested for

acceptance in organic chemistry, add considerably to the reference value of this scientifically valuable but by no means expensive book.

It is a pleasure to record that no error has been found in the text or in the complete index.

C. S. Gibson.

Rhenium. By J. G. F. Druce, M.A., M.Sc., R.Nat.Dr., F.R.I.C., F.C.S. [Pp. viii + 91.] (Cambridge: at the University Press, 1948. 10*s.* 6*d.* net.)

Great interest was aroused amongst chemists by the announcement in 1925 that Mendeléef's dvi-manganese had been found. The German discoverers gave the name rhenium to the new element, after Germany's "Westmark," the Rhineland. Within a comparatively short time, investigators both in this country and abroad, made important contributions to the chemistry of the new substance. Up to date nearly four hundred scientific papers dealing with rhenium have appeared and the author of this volume has brought together the results in an interesting and authoritative survey. No one is better qualified than Dr. Druce to do this, for he was early engaged in this field of work and he has made significant contributions. The element, although scarce, has become an article of commerce and some of its salts are also obtainable commercially. Therefore the investigator concerned with new uses for the rarer elements may well keep rhenium in mind. He will find in this volume a very interesting and suggestive chapter dealing with the applications and patents relating to rhenium. Further, the research chemist looking for new problems in physical or inorganic chemistry can turn with certainty of finding them to this little book. Although much has been achieved, many aspects of the chemistry of rhenium will repay further study. In most textbooks dealing with modern inorganic chemistry reference is generally made to the chemistry of this element. This means that the student of chemistry may hesitate to buy this book. He may not feel justified in allocating a half-guinea of his limited resources for this special survey of the chemistry of one element. However, the purchaser of this volume will possess a well-written account of a fascinating element and its compounds. The new book has an excellent bibliography and a useful index, and is admirably produced.

W. Wardlaw.

Introduction to Chemical Thermodynamics. By Luke E. Steiner. Second edition. [Pp. xiv + 510, with 42 figures.] (New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. 36*s.* net.)

The first edition of this book appeared seven years ago, at a time when most chemists in this country had little leisure to study new publications. On this account it probably received less notice than it deserves. According to the preface, the objectives of the book are to acquaint the student with the fundamental theory of thermodynamics and of the relations between thermodynamic functions, to prepare him to utilise thermodynamic data, and to give him a sound background for more extended work in thermodynamics. In each of these respects it should be very successful. The scope of the work is approximately the field of general thermodynamics normally included in courses for an honours degree in chemistry in this country, and it appears to cover very effectively the requirements of a student reading to this standard.

The treatment throughout is rigorous, and yet the approach and the style are so easy as to remove any suspicion of dullness. The applications of the laws and of the derived relationships are skilfully interwoven with the more theoretical aspects in such a manner as to preserve the realism of the subject and prevent it from becoming a mere matter of mathematical legerdemain. The illustrative examples used are drawn from recent publications, which are cited, thereby adding to the essentially practical approach of the book and encouraging reference to original publications. A series of problems at the end of each chapter are well selected as suitable for solution by the student, and lead him to the independent use of the thermodynamical concepts without which no true appreciation of the subject can be obtained.

The symbols and nomenclature used are based on those of Lewis and Randall, and are simple, although the former differ slightly from those now conventional in this country. The general layout of the book is so good, however, that one feels that it is worth overcoming any slight difficulties which may be introduced through this cause.

J. W. S.

Dipole Moments. By R. J. W. LE FÈVRE, D.Sc., Ph.D., F.R.I.C., F.A.C.I. Second edition. Methuen's Monographs on Physical Subjects. [Pp. vi + 117, with 23 figures.] (London: Methuen & Co., Ltd., 1948. 5*s*. net.)

THE first edition of this book was deservedly popular, for it gave a simple and clear account of the theoretical basis of the subject, a useful description of the methods of measurement, and a compressed but reasonably complete survey of the applications of the results to chemical problems. Since it was published in 1938 a fair amount of new work has appeared, so a new edition is timely. The general plan of the work is unchanged, the alterations have been in detail, and the length has been but slightly increased.

There are some points of criticism. Apart from details, the chief of these are that little is said about the recent use of micro-wave methods for determining moments, that Halverstadt and Kumler's modification of Hedestrand's method of calculation is not described, that the discussion of methods of deriving bond angles is not taken much beyond the stage reached in 1934, that moments are not related to electronegativity differences, nor is there any mention of Coulson's treatment of the C—H bond moment; and the reviewer feels that the last chapter should be replaced by one on atom polarisation. It must be remembered, however, that the author has a very difficult task of selection to keep within the limits of size applied in this series of monographs; and he would probably retort that not all of these topics can be included.

Within these limits the book is excellent and can be warmly recommended to all who want to make a preliminary acquaintance with the subject.

L. E. S.

Distillation and Rectification. By DR. EMIL KIRSCHBAUM. English translation by M. WULFINGHOFF. [Pp. xiv + 426, with 236 figures and 5 loose-leaf graphs.] (Brooklyn: Chemical Publishing Co., Inc., 1948. $10.00.)

KIRSCHBAUM'S book on distillation was written originally in 1939, published in Germany in 1940 and in the U.S.A. in 1943. It has already become a

classic among chemical engineers and university students. Those who have difficulty in understanding the German edition or who have not already made their own translation will, therefore, welcome this first edition in the English language.

The book is written with much greater clarity than Robinson and Gilliland and more extensively than Reilly. Rather does it resemble an improved up-to-date edition of Young. Mathematical complexities have been reduced by the graphical presentation, without over-simplification. Full use is made of enthalpy concentration diagrams and useful vapour-liquid equilibrium data are given. One might regret that the names of Sorel, Underwood and others are not in the author-index; and that the technique of laboratory distillation, which has advanced so rapidly since the book was first written, has been so lightly mentioned by the translator in his résumé of recent developments.

Mr. Wulfinghoff is to be congratulated on the excellence of his translation, with which few will find fault. It is a pity that, when such pains have been taken in presenting the text, the figures leave much to be desired. The method of reproducing them in German with an English translation below each is irritating to all but students of technical German. The Bibliography is now hard to find and might well have been left in the original footnote form. The translator's concise but comprehensive résumé of recent developments brings the book right up to date, and one can confidently recommend it as a standard reference work for all engaged in the wide field of purification and separation processes, to which the principles of distillation and rectification have contributed so much.

H. C. Highet.

The Chemistry of High Polymers. By C. E. H. Bawn, Ph.D. [Pp. x + 249, with 1 plate and 72 figures.] (London: Butterworth's Scientific Publications, Ltd., 1948. 17*s.* 6*d.* net.)

The application of the principles of physical chemistry to high polymers has been very successful in the last ten years, and the work of the organic chemist and chemical engineer in this field can now be supplemented by that of the physical chemist. Prof. Bawn's book gives an excellent, comprehensive account of the physico-chemical fundamentals underlying its subject; in fact, the book might well have been called "The *Physical* Chemistry of High Polymers." After a general introduction on the nature and types of synthetic polymers, the author deals with the kinetics of polycondensation reactions, and such matters as the molecular weight distribution of the high polymers formed thereby. Next follows a systematic treatment of addition polymerisation and co-polymerisation, the now classical rate and distribution equations being developed from a consideration of the elementary reaction steps: this chapter contains excellent sections on cross-linking and on the oxygen effect. The thermodynamics of polymer solutions and gels, and methods for determining the size and shape of macromolecules occupy the next two chapters: finally, polymer stereo-chemistry and crystallinity, and the relationship between polymer structure and physical properties are discussed in detail. A great deal of material, previously only available scattered in the literature or in expensive monographs, has been gathered together in this book, which contains references up to 1947. The only real criticism which must be made is that some subjects on which comparatively little

academic work has been carried out receive scant attention; for example, the urea- and phenol-formaldehyde resins are dismissed very briefly, while one feels that polyurethanes and polyethers are of sufficient importance for some mention. One may hope to see some extensions made in future editions, which, it is safe to predict, will be demanded. Among minor points which may call for comment, the effect of high viscosity on the termination reaction in certain vinyl polymerisations is mentioned only in connection with emulsion systems, and there is no reference to the earlier and more clear-cut results obtained in single phase reactions; while organic chemists will disagree with the formula given for the reaction product of benzoquinone and a vinyl di-radical, and with the statement that acenaphthylene does not form long-chain polymers. The book is clearly printed, there are few errors, and the price is very reasonable. All chemists interested in the fundamental study of high polymers will continually wish to refer to it.

C. E. HOLLIS.

Fourier Technique in X-Ray Organic Structure Analysis. By A. D. BOOTH, Ph.D. [Pp. viii + 106, with 48 figures, including 5 plates.] (Cambridge: at the University Press, 1948. 12*s*. 6*d*. net.)

THE growing interest of chemical crystallographers in the fine details of molecular structure, and particularly their desire to obtain bond-length measurements of sufficient precision to establish reliable standards of reference for comparison with wave-mechanical calculations, has in the last few years stimulated important developments in the technique of the interpretative part of X-ray structure analysis. Details of these developments are scattered through various journals or, in some not unimportant cases, have been known only through private exchanges of information between laboratories, and Dr. Booth has done good service in attempting for the first time to produce a collected account of them.

This book is essentially an introduction to the detailed technique of Fourier synthesis for research workers embarking on precision X-ray structural investigations, although more experienced workers in this field, and others, will find much in it to interest them. A working knowledge of the principles of elementary crystallography and X-ray diffraction, including space-group theory, is assumed, so that the intending structure analyst will be well advised to study an introduction such as Bunn's *Chemical Crystallography* first.

The unique feature of the book is the detailed description of calculating methods and mechanical aids. Though the results of X-ray analysis are of prime importance to them, the reluctance of many non-crystallographic chemists to familiarise themselves with the methods by which the results are obtained is understandable, if regrettable; unfortunately, however, this ignorance has tended to foster the belief among students that research in structure analysis consists almost entirely of computational drudgery, in which the researcher is little more than a cog in a vast structure-analytical machine. This book, with its intriguing pictures (not entirely devoid of Heath-Robinsonian touches) of how the machine really works, written by one whose enthusiasm has obviously not been diminished by a great deal of first-hand experience of it, should give intending research students a truer perspective.

In a book dealing with a subject which is undergoing such rapid development one could not expect to find the formal development of a completely

logical and infallible method of structure analysis ; it is not difficult, and very pleasant, to infer that the solution of many complex structures still depends not a little on the successful practice of the art and craft of the subject. Nevertheless, some sections of the book already begin to have an historical flavour, and it is certain that much of it would look very different if it were re-written now. We hope that it will soon be possible to produce a second edition recording the most recent advances in this important subject, many of which are being made by Dr. Booth himself. A second edition will also provide the opportunity to remove various errors, almost impossible to avoid in a first edition of a book which is largely mathematical ; one of the more serious which should be noted is the incorrect assignment of general positions in the space-group $P2_1/c$ (p. 22).

E. G. Cox.

The Chemistry of Organic Cyanogen Compounds. By Vartkes Migrdichian. [Pp. ix + 460.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. 72*s.* net.)

This book is a valuable addition to the American Chemical Society Monograph Series and it will receive a cordial welcome from organic chemists in general and from those who are primarily concerned with the study of cyanogen compounds in particular. Dr. Migrdichian has made a bold attempt to survey a large field, so large that those chemists who have made a special study of small sections of this field will find that some of the work (including recent work) has not been recorded in this book ; even so, it cannot fail to serve a very useful purpose, but its usefulness would have been enhanced if the author had brought his wide experience to bear on the critical examination of the material he has presented.

The book is divided into eighteen chapters and commences with an account of the formation and properties of nitriles, the account being supplemented with a list of about two hundred compounds in which certain constants and references to methods of preparation are given. The next chapter is concerned with the hydrolysis of hydrocyanic acid and nitriles, and this is followed by several chapters dealing with, *inter alia*, the action of strong mineral acids on nitriles, aminolysis of nitriles, simultaneous action of alcohols and acids on nitriles, reaction of nitriles with halogens, and the reactions of cyanogen halides and of cyanamide. Consideration is then given to reactions involving metallic cyanides, the reduction of nitriles, the reaction of hydrocyanic acid and alkali cyanides with aldehydes and ketones, the reactions of cyanohydrins, and the preparation of aminonitriles from aldehydes and ketones. A short account of the reaction of unsaturated compounds with hydrocyanic acid and alkali cyanides is followed by chapters on Gattermann's synthesis, the Houben-Hoesch synthesis, the reaction of Grignard reagents with nitriles, condensation reactions, including the Knoevenagel condensation (with eleven tables), the polymerisation of nitriles, and reactions of cyanates, thiocyanates, and *iso*cyanides. The book concludes with fifty-eight pages of author and subject indexes.

Among the many commendable features of this monograph mention may be made of the use which the author has made of structural formulæ, of the tables which he has compiled of physical constants and percentage yields of compounds, and of the extensive lists of references he has appended to the chapters. Although these lists are not complete, they will be invaluable to

every chemist who is interested in this field of work. Despite these and other omissions, Dr. Migrdichian has rendered a good service by producing in a handy form the most complete account of organic cyanogen compounds hitherto available.

W. H. BRINDLEY.

Chemistry of the Carbohydrates. By W. W. PIGMAN and R. M. GOEPP, JR. [Pp. xvii + 748, with 35 figures.] (New York: Academic Press Inc., 1948. $10.80.)

FOR many years, teachers of chemistry have been embarrassed by the question "Can you recommend a textbook on carbohydrates which will be something more than an introduction to the subject and yet which will not have the forbidding ponderosity of a reference book?" The answer to that question has now appeared—Pigman and Goepp.

The authors of this book describe it as a monograph written to "fill the urgent need for an introductory survey of the broad field of the carbohydrates, including sugars, derived products, and polysaccharides from organic, physical, analytical, biological, and industrial chemical aspects." It is the considered opinion of the reviewer that this comprehensive objective has been fully achieved.

The book begins with an historical survey, which rightly puts emphasis on the stereochemical aspect. Fischer's masterpiece of inductive reasoning, namely, the establishment of the configuration of the monosaccharides, is attractively arrayed in modern dress and, as an epilogue, the proof of ring structure is given and the furanose-pyranose nomenclature introduced.

The chapter devoted to "Occurrence, Properties, Analysis" is a mine of factual information without having the dullness which often accompanies purely descriptive writing. Succeeding chapters include within their scope all the known classes of sugar derivatives: esters, glycosides, acetals, oxidation and reduction products, ethers, anhydrides, and unsaturated glycals and glycoseens. A stimulating account of the inositols gives expression to the biological importance of these carbocyclic compounds and indicates the progress that has been made in relating them, in a configurational sense, to the ordinary carbohydrates.

The treatment of nitrogenous derivatives is so ordered that place is found for nucleic acid chemistry and related topics, and, under the same heading, the reactions of sugars with hydrazines and hydroxylamine are considered, due prominence being given to the use of sugar osotriazoles for the purpose of characterisation.

In the long chapter on "Naturally occurring Glycosides and Glycosidases," opportunity is taken for a thoroughgoing examination of the enzyme systems responsible for the hydrolysis and synthesis of the natural glycosides and oligosaccharides. This excursion into biochemistry is well-timed, since chemists are becoming increasingly concerned with the functional significance of carbohydrates. The imposing chemical edifice which has arisen on the foundations laid by Emil Fischer is now nearly complete and the builders are free to devote their labours to the larger issues of the chemistry of living things.

Not more than one-fifth of the text is devoted to polysaccharides, and this is as it should be in a book of this kind if a proper balance is to be maintained. The authors contrive, nevertheless, to give a fascinating picture of the problems

involved in polysaccharide chemistry, of the methods that are being devised for their solution and of the ferment of ideas and hypotheses which the newer discoveries have generated.

Where so much is in excellent harmony, it is difficult to introduce the dissonant note of criticism. The reviewer cannot, however, resist the desire to suggest that the authors of this book might have been a little less dazzled by American achievement, great as it is, in this field and a little more generous towards their British and European colleagues who, after all, well and truly laid the foundations on which the rest has been built.

S. P.

An Introduction to the Chemistry of Carbohydrates. By JOHN HONEYMAN. [Pp. viii + 143.] (Oxford: at the Clarendon Press, 1948. 15*s*. net.)

PROGRESS in carbohydrate chemistry has been rapid and few textbooks are available from which students can derive a general conspectus of the position now reached in this field. Dr. Honeyman's book will, therefore, receive a warm welcome and will interest all those who wish to gain insight into the fundamental problems of carbohydrate chemistry. It does not deal with biological and industrial applications, but is restricted to an authoritative survey of the structural chemistry of the carbohydrates. The greater part of the volume is devoted to the monosaccharides and includes chapters on configuration, synthesis, ring structure and oxidative degradation of the monoses, including ascorbic acid. The sections on oligosaccharides and polysaccharides are less detailed, but give the essential features of the simpler members of those groups. The author's difficulty has clearly been to select examples from the wealth of material available which will provide a balanced survey for non-specialist readers. In general, Dr. Honeyman has been most successful in his choice, but on occasion, for example in the account of the synthesis of disaccharides, it may be felt that the requisite balance has not been attained in the selection of examples. The proofs of structure are presented clearly and concisely with adequate formulæ which, with a few exceptions, such as the diagrammatic representation of amylopectin, are remarkably legible. There are commendably few misprints (but Loew's name is mis-spelt on p. 13) and one's chief criticism concerns the method of giving references to the literature. The author has rightly restricted the number of those in view of the nature of the book, and references to major discoveries are made by citing a name and year. This will, in general, suffice to locate a paper, but sometimes one such reference is used to cover work carried out several years after the given date and thus affords no help to the reader. Dr. Honeyman's excellent account of carbohydrate chemistry deserves a better system of references than this, and it is to be hoped that in subsequent editions, which will doubtless be called for, some alternative will be devised.

E. L. HIRST.

Recent Advances in Organic Chemistry. Vol. II. By ALFRED W. STEWART, D.Sc., and HUGH GRAHAM, D.Sc. Seventh edition. [Pp. x + 447.] (London: Longmans, Green & Co., Ltd., 1948. 35*s*. net.)

THERE must be many chemists who can look back with gratitude to the earlier editions of Stewart's *Recent Advances* both as informative reading and

as invaluable aid for examinations. The previous edition of the *Recent Advances in Organic Chemistry* dates back to 1931, although this was reprinted with additions in 1936, so that the appearance of a seventh edition with Dr. H. Graham as joint author will undoubtedly arouse considerable interest. Of the former two volumes, the first is now stated to be mainly of historical interest and is not reprinted, while the second has been rearranged as volumes II and III. The new volume II comprises a chapter on organic chemistry in the twentieth century, and chapters on some carbohydrate constitutions, some polysaccharides, pectic substances and alginic acid, sesquiterpenes, diterpenes, triterpenes, various groups of alkaloids, the indole group of alkaloids, anthocyanins, depsides and tannins, lignans and, finally, some theories of the natural synthesis of vital products. There are many outstanding omissions in this list, which admittedly excludes Volume III, but it is the author's privilege to choose his material. When, however, we turn to the treatment of the material and particularly the sections added to justify the title "Recent Advances," we are immediately conscious of the book's shortcomings. Thus, apart from a brief mention of the formula of penicillin in the first chapter, there is but one reference in the whole book to work reported since 1943 and, apart from the chapter on polysaccharides, only ten references to work reported since 1939. This is important, for not only are many recent advances completely overlooked but many of the statements are incorrect, having been amended by more recent work. Thus we can read on p. 162 that the double bonds of the azulenes are not fixed with certainty, which was certainly true in 1937, the last reference given to these compounds. Again on p. 135, the alternative formulæ given for cadinene are those given in an early paper of Ruzicka and Stoll (1924) and no mention is made of the accepted formula due to Soffer (1942), who also showed (1944) that the formula previously assigned to β-cadinene (p. 135) is in fact that of isozingiberene. This latter compound is misrepresented on p. 140, and similarly the structure given for copaene (p. 134) is now known to be incorrect. The degradative work on strychnine is not reviewed after 1934 and consequently a great deal of very important work is omitted, including the correct formulation.

The first and last chapters are of a more general nature and are the weakest in the book. The survey of chemistry in the twentieth century only goes to 1937, apart from the reference to penicillin already mentioned, but the majority of the chapter is devoted to work of twenty to thirty years ago. Thus an account is given of the applications of Tesla-luminescence spectra in organic chemistry, but only a very brief mention of u.v. and visible absorption spectra and no mention of i.r. and Raman spectra. The chapter is dated by the peroxide "tautomeric forms" of p-benzoquinone and diacetyl (p. 32) and by statements such as "By far the most interesting set of compounds prepared in the last quarter of a century have been the derivatives of various elements in a state of abnormal valency" (p. 4) and ". . . puzzling phenomena . . . of the Walden inversion; at the present day we still await a solution of the problem" (p. 18). The final chapter on the natural syntheses of vital products contains but one reference (1934) to work published since 1924. The above are examples taken at random; there are many more which could be chosen to show that the title is scarcely justified. These are subjects which deserve more than the occasional insertion of a paragraph or even a few new chapters into the text of seventeen years ago.

A. W. JOHNSON.

Advances in Protein Chemistry. Vol. IV. Edited by M. L. Anson and J. T. Edsall. [Pp. x + 575, with 111 figures.] (New York: Academic Press, Inc., 1948. 47*s.* 6*d.* net.)

The fourth volume of this valuable series follows the trend of its contemporaries in becoming longer (and more expensive) with each issue. The increased size arises from the greater space allowed to the contributors, the number of articles being the same as in previous years.

The physical and theoretical aspects of protein chemistry have been emphasised this year, and any comparison of the relative merits of the contributions will be strongly influenced by the personal interests of the reader. The formation and structure of protein gels is discussed by J. D. Ferry, who necessarily devotes much space to gelatin, the protein for which the most data have accumulated, but amongst others there is an interesting account of the fibrin-fibrinogen system. This is followed by an account, by F. W. Putman, of the interaction of proteins and synthetic detergents. This subject, which is of rapidly developing industrial importance, is shown to have considerable theoretical interest and to be capable of throwing light on other protein interactions. Well timed to coincide with the award of the Nobel Prize to Prof. Tiselius is the article from the same laboratory on preparative electrophoresis and ionophoresis. It becomes apparent that further development of these techniques, which could be invaluable, will be required before the preparative aspects achieve the same general utility as the analytical.

Much work and many reviews have been published on X-ray studies of proteins, so that it is interesting to find here a critical discussion of the results from the investigation of amino acids and simple peptides by the same method. Useful information has already been gained from this work on the basic units, and it is clear that there is considerable scope for its extension. Another article dealing with the amino acids is an authoritative review by A. Neuberger of the theoretical and biological significance of their stereoisomerism.

In a masterly article J. Wyman, Jr., discusses the physical, chemical and physiological properties of the hæm proteins. This is perhaps the best-known group of proteins and the vast amount of data available is concisely and clearly presented.

Of perhaps most interest to the biologists are an exhaustive discussion of plasma proteins in disease and a description of the proteins of pathogenic bacteria. Considerable interest has been aroused in the latter subject by the recent crystallisation of several bacterial toxins and the investigations of the contributor, A. M. Pappenheimer, Jr., into the mode of action of one of them.

This brief reference to the subjects covered in this volume indicates the interest of the series to workers in many widely separated fields of research.

R. R. Porter.

Petroleum Production. Vol. III: **Oil Production by Water.** By Park J. Jones. [Pp. x + 271, with 175 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall Ltd., 1947. 30*s.* net.)

The conditions of production from oil reservoirs are so complex that the practical operator tends to ignore the theoretical approach and base his judgment on common-sense application of experience gained from analogous cases. This series of textbooks by Park J. Jones is an attempt to provide

the concepts and formulæ to enable the engineer to predict oil reservoir performance on a quantitative basis.

The books deal with reservoirs consisting of porous and permeable media, but there is only brief mention of the problems connected with limestone fields and recovery from both fissures and pore space. Vol. I is concerned with the mechanics of production and Vol. II discusses conditions for optimum production ; Vol. III discusses the application of the formulæ of the previous volumes to problems connected with the recovery of oil by water injection.

About one third of Vol. III deals with foundations, including water encroachment, expansion of reservoir liquids, oil migration. The systems of injection wells for radial, linear and elongated reservoirs are discussed, together with modifications introduced by impermeable wedges, bottom water, and other complicating factors. A chapter is devoted to the economic aspect.

The book will be useful to the critical reader as a suggestive attempt to provide a simplified approach to the extremely complex problem of the quantitative evaluation of oil recovery by water drive. All the statements made, however, cannot be accepted without reservation, and the chief criticism is that the author does not clearly indicate the limitations of the formulæ quoted. Too often formulæ are stated without reservations or justification of any kind. In some cases the treatment may be misleading unless the reader is acquainted with Vols. I and II of the series or with the literature generally. Taking a case at random by way of illustration, the relation between maximum recovery and viscosity of reservoir crude and reservoir water is "defined" on page 2, with a reference to Vol. I where the relation is "suggested" (p. 149). Again, the relative permeability of porous media to oil and water is never even mentioned, although it is defined in Vol. I.

There are very few references to the literature.

B. N.

Petroleum Production. Vol. IV: **Condensate Production and Cycling.** By PARK J. JONES. [Pp. vi + 238, with 146 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. 30*s*. net.)

THIS volume, the last of a set of four, covers the problem of the exploitation of condensate reservoirs and can be divided into fundamentals, methods of production, economics of condensate production and applications. It has been planned and dealt with along the same lines as the three previous volumes and consists of a compilation of formulæ, graphs and methods of calculation, most of which have appeared in recent publications and which are reshaped in several cases.

The chapter on fundamentals gives some properties of hydrocarbon systems, with special regard to retrograde gases.

The methods of production are treated in four chapters: injection into rich gas, injection into water, production by expansion, displacement and expansion; they deal with the recovery of condensate by using extraneous energy such as injection of gas or water, or a combination of these means with the natural water drive, and with gas expansion. An analysis of the movement of the fluids within reservoirs of various types and shapes is made, and formulæ are given for the breakthrough and maximum recoveries for efficiencies and for economic limits of the operations. Based on the above,

indications are given as to when water or gas injection should be stopped and production continued by expansion alone, in order to ensure the maximum economic recovery.

The chapter "Economics of Condensate Production" provides some useful principles and formulæ for profitability calculations of condensate operations, and concludes that the selection of methods and rates of production is a function of the present value of the expected production.

The insertion of this chapter is welcome, especially for young engineers who are inclined to consider production problems exclusively from the technical point of view and to overlook their economical implications.

The above chapters are followed by applications to hypothetical radial, elongated and linear reservoirs, and by some considerations on maximum efficient rate.

The book is written in a difficult style, sometimes hard to follow, and there is no system and continuity in treating the various subjects. The calculations are insufficiently or not at all related to the underlying physical phenomena, the derivation of many formulæ is not given, nor is any mention made as to whether they are empirical or theoretically derived, and comparison of such formulæ with field observations is lacking.

References are insufficient for those who would like to follow a certain subject in more detail.

In conclusion, it may be stated that this book is a useful *aide mémoire* to the production engineer who is already familiar from other publications with the problems discussed herein.

C. A. DIMITRIU.

The Chemical Warfare Service in World War II. By the Chemical Corps Association, Washington, D.C. [Pp. iv + 222, with 3 portraits and 58 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. 24*s*. net.)

THIS volume is intended to be an interim summary of the accomplishments of the American Chemical Warfare Service (now designated the Chemical Corps) in World War II. It has been prepared for the benefit of the Chemical Corps Association, an organisation formed to maintain contact with all those who worked in the field of Chemical Warfare in the States, whether on research, development and production or as members of the various Chemical Warfare Units. Major-General Walter C. Baker, who was Chief of the Service from 1937 to 1941, was responsible for its compilation, but it would seem that his task has been to wield into a harmonious whole a mass of information supplied by a variety of sub-authors. This volume consequently suffers because of unequal treatment of the various parts of the story; for example, the technical side has not been given the importance it deserves. While the volume will have much interest for those connected with the operations of the Chemical Warfare Units in the field, it is not of such great value to those who desire details of the splendid research and development work carried out by the Service in association with the civilian American National Defence Research Council and with the intimate co-operation of the British and Canadian Research Organisations. In no phase of warfare were the contacts so close and the collaboration so harmonious, and for this the reviewer, who was in charge of the British research side, can never be too appreciative. It was a marvellous example of team-work in which the

British contribution was primarily in the field of new ideas and discoveries and the American in their development to the large-scale production and application stage. The field covered by the American chemical service was not restricted, as many people might think, to the problems of offence and defence against poison gas, but covered the much wider field of smoke screens —which played a leading part in the success of many operations—signal smokes, incendiaries of all kinds, including flame throwers, and by-products of these main activities, of which perhaps the most important was the large-scale application of insecticides such as D.D.T. to cope with the formidable malarial problems of the war against Japan. One of the most spectacular achievements was the development and use of their 4·2 inch mortar. The lack of any index detracts seriously from the value of this publication as a book of reference.

J. D. P.

Chemical Synonyms and Trade Names. A Dictionary and Commercial Handbook. By WILLIAM GARDNER. Fifth edition, revised and enlarged by EDWARD I. COOKE, M.A., B.Sc., A.R.I.C. [Pp. viii + 558.] (London: The Technical Press, Ltd., 1948. 50*s*. net.)

THAT this book not only meets but also satisfies a definite need is shown sufficiently by the call for a fifth edition. All users of the older editions will need the new one with its 3000 additional entries. For those not already familiar with the work, it should be explained that this is an alphabetical list of trade names and technical and commercial synonyms of natural and manufactured chemicals, of minerals, dyes, drugs, etc., and of some chemical preparations. So far as possible, the chemical composition is given, both in words and formula, often with an indication of the use, and sometimes a few salient properties are mentioned. The cross-referencing is extensive and adds greatly to the value of the book.

This work is valuable mainly to those who require a simple statement of a general nature, that is to the merchant, to the not too scientific manufacturer, to the pharmacist, and to the general reader requiring only a hint as to the general nature of a material. It will appeal less to the specialist or pure scientist. Most attention has been directed to minerals, alloys, dyes and pigments, and the older type of pharmaceutical preparation. A few "spot" searches showed a reasonable proportion of success. A book of this type cannot under present conditions be up to date; *e.g.* Paludrine is not included, but there are entries for Mepacrine and Plasmoquin; D.D.T, but not Gammexane, is recorded.

Obsolete terms are retained, "because it is often useful to know those materials which are now unobtainable." However, the next edition might well be severely pruned; entries such as "Oil of scorpions: oil in which scorpions have been digested," "Oil of smoke: creosote (*q.v.*)," "Derrid" (the definition of which is incidentally quite inaccurate), "Spirit of Hartshorn: a solution of ammonia; see Volatile Alkali" (!), should make way for modern material.

The organic chemistry is the weakest part of the dictionary. The nomenclature is erratic and does not follow any modern system, but there are few cases when the composition is really in doubt; the copious use of formulæ helps here too, although it is a pity to split so many between two lines. When

synonyms are available, that in pharmaceutical use is naturally preferred, but it seems wrong to have, for example, glycocoll without mention of glycine. Chemical trivial names for vitamins and trade names for preparations containing them are a notable omission from a work intended to appeal to pharmacists.

The book is well printed in clear type and is strongly bound as befits a desk-book for daily use. There seem to be few misprints.

R. S. C.

GEOLOGY AND GEOPHYSICS

Sequence in Layered Rocks. By R. R. Shrock. [Pp. xiv + 507, with 397 figures.] (New York, Toronto, London : McGraw-Hill Book Co., Inc., 1948. 45*s*. net.)

The sub-title of this important work is " A Study of Features and Structures useful for determining Top and Bottom or Order of Succession in Bedded and Tabular Rock Bodies." It contains comprehensive descriptions and illustrations of features in sedimentary, igneous and metamorphic rocks, which can be used by field geologists to determine the correct order of succession in folded or overturned beds and layers. The book is expressly designed for advanced students, and geologists investigating structures and successions in the search for oil, coal and metals. Specialists in sedimentary petrology will also find the book of great value.

After the first and introductory chapter, Chapter II deals with " Gross Relationships " (superposition, faunal succession, unconformity) ; and then come three chapters on Features of Sedimentary Rocks—Chapter III, Lithological, Textural, Mineralogical Sequences ; Chapter IV, Features on Upper- and Under-surfaces of Sedimentary Beds ; Chapter V, Internal Features. Finally Chapters VI and VII treat respectively of Features of Igneous Rocks and Features of Metamorphic Rocks. All the features listed as possibly useful in determining sequence in bedded and layered rocks are fully considered with reference to their variations, limitations, and dependability for the required purpose ; and some features dealt with in this book have never before been considered in this way or used as criteria for distinguishing top from bottom.

The Introduction (Chapter I) deals with the purpose and scope of the work, the plan of presentation of the data, the definitions of terms, the nature and variations of the features discussed, and the nature and origin of stratification. In regard to the last-named, Dr. Shrock points out how little fundamental research has been done on this most characteristic feature of sedimentary rocks, and commends this field of work to the consideration of researchers. The statement (p. 25) that " the order of succession is from younger at the top to older at the bottom " reads rather oddly, as if time went backward.

In Chapter VI igneous petrologists will find the features of tuffs, lavas, sills, and layered igneous rocks dealt with from the unusual point of view of the identification of top and bottom structures, with a wealth of new references and a number of most ingenious diagrams. It will certainly stimulate research on the conditions of eruption and intrusion of ancient igneous series.

In Chapter VII, metamorphic rocks are treated from the same point of view and in the same way, but in the nature of things the criteria of attitude

must be confined to inherited structures which are not completely obliterated by metamorphism.

The references are confined largely to North American sources, and the author keenly regrets that he has not had the time and opportunity to search British and Continental literature, in which, however, as the reviewer may assure him, there is a wealth of material on the subject of this book. The work is well written and splendidly illustrated. There are misprints on p. 79 and p. 100, and in the Table on p. 65 the grain-size of siltstone is wrongly given. This book will long remain the leading work on its subject.

G. W. T.

Swartland and Sandveld. A survey of Land Utilization and Soil Erosion in the Western Lowland of the Cape Province. By W. J. TALBOT. [Pp. xii + 79, with 8 folding plates and 59 figures.] (London: Oxford University Press on behalf of the University of Cape Town, 1947. 10*s*. 6*d*. net.)

THE south-western part of Cape Province of the Union of South Africa is a well-defined region lying between the edge of the high plateau on the east and the Atlantic Ocean on the west. It comprises two types of country. The coastal flats, until recent geological times covered by a shallow sea, are at the present day occupied by extensive sand deposits and known by the local name of the Sandveld, or Strandveld. In this flat plain there are occasional granite and quartzite masses which must formerly have been islands. The Sandveld, which includes the Cape flats, gives place inland to more undulating country, again with remains of old mountain masses. This second region of undulating country is known locally as the Swartland. Some of the mountain masses which rise from the Swartland are steep-sided spectacular hills of the type so well known from Table Mountain.

All this country enjoys a Mediterranean type of climate, with mild winters and hot dry summers. Except on high ground, frost is unknown over practically the whole of the region and, providing the "former rains" (August and September) are adequate, it is a region where nature may be said to smile kindly on the work of the farmer. True, such fruit trees as European apples, pears, peaches and plums may fail to distinguish which is the cold resting period and seem uncertain when to start their spring activities, with the result that they may fail to fruit properly, but all that vegetation which we associate with sunny Mediterranean lands will flourish in this climate.

The curse of the whole is that terrible scourge of soil erosion. Some twenty-five years ago the Union of South Africa made a pioneer study of the evils of soil erosion, but when the Social and Economic Planning Council was established by the Union Government in 1942, it was found that much of the basic knowledge essential to sound land planning in this area was simply not available. The present Report is one of several regional studies intended to supply that basic knowledge. It was undertaken under the auspices of the University of Cape Town, and Prof. Talbot, who is Professor of Geography there, has seized the opportunity of presenting a well-written, magnificently illustrated and wholly satisfying account of the area, intelligible to every reader, yet scientifically accurate and full of information which is of importance far beyond the immediate limits of the investigation.

Fortunately the whole area was covered in 1938 by an air survey undertaken for the new Topographical Survey of the Union. The photographs

have been studied stereoscopically and comparisons made with field observations in 1943 and 1944. Even in the brief space of five years tiny rills had become deep gulleys, a number of existing gulleys had lengthened and multiplied, land well cultivated in 1938 had been abandoned as useless. Whilst gulley erosion can be fairly clearly seen from the photographs, the sheet erosion, said to affect not less than 95 per cent. of the cropped land in the Swartland, is less easy to detect, and in addition there is wind erosion.

The story presented by the numerous maps, the magnificent series of photographs and the clear explanatory diagrams is one repeated in many parts of the world, and one is left in no doubt of the urgency of the problem of soil conservation if the world is not to starve itself to death. Prof. Talbot rightly says that soil erosion is the brand mark of bad farming, and sets out a tentative programme as a result of his findings. He suggests in the first place that land use must be planned in accordance with the limitations imposed by the natural conditions of climate, soil and relief, in order to avoid sub-economic grain production in areas of low and unreliable rainfall better suited to stock raising, and in order to protect by permanent vegetation areas particularly liable to erosion.

He suggests in the second place that farm economy must be re-organised, particularly to secure a balanced mixed farming which will preserve and build up the soil rather than deplete it. In the third place, land already damaged must be dealt with and further soil erosion prevented, whilst an intensive programme of education in sound principles of conservational farming must be "propagated by every possible means." Finally, he notes the serious effect of the policy of guaranteeing prices for wheat under the Wheat Control Board, and suggests that their policy must be revised.

The Report has been delayed in publication. Though dated 1947 it was not issued until the middle of 1948. Now it is available it is a book which should be studied by all who are concerned with the conservation of land throughout the world.

L. DUDLEY STAMP.

An Introduction to the Theory of Seismology. By K. E. BULLEN, M.A., Sc.D. [Pp. xiv + 276, with 43 figures.] (Cambridge: at the University Press, 1947. 18*s*. net.)

A VERY noticeable gap in geophysical literature has been filled by Bullen's new book. The author, who is Professor of Applied Mathematics in the University of Sydney, describes his work as an attempt to meet the needs of seismologists as well as of undergraduates who are interested in seismology as a branch of applied mathematics. The great variety of relevant theoretical and observational data has been packed most successfully into this slender volume. Should the reader require additional information, he will refer to standard works, particularly those by Jeffreys, to whose inspiration the author, his former pupil and collaborator, pays great tribute.

The book opens with a concise outline of the mechanics of a deformable body and a general survey of wave motion. This is followed by the mathematical theory of elasticity (good use is made of tensor treatment) and of vibration and waves, which comprises about half of the text and includes bodily elastic and surface waves, reflection and refraction, and seismic rays in a spherically stratified earth-model.

Instructive chapters analyse the principle of the seismograph and the

construction of the Travel-Time-Tables, which are the practical seismologist's main tool in treating the instrumental data and in disentangling the often very involved records. Applications to the investigation of the structure of the interior of the earth are fully dealt with. It is left to the last two chapters to introduce the vast accumulation of data on conditions at the earthquake-focus. The related problems discussed here include estimates of the energy liberated in earthquakes, their magnitudes, possible causes, geographical distribution and the important questions of the depth of the focus. On the problem of periodicities and the attempt to predict earthquakes we quote with delight: "A distinguished Canadian seismologist reports his answer to a question concerning the time of occurrence of the next major earthquake in a certain region in the words of Mark Twain: I was gratified to be able to answer promptly, and I did. I said that I did not know."

Finally, a short chapter, "Further Topics," mentions some border-line subjects such as the effect of earthquakes, design of shock-resisting structures, microseisms with their latest successful application to the forecasting of hurricanes, and the effective practical methods of seismic prospecting. The very last paragraph, "Atomic bombs and seismology" (giving also Gutenberg's statement that the atomic bomb energy was of the same order as that of an average earthquake), fittingly illustrates the up-to-date character of Bullen's admirable book. Its structure may be considered a model.

A. Beer.

BOTANY

Plants and Environment. By R. F. Daubenmire. [Pp. xiv + 424, with frontispiece and 87 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 27*s.* net.)

This is an ecological textbook which describes and analyses in some detail the wide group of habitat conditions which affects plant growth. The subject is introduced by an attempt to define what is meant by environmental factors and habitat and how they affect plants. The view is taken that a habitat factor exhibits optimum and limiting intensities affecting any plant species. Outside the limiting intensities the species cannot survive. Within them the species is presumably most efficient around the optimum value. Little evidence is given to illustrate this generalisation.

The factors themselves are discussed in some detail in the next seven chapters, dealing with soil, water, temperature, light and atmosphere as habitat factors, and finally with the nature of biotic factors. It is somewhat curious to a British reader to find a whole chapter devoted to fire effects. The two final sections of the book deal with the environmental complex and its interplay and with ecological adaption and evolution.

On the whole, the survey of habitat factors is well done. There are, of necessity, in a book of limited size and scope, some things omitted, but there is a laudable attempt to give a balanced picture of the effects of habitat on plants. The book is well illustrated and clearly written, containing numerous references, though these deal mainly with North American examples, as do the illustrations. This is not a disadvantage, on account of the wide scope of the treatment in other respects.

There is perhaps one criticism that may be levelled at this book, that is that the title of the volume should be inverted or even turned into "the

environments of plants." In a large part of the book, plants are treated in an abstract sense—much like the ideal "plants" of the physiologist—and species and genera are hardly named at all. In fact, the field botanist almost always has so often to deal with a specific type with its own peculiarities and requirements that it is rarely possible to generalise. Thus it must be said that the work is not altogether free from the common failing of many ecological books—that of trying to generalise too widely. Successful ecology is largely the investigation and analysis in individual examples of the relation between an organism and its environment. Thus the subject often loses its force when generalised, so that attempts to teach ecology on general lines almost always leave an impression of vagueness. As an account of the ways in which habitat may affect plants this book will serve a very useful purpose, even though it may not be the ideal textbook of plant ecology, or even intended to be so. It will be of value to the British reader in particular as drawing attention to the soil types, and the ecological problems which are met with in the wider continental areas.

W. H. P.

The Growth of Plants. By G. W. CROCKER. [Pp. vi + 459, with 171 figures, including 7 coloured plates.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. 60*s*. net.)

DESPITE the title, the subject matter of this work includes topics, such as research on insecticides and fungicides, that are only indirectly connected with the growth of plants, but even if the contents, by reason of their miscellaneous character, are only remotely related they constitute a fitting memorial to Colonel Boyce Thompson, since they summarise the twenty years of research carried out in the institute which he endowed and which bears his name.

A mining engineer by profession, Thompson is quoted as having said that "by helping man to study plants I may perhaps be able to contribute something to the future of mankind," and in this belief he founded the institute, which was opened in 1924.

We read here of the investigations on Aster Yellows and the discovery of the incubation and transmission of the virus by the leaf hopper *Cicadula sexnotata*, of Peach Yellows and its transmission by *Macropsis trimaculata*.

The work carried out on the causes of the sudden disappearance of the abundant ducks and geese in the Chesapeake Bay area, subsequent to the enlargement of the canal, was an excellent example of the importance of the balance of life, since it was shown that the disappearance of the phanerogamic food plants of the birds was an indirect effect of higher salinity, producing conditions favourable to superabundant growth of the Hydroid *Cordylophora lacustris*, bringing in its train turbidity of the water and a wholly unfavourable fauna and flora. Researches on life span and dormancy in seeds, with which the editor himself was chiefly concerned, occupy over 100 pages, in which the influence of the seed coat on dormancy and the effects of after-ripening and stratification are considered.

In Chapters 4 and 5 the effects of physiologically active gases, such as ethylene, and of lethal ones, such as sulphur dioxide, are considered. Further chapters treat of hormones, of dormant buds, of cell membranes and the growth of plants under controlled conditions, particularly in respect to illumination.

The work of the institute itself is placed in a setting of investigation carried out elsewhere, and each chapter has an adequate bibliography, so that the entire production constitutes a useful summary with respect to the diverse subjects of enquiry; it is therefore the more to be regretted that the volume is so costly.

E. J. SALISBURY.

The Potato. By W. G. BURTON. [Pp. xiv + 319, with 25 figures, including 15 plates.] (London: Chapman & Hall, Ltd., 1948. 25*s*. net.)

THIS book covers a wide field. It deals with the history of the potato in the British Isles, potato varieties, factors influencing yield and content of dry matter, composition and distribution of dry matter in the tuber, nutritive value, cooking quality and storage. The author's main object is to summarise the knowledge of the potato as a source of food, particularly from the viewpoint of a plant physiologist. There are ten chapters, each with a useful list of references, and three appendices, one devoted to uses of the potato other than as food, one in which specific gravity as a guide to the dry matter and starch content of tubers is discussed and one giving a conversion table.

It is natural in a book of this type, covering such a field, to find the author more at home in some chapters than in others. The author's imprint is greater in the last four chapters than in the remainder of the book, which to some extent is a compilation of existing knowledge on the subjects. The section dealing with those factors influencing yield and dry-matter content is somewhat incomplete. Amongst the factors not mentioned are: (*a*) the root systems, which in dry years assume considerable importance; (*b*) vegetative variations of all kinds, *e.g.* bolters, wildings, aberrant types generally, and clonal differences which are deserving of serious note owing to the modern system of building-up stocks from individual plants; and (*c*) some diseases and pests, such as Blackleg, Powdery Scab and Slugs. Should a second edition be published, it would widen the appeal of this book were the chapter on storage extended to include more data on the structural side of storage buildings.

The illustrations are all good, although Figs. 8, 9, 10 and 11 are not helpful. Stipules are shown in Fig. 2; leaves of the *Solanaceæ* are, however, exstipulate.

The author appears more conversant with U.S.A. than with continental literature and some Scottish references are omitted.

The book is well written and will be useful for reference to all those to whom the potato crop is important.

T. M.

A Handbook of Coniferæ, including Ginkgoaceæ. By W. DALLIMORE, I.S.O., V.M.H., and the late A. BRUCE JACKSON, A.L.S. Third edition. [Pp. xvi + 682, with frontispiece, 39 plates and 120 figures.] (London: Edward Arnold & Co., 1948. 50*s*. net.)

THE first edition of this comprehensive work on the Family of Conifers was published in 1923, and was followed in 1932 by a second edition. Sixteen years have now elapsed, eventful botanically as well as politically, and the present (third) edition will be warmly welcomed. This new edition brings up to date the record of all species of all the genera of this important Family,

which includes species the products of which are indispensable to the aid and comfort of mankind. The one omission, noted by Mr. Dallimore in his preface, is that of the newly discovered *Metasequoia*, particulars of which did not arrive in this country until too late for inclusion.

An introductory chapter gives a broad survey of the principal characteristics of conifers, their timbers and economic uses, propagation and cultivation, and diseases and pests, together with a synopsis of families and species and a key to the genera in cultivation which, supplemented by the keys to species in the text and the excellent drawings, should enable students to identify any conifer to be met with in Great Britain. In this edition the old, familiar, specific names have been changed to comply with International Rules: thus *Larix europea* becomes *L. decidua* and *Pseudotsuga Douglasii* is changed to *P. taxifolia*. Some will feel regret at this surrender to orthodoxy, especially because it is undesirable that the nomenclature of species of economic value should be at the mercy of arbitrary regulations, and because under the Rules the decisions of taxonomists must always remain liable to possible further revision. Synonyms are freely quoted, however, and the authors must perhaps be deemed right to conform.

Descriptions of individual species and varieties are given with great clarity. Additional information and more detailed descriptions concerning species of particular value or interest include notes of remarkable specimens, cultural information, and the properties of timbers and other products.

The untimely death of Mr. Bruce Jackson has thrown the main burden of the preparation of this edition on to Mr. Dallimore's shoulders, a burden which, as the edition proves, has been ably sustained. The 39 full-page photographs portray many noble specimen trees and are well reproduced, while it must again be said that the numerous drawings by Miss G. Lester are beyond praise for their accuracy and merit.

This edition should be in the hands of all tree lovers. It is indispensable to students, gardeners, and nurserymen, and should find a place in every country house library.

W. L. T.

BIOLOGY

Our Enemy the Termite. By Thomas E. Snyder. Second edition. [Pp. xiii + 257, with frontispiece and 84 figures.] (Ithaca, N.Y.: Comstock Publishing Co., Inc.; London: Constable & Co., Ltd., 1948. 20*s*. net.)

In these days of increasing colonial development and of fuller realisation of the importance in this connection of the damage caused by insects to man, crops, and stored products, it is surprising that, in comparison with other pests, greater attention has not been given to termites (white ants) which, in tropical and sub-tropical countries, may cause extensive damage to buildings, and to materials containing cellulose. This may in part be due to the lack of easily available publications on the habits and control of these insects, which would help the uninitiated to prevent or reduce the damage so often regarded as inevitable.

This need for a semi-popular yet scientifically accurate account of termites has been met in the United States by Dr. Snyder's book, first published in 1935. Dealing essentially with termite problems in the U.S.A., it is none the less most useful to all concerned with these insects in other parts of the

world. The publication of a revised edition in 1948 will be warmly welcomed by British workers as being particularly opportune when a greater interest is being taken in termite control within the British Commonwealth, especially in the colonies.

Slightly less than half of this edition again deals with the classification, biology, and habits of termites, the remainder giving a comprehensive account of the damage caused and of control measures. In parts, the text has been re-written or re-arranged, and amplified in the light of recent knowledge and experience. Examples of such additions include: a fuller account of the use and limitations of termite shields in the foundations of buildings; an expanded description of the use of chemicals as soil poisons; more detailed information on the control of dry wood termites. An account of the fundamental principles of control as advocated by the National Pest Control Association in the United States is a valuable addition to the Appendix, which also includes keys to the genera of the groups of termites in the States.

The new edition is attractively produced, with chapter and other headings set out more clearly than formerly, and the numerous illustrations include several additional photographs.

R. C. Fisher.

On the Periodicity of Salmon Reproduction in the Northern Baltic Area and its Causes. By T. H. Järvi. Conseil Permanent International pour l'Exploration de la Mer, Rapports et Procès-Verbaux des Réunions, Vol. CXIX. [Pp. 131, with 34 figures.] (Copenhagen: Andr. Fred. Høst & Fils, 1948. Kr. 15. –.)

From scale analysis of 30,000 salmon caught 1936–44 and river water heights and temperatures Prof. Järvi determines the age composition of the Finnish catch and attempts to ascertain the chief causes of the annual fluctuations in the stock.

The smolt age is dominantly three years (84 per cent.): small numbers are two (9 per cent.) and four (7 per cent.) years respectively.

The catch in the Gulf of Bothnia is chiefly of large spring fish (3 winters in sea)—38 per cent.; small summer fish (2 + winters)—29 per cent.; together with a smaller number of grilse (1 + winters)—17 per cent.; and of very large fish (4 winters)—11 per cent.; previously spawned fish form 5 per cent. of the total. The proportions vary materially in the three main rivers: the Kemi and the Oulu are big-fish rivers (63–70 per cent. of 3 and 4 winters in sea fish and only 5–9 per cent. grilse); the Tornio holds more young fish (41 per cent. of 3 and 4 winters fish, 34 per cent. of 2 winters fish and 21 per cent. grilse). Those are total figures for 1936–44: the individual years show great variation in all three rivers.

Assuming that 6-years-old fish are dominant (although the 3 winters in sea fish form on the average less than half the total), Prof. Järvi finds that generally an ample breeding stock produces an equally good stock in the appropriate (seventh) succeeding year for possibly four generations and vice versa, but he does not find any satisfactory explanation for what he regards as the abnormal results. His approach is fresh, but much more knowledge is required of the youngest stages of the fish before the factors producing the success or failure of a brood can be evaluated: Prof. Järvi's results suggest that these factors in river and sea alike are stable in the majority of years

—a result that is difficult to accept. A more exact analysis than is provided by an average total age of 6 years is desirable.

The illustrations of the very distinct scales are excellent, but the blocks for Figs. 9 and 10 have been transposed.

W. J. M. MENZIES.

The Ways of Fishes. By LEONARD P. SCHULTZ and EDITH M. STERN. [Pp. xii + 264, with 80 figures.] (New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1948. 22*s*. net.)

THE scope and purpose of this book are clearly stated in the preface by Dr. Schultz himself: "This book will make no attempt to give a comprehensive picture of the behaviour of fishes. It will simply tell something about the habits of those fishes which seem to me the most unusual and interesting. My object in writing it is to share with others who are not ichthyologists, at least a little of the enjoyment my colleagues and I get out of knowing the bizarre, astonishing and manifold ways of our favorite animals." Certainly, in sixteen more or less self-contained and independent chapters, dealing with first one and then another aspect of fish-life and study, the author reveals his own enthusiastic interest in a homely and infectious manner—he writes with an eagerness to entertain and excite interest, rather than to instruct—and he does so as a citizen of the United States, addressing fellow-countrymen, in their own home. I do not say that readers on this side of the Atlantic will not find interest and pleasure in this book, but they may feel as they read that it was not really written for them. For my own part, I would consider it a good companion to the late Dr. Norman's *History of Fishes*, but not as a substitute for it. A word should be said concerning a classification of fishes, given as an appendix extending over some 30 pages. As this amounts to little more than a list of orders, sub-orders and families, one wonders why so much space has been devoted to it—those pages would surely have been better filled with one or more readable chapters. Dr. Schultz reminds readers that, among active ichthyologists, practically no two agree upon the arrangement of the various categories of classification: he regards such disagreement as a healthy condition in the field of ichthyology, because it indicates that new facts are being discovered and these change the conceptions of relationships, thus altering the arrangement of the orders and families of fishes. I wonder how many will agree with him entirely? Some of us, at any rate, regard the continual state of flux in systems of classification and of the scientific naming of fishes, even the most common and best-known ones, as a major trial and tribulation, save, perhaps, to the taxonomist himself!

E. F.

Rational Fishing of the Cod of the North Sea. The Buckland Lectures for 1939. By MICHAEL GRAHAM. [Pp. 111, with frontispiece and 9 figures.] (London: Edward Arnold & Co., 1948. 5*s*. net.)

I THINK it should be mentioned that this valuable little book is wider in its scope than is indicated by its title. There are three chapters. The first is a study of the life and work of Frank Buckland as the "father" of modern fishery research—an account which supplements in a useful way the one given by Prof. W. Garstang in the inaugural Buckland Lecture of

1929. The second is devoted to an historical survey of the development of the conception of "rational" fishing, from about 1883 onwards, and links up the major contributions made by Huxley, MacIntosh, Holt, Cunningham, Fulton, Peterson, and others, with the more recent work and opinions of E. S. Russell, W. F. Thompson, Baranov, and Mr. Graham himself, with his colleagues at the Lowestoft Laboratory. In the third chapter, the cod of the North Sea, and the important fishery dependent upon them, are considered in light of the requirements of a rational exploitation. Altogether, the book is a most handy reference manual—one to give to any serious student wishing to learn what has already been done, and what still remains to be done, in securing for mankind the fullest and most enduring benefit from a commercial exploitation of the fish stocks of the seas. The references given are wisely and skilfully chosen, and they are introduced without detracting from the interest and argument in the text. It is pleasing to note that the book is dedicated to Dr. E. S. Russell; there are many other workers in all parts of the world who with Mr. Graham will think of him as "Master, Guide and Friend."

E. F.

Animal Breeding. By L. M. WINTERS. Fourth edition. [Pp. viii + 404, with 155 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 27*s*. net.)

THIS edition has been considerably changed and expanded to include the recent rapid developments in the practical application of science to problems of reproduction and genetics in farm animals. The author's opinions on breeding should command considerable respect, since he has produced a new breed of pig—the Minnesota No. 1—which is now being widely used in the U.S.A.; he gives full details of the methods he used. Throughout, not only are scientific principles discussed, but the ways in which these can be used in application to the larger and slower breeding farm animals are pointed out. This is particularly so with respect to inbreeding and out-crossing with the utilisation of hybrid vigour, for the practical application of these principles to farm animals differs considerably from the methods which have been so successfully used with maize in plant breeding. He concludes that if full advantage is to be made of hybrid vigour in livestock production, crossbred females need to be utilised as breeders. In pigs, for example, he finds that the Three-Breed sows have an advantage of 20 per cent. in the number of pigs weaned per litter, 60 per cent. in the litter weight at weaning and 3·8 per cent. in saving of feed over the corresponding Pure breeds, due to the fact that for half the bacon pig's normal life it is dependent upon the mother for nourishment. He shows also that these same principles of inbreeding and crossing inbred lines can also be applied to the improvement of pure breeds. Other interesting features of the book, which will be of use to our breeders at the present time, include lists of the lethal characters found in farm animals, methods used in Progeny testing, the heredity of different characters, and improvements by "grading up." There are also interesting illustrations of the development of the sheep from the egg to the lamb at birth, and chapters on fertility and artificial insemination. The book should prove most useful to those who are engaged in the practice of livestock improvement.

J. H.

Advances in Genetics. Vol. II. Edited by M. Demerec. [Pp. viii + 373, with 66 figures.] (New York: Academic Press, Inc., 1948. $7.50.)

Genetics as a progressive science is barely fifty years old. Yet from its beginning as the effort of a few enthusiasts seeking to establish the general rules of hereditary transmission, it has come to command the activities of scientists throughout the world, and to range in its field of interest from biochemistry to sociology, from the commercial breeding of fungi to the treatment of cancer. The annual output of original literature has grown enormously: and it is still growing. The non-geneticist cannot hope to keep up with it all, and even the genetical specialist is finding it increasingly difficult. The value of authoritative reviews, collating and summarising developments in selected branches of the science, can therefore need no emphasis. They will be appreciated by all, and not least by the geneticist himself.

It is with this welcome aim that *Advances in Genetics* is published. In the present volume we have articles on Cytoplasmic Inheritance by E. Caspari, Genetics of Human Populations by Gunnar Dahlberg, Genetics of Cancer by W. E. Heston, Cytology of Coccids by Sally Hughes-Schrader, the New Systematics and the Nature of Species by Ernest Mayr, Cytology and Genetics of Wheats and their Relatives by E. R. Sears, and Genetic Effects of Radiations by D. G. Catcheside.

Several of the reviews are essentially compilations of existing information, and indeed some of them could at present be little else. Dr. Caspari makes available for the first time in English the wealth of German observation on the intricate relations of nuclear genes and permanent cytoplasmic bodies. In Dr. Sears' article we can find the latest information and views on the genetic relation of the wheat group and on the origin of wheat itself. The peculiar cytological puzzles of the coccids are laid before us by Dr. Hughes-Schrader, and we can see how their solution will aid not merely the special taxonomy of this group but also the general theory of chromosome mechanics. From a combination of taxonomic and genetic observations Dr. Mayr concludes that "species are natural units characterised by their reproductive isolation from other such units," and that in cases of difficulty the taxonomist must supplement his morphological criteria by the more genetical one of genic immiscibility. In considering human genetics Prof. Dahlberg concentrates on what he regards as the basic mathematical foundation, for the development of which the right sorts of observation are needed, but are not yet available. Finally, in a most valuable article, Dr. Catcheside shows us how physical theory can be built on biological fact.

The modes of treatment adopted by the seven authors vary as widely as their subjects, but they combine to make this volume a valuable addition not only to the genetical, but also the general biological, bookshelf.

K. M.

Soviet Biology. By T. D. Lysenko. [Pp. 51.] (London: Birch Books, Ltd., 1948. 2*s*. 6*d*. net.)

By this time people are becoming a little tired of the controversy over Lysenko and Soviet genetics, because the subject has been used as a sounding board for political propaganda by Marxists and anti-Marxists alike. However, the book under review is an important piece of evidence, and it is perhaps worth while to analyse the situation just once more.

The basic facts are simple to grasp. For over ten years there have been two schools of genetics in the Soviet Union: one indistinguishable from genetics elsewhere, the other making a number of unfamiliar assertions which involve a denial of the "laws" of Mendelism, which attribute comparatively little importance to the role of chromosomes, and which depend upon the inheritance of acquired characters. Last July one of these schools secured official recognition and support from the Soviet Government, and the other school is now likely to perish through lack of funds for research, lack of opportunity for publication, and petty persecution. The book under review, which must be read to be believed, is Lysenko's speech on this historic occasion. It contains nothing the student of Soviet genetics did not already know; and no data which are not already eight years old. But it is authentic Lysenko; and it is as clear and forceful a presentation of Soviet genetics as one could expect.

In a country where Science and State are so closely integrated as they are in modern Russia, one cannot disregard the political aspects even of genetics. Therefore there are two questions at issue. The first is: What is the experimental evidence in support of Lysenko's views on genetics? The second is: Has there been in Russia a Government-inspired, deliberate suppression of a group of research workers, solely on account of their scientific work? Both these questions can be answered clearly. To the first question one can answer categorically that Lysenko and his colleagues have never published data which in the least justify their views on genetics. It may be true that workers on *Paramœcium* and bacteria and *Neurospora* have revolutionary evidence which indicates acquired enzymatic adaptation, not inconsistent with some of Lysenko's views. But Lysenko himself knew nothing of this work as late as 1946 and he has never referred to it in his writings. Therefore, on the canons of criticism which are current throughout the scientific world, Lysenko's views on genetics must be dismissed as unsupported by his data.

As to the second question, there is no doubt that geneticists who base their research on the foundations laid by Mendel and Morgan cannot now work openly in the Soviet Union. This issue has been clear only since August 26, 1948, though some Russophobes create the impression that it has been clear for ten years.

What is the reason for it? A careful reading of Lysenko's speech provides the answer. In a national emergency most scientists are willing to be directed into national service, and to put aside academic research for the time being. The Soviet Union is in a state of national emergency all the time. Her annual food surplus is dangerously low. Her farms are inefficient. Her livestock is pitiably poor. Plant and animal breeders must therefore be mobilised to fight (as *Pravda* puts it) "on the agriculture front." It is a tragedy that genetics should be suppressed, and it quite properly arouses indignation in Britain and America. It is important that, beside being indignant, we should understand.

ERIC ASHBY.

Heredity. By A. FRANKLIN SHULL. Fourth edition. [Pp. 311, with frontispiece and 153 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1948. 24*s*. net.)

THE third edition of this standard work was almost completely re-written in 1938, and now, ten years later, we have the fourth edition, which has

been revised to include some of the more important recent advances in genetics.

This revision has resulted in a somewhat shorter book, with twenty-six in place of thirty chapters. The reduction, with some changes in the headings, has been achieved by a different presentation of some of the material, and one new chapter. There has consequently been a condensation of some of the subject matter, and this has made possible the inclusion of new material without making the book too unwieldy. These changes are an improvement, while the reduction in the thickness of the book by using thinner paper is a great advantage.

The author has tried to bring the book up to date both in his treatment of the fundamental science of genetics and in the practical applications. This is a difficult task in a subject which is advancing and developing so rapidly as is genetics, and the author has done well to present the recent developments in experimentation and in scientific thought so cogently and clearly.

The book was originally written with special reference to the reader who is "more interested in human applications than in any other features of heredity." This meant that the genetic phenomena included either occurred in man or were concerned with characters of domestic animals or cultivated plants which were of economic interest. This method of selection of topics has in general been followed in the subsequent editions, and now in the fourth edition certain phenomena have been omitted usually because they have not yet been demonstrated to occur in man.

The special interest of the book, therefore, continues to be in the application of genetics to human activities, and the later chapters, devoted almost entirely to human heredity and the practical problems of eugenics, population and immigration, are the culmination of a treatment of heredity which will continue to have a wide appeal.

G. D. H. B.

A Laboratory Manual of Comparative Vertebrate Embryology. By Allyn J. Waterman. [Pp. viii + 248, with 60 plates.] (New York: Henry Holt & Co.; London: Constable & Co., Ltd., 1948. 21*s*. net.)

This work is well-defined by its title; it is a laboratory manual of embryology, not a textbook, but it contains an astonishing amount of valuable information, much of it arranged in the form of tables. Some of these may be mentioned as they will give a good idea of the scope of the work:—Sperm Viability; First Appearance of Primordial Germ Cells; Seasonal Reproduction in Male and in Female Animals; Œstrous Cycles and Ovulation; Brooding Habits of Amphibia; Hatching Times; Gestation Periods; and Delayed Implantations among Mammals.

The book is well provided with bibliographic references, many of which are to very recent experimental work. There are instructions on what to observe in the laboratory, and questions for revision. The illustrations, almost all photomicrograms, are clear and original, but the British reader would have expected to find them provided with label-lines and labels, particularly in the case of the selected transverse sections through chick and pig embryos.

The work is strictly objective, to the exclusion of theoretical considerations, and should prove useful.

G. R. de Beer.

Human Embryology and Morphology. By Sir Arthur Keith, F.R.S. Sixth edition. [Pp. xii + 690, with 578 figures.] (London: Edward Arnold & Co., 1948. 40*s*. net.)

This familiar treatise is unique among its kind in purpose and construction, for no other book attempts (let alone so successfully achieves) that combination of morphological and embryological data which has characterised the present work from its inception at the beginning of the century. The value and the sustained appeal of the book rests largely upon its skilful and attractive integration of the topographical, the embryological and the comparative in the elucidation and interpretation of human form and function, and in this art Sir Arthur is past master. Throughout he describes and illustrates ontogenetic processes against an interpretative background of phylogeny and evolution.

This new edition summarises and incorporates with impressive felicity the advances made during the last fifteen years in an ever-widening and progressively more complex field, yet without unduly enlarging the bulk of the volume or defacing the hallmark of personality with which it is so emphatically stamped.

Two new chapters have been found necessary and the number of illustrations has been increased from 535 to 578. These latter, all black-and-white, are generally well chosen or conceived and establish their point by reason of their clarity and apposite simplicity.

The references and notes at chapter ends summarise a wealth of relevant literature and permit the author comments and reflections upon divergent views regarding particular developmental processes. Remarkable and commendable is the tribute paid therein to the work of the British school of anatomists, thus dispelling any erroneous notion that the growth of embryological science is exclusively the product of the German and American schools.

The volume is clearly and attractively printed and is well indexed. It contains not only a plenitude of essential embryological data, but also much related information not readily procured elsewhere. It is educative as well as instructional and will commend itself to students of medicine and embryology and to all who have a professional interest in the morphogenesis of the human body.

A. J. E. Cave.

BIOCHEMISTRY

Advances in Enzymology and Related Subjects of Biochemistry. Vol. VIII. Edited by F. F. Nord. [Pp. xii + 538, with 46 figures.] (New York and London: Interscience Publishers, Inc., 1948. 48*s*. net.)

This book is the latest in a series of volumes designed to present critical discussions of topics in biochemistry, particularly in relation to enzymology. The editor has avoided repetition of reviews published elsewhere by selecting the less-well-developed aspects, many of which await the application of enzymic methods of research.

A review on alkaloid biogenesis by R. F. Dawson is noteworthy in this respect, being the first on a subject which has so far been largely the domain of organic chemists and plant physiologists. The critique of the methods used will provide a valuable basis for development in this field.

J. P. Greenstein gives a timely review on the dehydropeptidases which covers all the work done on this recently discovered class of enzymes.

A very comprehensive account of fat metabolism is given, the article on fatty acid catabolism by F. L. Breusch being worthy of particular mention. The author gives a detailed discussion of research from the classical work of Knoop up to the present day. Two further articles, one by A. Kleinzeller dealing briefly with lipid and wax synthesis, the other by S. Bergström and R. T. Holman on autoxidation of unsaturated fatty acids, cover additional aspects of fat metabolism, while a review by I. L. Chaikoff and C. Entenman on the antifatty-liver factor of pancreas presents a very readable account of this related topic.

An article, entitled "Functioning of the Cytoplasm," by L. Monné, sets itself the difficult task of correlating the physical structure of the cell with its biochemical activities. The approach is uncritical and some of the conclusions reached by the author, such as the one that " . . . activity of all enzymes is controlled by lipides," cannot be accepted on the evidence cited. Nevertheless, the review is worth attention for the interesting account of cell structure given. The author emphasises that biochemistry must eventually be interpreted in terms of cellular organisation. A useful account of the choline-esterases is included in an interesting article on the enzymes of snake venoms by E. A. Zeller.

A wide range of subjects is dealt with in the book and only lack of space prevents mention here of other articles. A cumulative index for the whole series is included. The volume maintains the high standard of its predecessors and can be warmly recommended.

W. H. E.

Recent Progress in Hormone Research. Vol. II. Edited by GREGORY PINCUS. [Pp. vi + 427, with 148 figures.] (New York: Academic Press, Inc., 1948. $8.00.)

THE second session of the Laurentian Hormone Conference took place in September 1946 and included many of the most active investigators in hormone research in the United States and in Canada, and also Prof. B. A. Houssay from Argentina. The record of the papers and the free discussions which took place after each paper has now been published as Vol. II of *Recent Progress in Hormone Research.*

The meeting was divided into a number of sessions dealing with different aspects of the subject and covering a very wide range of interests. The sections as published are as follows: *Section I*: Physical Methods in Hormone Research. The characterisation of sterol hormones by ultraviolet and infrared spectroscopy, by R. Norman Jones; The identification and quantitative microdetermination of estrogens by ultraviolet absorption spectrophotometry, by Harry B. Friedgood and Josephine B. Garst. *Section II*: Pituitary Control and Activity. The pituitary-adrenal system, by George Sayers and Marion A. Sayers; Adrenergic control of the release of luteinizing hormone from the hypophysis of the rabbit, by J. E. Markee, C. H. Sawyer and W. Henry Hollinshead; The hormonal control of lactation, by W. E. Petersen. *Section III*: Hormone Metabolism. The role of factors of the B-complex in estrogen metabolism, by Roy Hertz; The metabolism of androgens, by Ralph I. Dorfman. *Section IV*: Hormonal

Regulation of Metabolism. The hormonal influences on the secretion of insulin, by Evelyn Anderson and Joseph A. Long; The production of experimental glycosuria in the rat, by Dwight J. Ingle; Physiology and endocrinology of the toxic thioureas, by Curt P. Richter; The action of the thyroid on diabetes, by Bernado A. Houssay. *Section V*: Aspects of Clinical Endocrinology. Sex hormone deficiencies. Some clinical considerations, by E. Perry McCullagh; The pathologic physiology of adrenal cortical tumors and Cushing's syndrome, by Edwin J. Kepler, Randall G. Spraguo, Harold L. Mason and Marschelle H. Power; Genetic and endocrine factors in the growth and development of childhood and adolescence, by Lawson Wilkins.

Section I bears witness to the growing importance of spectroscopy not only in the basic sciences but also in application to biology. The relative simplicity of use of the complicated apparatus involved and the objectivity of the results obtained thereby are factors exerting a big influence on investigations depending for their development on the estimations of steroid hormones. The articles in this section bring together material which otherwise is widely scattered through the literature and provide a very useful summary of work which has been developed in this field since the last edition of Prof. R. A. Morton's classic work on the *Application of Absorption Spectra to the Study of Vitamins, Hormones and Co-enzymes.*

The advance of investigations dealing with steroid hormones is naturally more rapid than that in respect of protein hormones such as those from the anterior pituitary gland, and in Section III, concerned with the metabolism of estrogens and androgens, in Dr. D. J. Ingle's article (Section IV), of which a large part is devoted to adrenal steroid diabetes, and in Section V, where sex hormone deficiencies and pathological over-secretion of adrenal hormones are discussed, steroid hormones are of outstanding importance.

The paper on the hormonal influences on the secretion of insulin by Anderson and Long is of particular interest to the reviewer and owes much to the development of an elegant method of perfusion of the pancreas of the rat. Endocrine research must certainly draw its pabulum from a wide range of subjects and this and other articles bear vivid witness to the necessarily wide interests of the endocrinologist.

This volume is one which every worker in the field of hormones should, and probably will, have on his bookshelf. It does not provide a complete survey of the various aspects of the subject which would be comprehensible to the non-specialist, and indeed it does not set out to do so, but as a discussion by experts themselves of their recent investigations it is of prime importance, and it is perhaps a pity that this volume has not been available to workers in Great Britain more quickly after the Conference itself took place.

The expenses of the Conference were met through contributions from a large number of American commercial houses, and the fostering of such conferences in similar circumstances is an example which might be commended. The editing of the volume and the responsibility for its production must have been an arduous task for Dr. G. Pincus, and he is to be congratulated on the final result. As Dr. Pincus notes in his preface, the interest of those supporting these Conferences "has made possible a series of Conferences that gives promise of becoming a vital institution."

F. G. Y.

PHILOSOPHY AND HISTORY OF SCIENCE

Déterminisme et Libre Arbitre. Entretiens présidés par Ferdinand Gonseth, Professeur à l'École Polytechnique Fédérale, recueillis et rédigés par H. S. Gagnebin. Second edition. [Pp. 191.] (Neuchatel: Éditions du Griffon, 1947. London Agents: H. K. Lewis & Co., Ltd. 10*s*. net.)

THIS well-produced little book consists of four dialogues based on discussions which took place in Switzerland in the later years of the war, under the presidency of Prof. F. Gonseth of Zürich. Those taking part (they are, in fact, symbolic representatives of the views expressed by those of their type in the actual discussions) include a mathematician, an astronomer, a physicist, a physician, a sociologist, a psychiatrist, an engineer, a philosopher, an individualist and a sceptic. There is also, of course, the President, and a Host who speaks from no recognisably distinct point of view, but since presumably he provided the refreshments he was obviously entitled to say his piece and, as is only just, he has in effect the last word. The debate pursues a leisurely course from the evidence of determinism (tempered but not destroyed by the intrusion of probability), which is discovered by all the sciences, to the undoubted fact that, unless one is free to choose the true from the false and to arrange his experiments in a legitimate way, science itself becomes impossible. The practice of science thus presupposes freedom and discovers determinism. A contradiction! *Qu'y faire?* The principle of uncertainty, or complementarity, in modern physics suggests the answer. A new dialectic must be adopted in which human action can be regarded either under the aspect of freedom or under that of necessity, but not both together. The two views are complementary, and contradiction is thus impossible. Everyone is satisfied—or at least silenced—and the meeting ends happily.

It is all very nicely done, but it may be doubted whether, when one comes to apply the conclusion to a particular case, he will find the old misgivings and crooked questions quite dispersed. When the coster's finished jumping on his mother, is the proper sequel a fit of remorse or is he to lie a-basking in the sun with the comfortable reflection that his behaviour is the necessary effect of his (and her) hormones? But perhaps we should not ask too much, and the reader of the book will find himself well repaid by the clearness of expression, the numerous apposite quotations, and the elucidation of some knotty points.

H. D.

Fontes Historiæ Botanicæ Rossicæ. By VLADIMIR C. ASMOUS, B. A. (*Chronica Botanica*, Vol. 11, No. 2). [Pp. 87–118, plates 53–56.] (Waltham, Mass., U.S.A.: The Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1947. $1.25.)

THIS comprehensive catalogue of (as the author hopes) "all publications which deal, in some way, with the history of botany in Russia," is arranged in one alphabetical author-sequence, with a useful analytical subject-index in three sections (bibliographies; biographies; history and progress of botany). Each item is followed by a very short summary of its contents. Russian titles are transliterated into roman characters—a feature which will be regretted by students of Russian and will convey little information to others—and are provided with an English translation except where a French or German title and résumé are supplied in the original.

The reproductions of views or plans of botanic gardens and other vanished splendours of the nineteenth-century give a slightly nostalgic flavour to the work. The frontispiece showing a page from a seventeenth-century Russian manuscript herbal is interesting.

Workers on the Russian flora should find this list a valuable adjunct to their studies. For this reason we are glad to note that, whereas the publication itself is marked "for Subscribers and Editors of *Chronica Botanica* only," a later price-list from the Chronica Botanica Co. lists the work as "available to non-subscribers," at $1.25.

H. K. Airy Shaw.

Tour on the Continent 1765. By Thomas Pennant, Esq. Edited with Notes by G. R. de Beer, F.R.S., P.L.S. [Pp. xii + 178, with frontispiece and 8 plates.] (London: The Ray Society, 1948. 17*s.* 6*d.* net.)

The Ray Society deserve our gratitude and congratulations. They have brought to the light of day a book about eighteenth-century men, their modes and their manners, their creations and religions, their food and their drink.

Thomas Pennant is best known to us as the correspondent to whom Gilbert White wrote most of his memorable letters. Neither as an observer of natural phenomena nor as a writer of beautiful English prose can Pennant be compared with White. Pennant, however, though he never approached the magic of Gilbert White's style, could write pleasantly about natural objects, as when he describes the lime tree, page 83; lizards, page 85; laburnum, page 90; glaciers, page 107; and avalanches, page 109. In Pennant's *Tour of the Continent* references to Natural History are comparatively few. A large part of the book consists of descriptions of buildings, especially churches and their inventories. His meetings with leading men of the day make interesting reading, specially his description of Voltaire (page 76). "At *Ferney*, in the extremity of the same province [Burgundy], I visited that wicked wit *Voltaire*; he happened to be in good-humour, and was very entertaining; but in his attempts to speak *English*, satisfied us that he was perfect master of our oaths and our curses" (page vii). The eight plates of scientists are of great charm. It is amusing to compare the attire of Rousseau "herborisant" with that of a present-day field botanist. Pennant was a many-sided man, and the *Tour on the Continent* is a rich store of interest and amusement, which will find a happy resting-place on many bedside tables.

The book is well printed with numerous incut notes. The introductory matter containing an "*extract from the literary life of the late Thomas Pennant, Esq.*, By himself . . ." is an illuminating prelude to the Tour.

There is an index of proper names, but not of subjects. A general index would have been useful, though its compilation would have been a formidable task.

H. Gilbert-Carter.

Scientific Discovery. By R. R. Butler, M.Sc., F.R.I.C., F.C.S. [Pp. 252, with 71 plates.] (London: The English Universities Press, Ltd., 1947. 15*s.* net.)

For teachers who may be interested in the history of scientific discovery and invention and who may wish to impart some of that interest to their pupils,

the author has set out his material in brief studies arranged in a series of anniversaries, one or two for each week of the year. Whether this is or is not the best way to inculcate a sense of history may be argued : the reviewer is emphatically of the opinion that it is not. However, many interesting biographies and other facts are here gathered together ; the human and social aspects are considered ; and the illustrations have been well chosen. It would be an advantage if the fable of Galileo's having dropped weights from the Leaning Tower of Pisa were expunged from our textbooks, but it seems to bear a charmed life.

D. McKie.

MISCELLANEOUS

Papers of the Michigan Academy of Science, Arts and Letters. Vol. XXXI (1945). [Pp. x + 324, with numerous illustrations.] (Ann Arbor : Michigan University Press ; London : Oxford University Press, 1947. 21*s*. net.)

The appearance of this latest volume in the series of papers from the Michigan Academy has been long delayed. It contains papers which were to have been read at the 1945 meeting of the Academy, which owing to wartime restrictions was cancelled. The date on the title-page is 1947, but the volume did not appear until May 1948.

The high standard of production of previous volumes is maintained, with clear type and excellent line or half-tone figures.

In the section of Botany Miss Ruth Chen-Ying Chou has a second paper on the Pacific species of *Galaxaura*, with descriptions of the sexual forms. Clarence R. Hanes contributes some notes on the habits of water-plants in Kalamazoo County, and W. C. Steere has a check-list of the Bryophytes of Michigan with a full list of literature. Dr. W. Randolph Taylor contributes a third paper on the Algæ collected by the *Hassler, Albatross* and Schmitt expeditions, this one dealing with marine algæ from Peru and Chile ; nine new species and two new forms are described.

In the Forestry Section Dr. D. V. Baxter has a detailed study, illustrated by photographs, of the development of forest in parts of Alaska, and the interaction of tree and fungus populations. Dr. Baxter also continues his studies of resupinate Polypores with a seventeenth paper in the series "Resupinate Polypores from the Region of the Great Lakes." These studies are of the greatest value, and it is satisfactory to note that Dr. Baxter contemplates a comprehensive treatment of the resupinate Polypores of North America.

Zoology is represented by five papers, namely : A. M. Chickering on the male and female types of *Storena barroana*, R. R. Dreisbach on three genera of Hymenoptera, Philanthidæ, C. L. and L. C. Hubbs on natural hybrids between two species of Catastomid fishes, Lagler and Clark Salyer on the food and habits of the common water-snake, and G. C. Steyskal on Nearctic species of *Xylomyia* and *Solva*.

In the geology section there is a short paper recording the finding of Pennsylvanian plants in glacial drift near Jackson, Michigan. The remainder of the volume is made up by papers on anthropological, sociological, psychological, historical and literary subjects.

E. M. W.

Science in Progress. Edited by George A. Baitsell. Fifth series. [Pp. xvi + 353, with 151 figures.] (New Haven: Yale University Press; London: Oxford University Press, 1947. 27*s*. 6*d*. net.)

As is becoming well known, each of the books in this series contains a number of self-contained articles by scientists engaged in active research in the fields about which they write. The articles are based on lectures given to various University audiences under the auspices of Sigma XI, the American Scientific Research Society. As the title of the series indicates, each article is intended to present to non-specialists an authoritative picture of recent progress.

The latest volume again covers a wide variety of subjects. Perhaps the contribution of greatest general interest is the Introductory Article by Dr. F. B. Jewett on "The Future of Scientific Research in the Postwar World." This article should be read by everybody, because it states sanely, concisely and convincingly many of the views of thoughtful scientists in this country and in U.S.A. During the war practically all fundamental research in the physical sciences ceased, and there were few students receiving the broad preparatory training so necessary for successful advanced work. In fundamental science we have lost irrevocably the better part of a generation of creative research men and the knowledge they would have contributed. There is a popular misconception of what science really did do during the war, and what it can do now. Scientific men are being consulted, and in a few instances are giving advice, on matters concerning which they are not qualified to speak. These and other important topics are discussed in an excellent article.

The other ten chapters are much more specialised. J. B. Macelwane discusses our knowledge of the earth's interior from the point of view of a seismologist. D. W. Kerst describes the development of the betatron and discusses various applications of high energy radiations (including X-rays) in medicine, physics and engineering. H. S. Taylor outlines lucidly the development of theories of contact catalysis between the two wars, particularly activated adsorption. L. Michaelis ("Fundamentals of Oxidation and Respiration") discusses oxidation in organic chemistry, the role of free radicles, and the behaviour of hæmoglobin. M. Heidelberger describes the curious properties and behaviour of the substance "complement" found in blood serum. G. W. Beadle describes some detailed investigations on genes, giving a very good idea of the care, patience and insight involved. P. Rous gives a well-written account of recent work on cancer, very suitable for the general reader. E. C. Stakman ("Plant Diseases are Shifty Enemies") begins by outlining the history of plant diseases, and then shows how particular fungus diseases have been studied, and how rapidly the problems change as the emphasis is shifted from one factor to another. C. C. Speidel describes his own work on cinéphotography of changes in various types of cells in the tails of tadpoles, and, finally, H. M. Evans gives an account of recent work on anterior pituitary hormones.

The present reviewer cannot pretend to pronounce on the accuracy of articles covering such a wide range of recent work, but the standing of the authors is some guarantee. Most of the articles are well written and illustrated (in fact, some of the illustrations are just "pictures," and do not add much to the text). The book is well printed, except for the small amount of mathematics, and well produced. It would be a queer scientist indeed who could not find something interesting and new to him in the text.

F. A. V.

The Presentation of Technical Information. By REGINALD O. KAPP, B.Sc.(Eng.), M.I.E.E. [Pp. xii + 148.] (London: Constable & Co., Ltd., 1948. 6s. net.)

Is it only a coincidence that so soon after the publication of Whitaker-Wilson's *Modern English Speech* and Sir Ernest Gower's Government-sponsored *Plain Words* there should appear another book on the proper use of the English language? Or is it a sign that we are at long last growing conscious of the ways in which our mother tongue is so often misused? The technical man, alas! is one of the worst offenders, and it is to him that the book under review is addressed. After all, much of the literature that now circulates at home and abroad is technical literature, and it behoves its authors to learn to use the pen as effectively as they have learnt to use the microscope or the slide-rule. For this reason, if for no other, all scientists would do well to study *The Presentation of Technical Information*; there are few who will put it down unrewarded. Nor should they be put off by the rather ponderous title. This is a book about the writing of English, not about English grammar or style, but about "the art of conveying information," as the author expresses it. What a pity he did not use this as his title, and so acknowledge his evident affinity with the Quiller-Couch tradition.

No executive responsible for the control of scientific work will quarrel with Prof. Kapp about the importance of his self-appointed mission, and all will be profoundly grateful to him for having produced such a sensible, readable treatise—if a book of 140 small pages can be so described. There can be few groups of educated men capable of producing such involved, tortuous and ambiguous expressions as scientists. The task of reading and assimilating technical papers and reports, of assessing the significance of the facts presented in them, and of taking a decision on such facts is difficult enough when the information is clearly presented. But many scientists appear able to convey their thoughts only in dull, lifeless, involved language and they qualify their conclusions by so many provisos that the search for the truth becomes in itself a major research. There may be various good reasons for this; the scientist has been trained to worship at the shrine of scientific method, and he is often over-cautious in endeavouring to present fairly the pros and cons of his thesis; he often assumes that his reader is as familiar as he is with the background of his work and he, therefore, may not bother to refer to the more obvious steps of his arguments; sometimes, unfortunately, he apparently does not even bother to think out the reasons why his information ought to be of interest to his reader, and so he fails to emphasise the high-lights of his work.

Such is the sad state of affairs that Prof. Kapp sets himself to remedy in this modest little book. It is to be hoped that all technical men who have to prepare reports—and which of them has not—will buy this book, study it and pass it on to others to read. Many are probably quite unaware of the literary crimes they daily commit and to them this book will come as a revelation. Others, uneasily aware that their writing is not always as stimulating as it should be, and anxious to know what to do about it, will find many of the answers in this little volume. Prof. Kapp devotes a good deal of attention to an analysis of the mental processes of the person addressed, as he rightly holds that an understanding of these is the key to the art of conveying information. He shows how important it is that new ideas should be linked in some way with what is familiar to the person addressed. He

stresses the importance of avoiding the twin evils of circumlocution and over-compression, and gives much sound advice on how to attain a desirable mean between the two. Perhaps those who during the war achieved the then laudable habit of brevity in writing, a habit that is still regarded as a virtue in those who read papers before learned societies, will profit from his salutary warning that a concise, laconic style may be as trying to the reader or the hearer as the more usual fault of prolixity.

Prof. Kapp's book is itself a model of the clear, functional style that the author advocates. It is a delight to read, and contains not one superfluous paragraph nor one dull page. Every chapter is enlivened with some amusing illustration or some striking phrase that drives home the point he wishes to make. This is the type of book that has long been needed and it can be warmly recommended to all, young or old, who wish to see their scientific results presented in a manner worthy of their intrinsic importance.

F. A. Robinson.

The Genius of Industrial Research. By D. H. Killeffer. [Pp. x + 263, with 7 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. 27*s*. net.)

An intriguing title and an intriguing book, easy to read, fascinating in the steady onward sweep of the story as it unfolds, with a multitude of appropriate illustrations always, as it were, ready to hand—in short, a book well worth study and a place in any appropriate collection.

To avoid misunderstanding, the word genius refers to that superior inborn quality or power of mind or special faculty possessed by some individuals which has resulted in a selected few becoming identified with particular outstanding discoveries. The examples of successful researches chosen as examples, and often discussed at length with quotations from the original papers or other writings of those responsible, are, from the nature of the subject, mainly chemical in character.

Five important chapters deal with process research, product research, equipment research, the pilot plant and what happens there, and one entitled "Reports are Essential." Literature reading and plain thinking about the problem are not sufficient; reporting may be necessary to keep colleagues and others informed of progress or lack of it for business or other reasons, but it is vital to efficiency that the individual should report frequently to himself as it were, and so by each successive step or new statement of the problem narrow it progressively until the crucial experiments are visualised and realised.

The first process described, which aptly illustrates and justifies this thesis, is the apparently simple operation of making ethyl acetate from the alcohol and acid. Now, it may seem strange to state that as recently as 1919 the fact had to be learned, and learned by experiment, that sulphuric acid plays a catalytic, rather than a dehydrating, role in the reaction between ethyl alcohol and acetic acid. The impulse to investigate the matter was prompted by the fact that the reactants, although as technical grades available in quantity and relatively cheap, are expensive to concentrate and dehydrate.

Other stories of processes, described in the words of the inventors, are the Twitchell process for hydrolysing fats, Hall's work on aluminium, Weston on electroplating of nickel, and of course Langmuir, that master of the art

of securing the greatest possible good from his experiments, on the gas-filled lamp and atomic hydrogen.

The principles covering the approach to product research are illustrated by the story of synthetic rubber, and the production of nitrocellulose lacquer out of vast war stocks after World War I; nitrocellulose naturally led to the story of celluloid, the inventor of which was primarily concerned with product research to win a prize of 10,000 dollars offered for a satisfactory substitute for ivory billiard balls; from celluloid to plastics is an easy transition and so to the major developments in resin-forming compounds, which in turn have led to a wide variety of quick-drying surface coatings of high durability.

Every page of the book has its interest and brings its quota to the argument, for there is scarcely a process of consequence to which reference is not made somewhere in the text—high octane gasolene, the recovery of bromine from sea-water, the synthesis of methanol, gelled gasolene for incendiaries, the desulphurisation of petroleum, the alkaline isomerisation of drying oils and many others.

The chapter on equipment research starts with three general rules which aptly reflect the realism of the writing: First, learn all of the fundamental requirements for the process or the machine and the special conditions that must be met; secondly, select a line of research as far removed as possible from past, present or future activities of any competitor so far as those are known. If possible do not compete at all; the third principle is—do only one thing at a time. . . . That is the modern approach.

At the other end of the time scale the methods advocated by Leonardo da Vinci, Francis Bacon, Charles Darwin (through the eyes of his son Francis) and many others are described. Thus it is recorded that Leonardo stated his basic method of research as follows: "In treating any particular subject . . . my design is first to refer to experiments and then to demonstrate why bodies are constrained to act in such a manner. This is the method we ought to follow in investigating the phenomena of nature. Theory is the general, experiments are the soldiers. Experiment is the interpreter of the artifices of Nature. It is never wrong: but our judgment is sometimes deceived because we are expecting results which experiment refuses to give. . . ." Leonardo's personal success in such widely varied fields attests the value of his method, which may be expressed shortly and succinctly as "Think first, then experiment, and do not deceive yourself about the results."

Not a little of the charm of this book is found in the delightful spread of ideas on a philosophic background, which continually shines through the developing argument about research methods, stating the problem, thinking out the problem, the development of an idea and evaluation. It may well be said that "though strait be the gate which leads to valid discovery, indisputable truth, it is well to learn from those who have made the passage".

L. A. JORDAN.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Theory of Equations. By J. V. Uspensky, late Professor of Mathematics, Stanford University. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. viii + 353.) 27*s*. net.

Number Theory and its History. By Oystein Ore, Sterling Professor of Mathematics, Yale University. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. x + 370, with frontispiece and 24 figures.) 27*s*. net.

Regular Polytopes. By H. S. M. Coxeter, Professor of Mathematics in the University of Toronto. London: Methuen & Co., Ltd., 1948. (Pp. xx + 321, with frontispiece, 8 plates, 88 figures and 8 tables.) 50*s*. net.

Hyperbolic Functions. Their Derivation and Applications in Vector Algebra. By C. A. Grover, M.I.E.E. Manchester: Classifax Publications, 1948. (Pp. ii + 40.) 4*s*. net.

Algebra by Visual Aids. Book 1: The Polynomials; Book 2: The Continuum; Book 3: The Laws of Calculation; Book 4: Choice and Chance; and Answers. By G. Patrick Meredith, M.Sc., M.Ed., under the Editorship of Lancelot Hogben, M.A., D.Sc., F.R.S. London: George Allen & Unwin, Ltd., 1948. Book 1 (pp. 1–160 + xxxiv, charts 1–43), 10*s*. net; Book 2 (pp. 161–282 + xvi, charts 44–82), 8*s*. 6*d*. net; Book 3 (pp. 283–414 + xxvi, charts 83–109), 7*s*. 6*d*. net; Book 4 (pp. 415–550 + xxii, charts 110–146), 9*s*. 6*d*. net; Answers (pp. 96), 6*s*. net.

The Night Sky. By J. G. Porter, Ph.D., F.R.A.S. The Winchester Study Library. London: Winchester Publications, Ltd., 1948. (Pp. 96, with frontispiece and 29 figures.) 5*s*. net.

Ballooning. By C. H. Gibbs-Smith, Companion Royal Aeronautical Society. A King Penguin. London: Penguin Books, 1948. (Pp. 40, with 32 plates and 5 figures.) 2*s*. 6*d*. net.

Principles of Mathematical Physics. By William V. Houston, Rice Institute. Second edition. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. xii + 363, with 41 figures.) 30*s*. net.

Kinematic Relativity. By E. A. Milne, M.A., D.Sc., F.R.S., Rouse Ball Professor of Mathematics in the University of Oxford. The International Series of Monographs on Physics. Oxford: at the Clarendon Press, 1948. (Pp. viii + 238.) 25*s*. net.

Energy and Matter. By R. L. Worrall, M.B., Ch.M., D.P.H. (Pp. 144.) 10*s*. 6*d*. net.

Thermodynamics. By Edward F. Obert, Associate Professor of Mechanical Engineering, Northwestern Technological Institute, Northwestern University. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. (Pp. xiv + 571 with 207 figures and 36 tables.). 33*s*. net.

Thermodynamics. By Lester C. Lichty, Professor of Mechanical Engineering, Yale University. Second edition. New York and London: McGraw-Hill Book Co., Inc., 1948. (Pp. xiv + 341, with 152 figures, 26 tables and 12 charts.) 27*s*. net.

Physical Aspects of Colour. By Dr. P. J. Bouma, Philips Research Laboratories, Eindhoven. Philips Technical Library, distributed by the Cleaver-Hume Press, Ltd., London. (Pp. 312, with 113 figures and 15 tables.) 30*s*. net.

Photography in Crime Detection. By J. A. Radley, M.Sc., F.R.I.C. London: Chapman & Hall, Ltd., 1948. (Pp. 186, with 104 figures, including 72 plates.) 21*s*. net.

Preparation and Characteristics of Solid Luminescent Materials. Edited by Gorton R. Fonda and Frederick Seitz. Symposium held at Cornell University, Oct. 24–26, 1946. Sponsored by the Division of Electron Optics of the American Physical Society. Published under the auspices of the National Research Council. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xvi + 459, with numerous figures.) 30*s*. net.

Optical Design and Lens Computation. By B. K. Johnson, Lecturer in the Physics Department (Technical Optics Section) of the Imperial College of Science and Technology. London: The Hatton Press, Ltd., 1948. (Pp. viii + 175, with 80 figures, including 5 plates.) 36*s*. net.

The Optical Principles of the Diffraction of X-rays. By R. W. James, M.A., B.Sc., F.Inst.P., Professor of Physics in the University of Cape Town. The Crystalline State, Vol. II. London: G. Bell & Sons, Ltd., 1948. (Pp. xvi + 623, with 224 figures, including 16 plates, and 30 tables.) 80*s*. net.

Atoms in Action. By George Russell Harrison. Third edition. London: George Allen & Unwin, Ltd., 1948. (Pp. x + 410, with 16 plates.) 12*s*. 6*d*. net.

The World and the Atom. By C. Møller and Ebbe Rasmussen. Translated from the second Danish edition by Gerald C. Wheeler and Bernard Miall. London: George Allen & Unwin, Ltd., 1948. (Pp. 192, with 41 figures, including 8 plates.) 12*s*. 6*d*. net.

A Textbook of Radar. A Collective Work by the Staff of the Radiophysics Laboratory, Council for Scientific and Industrial Research, Australia. London: Chapman & Hall, Ltd., 1948. (Pp. viii + 579, with 347 figures, including 31 plates, and 11 tables.) 50*s*. net.

Supersonic Flow and Shock Waves. By R. Courant and K. O. Friedrichs, Institute for Mathematics and Mechanics, New York University. Pure and Applied Mathematics, Vol. I. New York and London: Interscience Publishers, Inc., 1948. (Pp. xvi + 464, with 216 figures.) 42*s*. net.

Underwater Explosions. By Robert H. Cole. Princeton, N.J.: Princeton University Press; London: Oxford University Press, 1948. (Pp. x + 437, with 7 plates, 121 figures and 22 tables.) 42*s*. net.

Centrifugal and Other Rotodynamic Pumps. By Herbert Addison, O.B.E., M.Sc., M.Inst.C.E., M.I.Mech.E., Professor of Hydraulic Machines at the Fouad I University, Giza, Egypt. London: Chapman & Hall, Ltd., 1948. (Pp. x + 492, with 236 figures and 2 folding plates.) 36*s*. net.

Photoelasticity. Vol. II. By Max Mark Frocht, Ph.D., Research Professor of Mechanics, Illinois Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp.

xviii + 505, with frontispiece and 368 figures, including 1 coloured plate.) 60*s*. net.

Engineering Metallurgy. By W. E. Woodward, M.A.(Cantab.), University Lecturer in Engineering, Cambridge University. London: Constable & Co., Ltd., 1948. (Pp. x + 176, with 6 plates and 35 figures.) 15*s*. net.

Elasticity and Anelasticity of Metals. By Clarence Zener. U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1949. (Pp. x + 170, with frontispiece, 56 figures and 13 tables.) 22*s*. 6*d*. net.

British Chemical Nomenclature. By A. D. Mitchell, D.Sc., F.R.I.C. London: Edward Arnold & Co., 1948. (Pp. viii + 156.) 21*s*. net.

British Chemicals and their Manufacturers. The Directory of the Association of British Chemical Manufacturers, 1949. (Pp. 141.) Gratis to purchasers of chemicals.

Physico-Chemical Methods. Vol. III (Supplementary). By Joseph Reilly, M.A.(Cantab. and N.U.I.), D.Sc.(N.U.I.), Sc.D.(Dub.), D.ès.Sc.(Geneva), F.Inst.P., F.R.C.Sc.I., F.R.I.C., Professor of Chemistry, National University of Ireland, and William Norman Rae, V.D., M.A.(Cantab.), Sc.D.(Dub.), F.R.I.C., Professor of Chemistry and Physics, Royal College of Surgeons in Ireland. London: Methuen & Co., Ltd., 1948. (Pp. x + 697, with 1 plate and 310 figures.) 55*s*. net.

Crystals and X-Rays. By K. Lonsdale, D.Sc., F.R.S., Reader in Crystallography in the Chemistry Department of University College, London. London: G. Bell & Sons, Ltd., 1948. (Pp. viii + 199, with 13 plates and 138 figures.) 21*s*. net.

The Theory of Solutions of High Polymers. By A. R. Miller, Imperial Chemical Industries Research Fellow at the Royal Society Mond Laboratory in the University of Cambridge. Oxford: at the Clarendon Press, 1948. (Pp. viii + 118, with 12 figures and 3 tables.) 12*s*. 6*d*. net.

Elastomers and Plastomers. Vol. III: Testing and Analysis; Tabulation of Properties. Edited by R. Houwink, External Lecturer in the Technical University at Delft. Elsevier's Polymer Series. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. (Pp. 174, with 48 figures and 36 tables.) 25*s*. net.

Recent Advances in Organic Chemistry. Vol. II. By Alfred W. Stewart, D.Sc., and Hugh Graham, D.Sc., Reader in Organic Chemistry in the Queen's University of Belfast. Seventh edition. London, New York, Toronto: Longmans, Green & Co., 1948. (Pp. x + 447.) 35*s*. net.

An Introduction to the Chemistry of Carbohydrates. By John Honeyman, Lecturer in Organic Chemistry, University of London, King's College. Oxford: at the Clarendon Press, 1948. (Pp. viii + 143.) 15*s*. net.

Les Diastases. Par Paul Fleury, Professeur, et Jean Courtois, Maître de Conférences, à la Faculté de Pharmacie de l'Université de Paris. Collection Armand Colin No. 251. Paris: Librairie Armand Colin, 1948. (Pp. 216, with 10 figures.) Frs. 150.–.

Fatty Acids and their Derivatives. By A. W. Ralston, Assistant Director of Research, Armour & Co. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. x + 986, with 56 figures and 136 tables.) 60*s*. net.

Waste-Heat Recovery from Industrial Furnaces. A treatise based upon a series of papers presented to the Institute of Fuel. London: Chapman & Hall, Ltd., 1948. (Pp. x + 384, with 112 figures and 61 tables.) 35*s*. net.

Minerals and Mineral Deposits. By W. R. Jones, D.Sc., D.I.C., Pres.I.M.M., F.G.S., and David Williams, Ph.D., M.Sc., B.Eng., M.I.M.M., Sec.G.S. The Home University Library of Modern Knowledge No. 202. London: Oxford University Press, 1948. (Pp. viii + 248, with 56 figures.) 5*s*. net.

A Handbook of Coniferæ, including Ginkgoaceæ. By W. Dallimore, I.S.O., V.M.H., late Keeper of Museums, Royal Botanic Gardens, Kew, and the late A. Bruce Jackson, A.L.S., late of the Dept. of Botany, British Museum (Natural History). Third edition. London: Edward Arnold & Co., 1948. (Pp. xvi + 682, with frontispiece, 39 plates and 120 figures.) 50*s*. net.

Vernalization and Photoperiodism. A Symposium. By A. E. Murneek, R. O. Whyte and others. Lotsya, Vol. 1. Waltham, Mass.: Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1948. (Pp. xvi + 196, with 12 plates and numerous figures.) $4.50.

Factors in Botanical Publication and Other Essays. By Neil E. Stevens, Ph.D., Professor of Plant Pathology, University of Illinois. Chronica Botanica, Vol. II, No. 3. Waltham, Mass.: The Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1947. (Pp. 119–206, with frontispiece). $2.00.

Science in Agriculture. By John W. Paterson, B.Sc., Ph.D., formerly Professor of Agricultural Chemistry in the West of Scotland Agricultural College. Fourth edition. London, New York, Toronto: Longmans, Green & Co., 1949. (Pp. viii + 303, with 181 figures and 29 tables.) 8*s*. 6*d*. net.

Soil Physics. By L. D. Baver, Director, Experiment Station, Hawaiian Sugar Planters Association. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xiv + 398, with 89 figures and 58 tables.) 28*s*. 6*d*. net.

Dictionary of Genetics. Compiled by R. L. Knight, D.Sc., Ph.D., A.I.C.T.A., Senior Economic Geneticist, Empire Cotton Growing Corporation and Sudan Government. Lotsya, Vol. 2. Waltham, Mass.: Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1948. (Pp. xii + 183.) $4.50.

Birds in Britain. By Frances Pitt. London: Macmillan & Co., Ltd., 1948. (Pp. viii + 576, with 17 coloured plates and 350 illustrations in the text.) 25*s*. net.

The Lungfish and the Unicorn. By Willy Ley. London: Hutchinson's Scientific and Technical Publications, 1948. (Pp. 254, with 20 figures.) 10*s*. 6*d*. net.

Trout Fisheries in New Zealand: Their Development and Management. By Derisley F. Hobbs, Senior Fishery Officer (fresh-water) Marine Department. Wellington, N.Z.: New Zealand Marine Department. Fisheries Bulletin No. 9, 1948. (Pp. viii + 175, with 31 plates and 23 tables.)

Organic Form and Related Biological Problems. By S. J. Holmes. U.S.A.: University of California Press; Great Britain and Ireland: Cambridge University Press, 1948. (Pp. viii + 169.) 27*s*. 6*d*. net.

Biological Reactions caused by Electric Currents and by X-rays. By J. Th. van der Werff, M.D., D.Sc. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. (Pp. xii + 203, with 38 figures.) 30*s*. net.

Soviet Biology. By T. D. Lysenko. A Report to the Lenin Academy of Agricultural Sciences, Moscow, 1948. London: Birch Books, Ltd., 1948. (Pp. 51.) 2*s*. 6*d*. net.

Principles of Biological Assay. By C. W. Emmens, D.Sc., Ph.D., National Institute for Medical Research. London: Chapman & Hall, Ltd., 1948. (Pp. xvi + 206, with 25 figures and 66 tables.) 21*s*. net.

The Physiology of the Eye. By Hugh Davson, D.Sc.(Lond.), Honorary Research Associate, University College, London. London: J. & A. Churchill, Ltd., 1949. (Pp. xii + 451, with 301 figures.) 32*s*. net.

Comparative Physiology. By Bradley T. Scheer, Ph.D., Assistant Professor of Biochemistry and Lecturer in Zoology, University of Southern California. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. x + 563, with 72 figures and 23 tables.) 36*s*. net.

Vitamins and Hormones. Vol. V. Edited by Robert S. Harris, Professor of Biochemistry of Nutrition, Massachusetts Institute of Technology, and Kenneth V. Thimann, Professor of Plant Physiology, Harvard University. New York: Academic Press, Inc., 1947. (Pp. xvi + 478, with 75 figures and 22 tables.) $7.50.

Recent Progress in Hormone Research. The Proceedings of the Laurentian Hormone Conference, Vol. III. Edited by Gregory Pincus. New York: Academic Press, Inc., 1948. (Pp. viii + 378, with numerous figures and tables.) $7.80.

Industrial Hygiene and Toxicology. Vol. I. Edited by Frank A. Patty. New York and London: Interscience Publishers, Inc., 1948. (Pp. xxviii + 531, with numerous figures and tables.) 60*s*. net.

Bacteriology. By Fred Wilbur Tanner, Professor of Bacteriology and Head of the Department, University of Illinois, Urbana, and Fred Wilbur Tanner, Jr., Bacteriologist, Fermentation Division, Northern Regional Laboratory, Bureau of Agricultural and Industrial Chemistry, U.S. Department of Agriculture, Peoria, Illinois. Fourth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. x + 625, with 137 figures and 8 tables.) 27*s*. net.

A History of Science and its Relations with Philosophy and Religion. By Sir William Cecil Dampier, Sc.D., F.R.S., Fellow and sometime Senior Tutor of Trinity College, Cambridge. Fourth edition. Cambridge: at the University Press, 1948. (Pp. xxviii + 527, with 18 figures.) 25*s*. net.

Grundzüge der Naturphilosophie. By Moritz Schlick. Edited by Walter Hollitscher and Josef Rauscher. Vienna: Gerold & Co., 1948. (Pp. x + 115.) $1.45.

Gesetz, Kausalität und Wahrscheinlichkeit. By Moritz Schlick. Vienna: Gerold & Co. 1938. (Pp. 116.) $1.45.

Science Outpost. Papers of the Sino-British Science Co-operation Office (British Council Scientific Office in China) 1942–46. Edited by Joseph Needham, F.R.S., and Dorothy Needham, F.R.S. London: The Pilot Press, Ltd., 1948. (Pp. 313, with 26 plates.) 25*s*. net.

Science in Education. By M. C. Nokes, M.C., M.A., B.Sc., Senior Science Master at Harrow School. London: Macdonald & Co. (Publishers), Ltd., 1949. (Pp. xii + 158.) 8*s*. 6*d*. net.

Topographie. Par le Général de Fontangos, Ancien Chef de la Section de Topographie du Service Géographique de l'Armée. Collection Armand Colin No. 252. Paris: Librairie Armand Colin, 1948. (Pp. 224, with 46 figures.) Frs. 150.–.

Statistical Year-Book of the World Power Conference, No. 4. Data on Resources and Annual Statistics for 1936–46. Edited by Frederick Brown, B.Sc.(Econ.), F.S.S., Reader in Commerce in the University of London. London: The Central Office, World Power Conference, 1948. (Pp. 212, with 20 tables.) 45*s*. net.

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SCIENCE PROGRESS

JURASSIC AMMONITES IN 1949

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1. Preliminary Retrospect

In no subject is it more important at the present time that the student should refresh himself at the classics.

It is just a hundred years since the completion of the first thorough scientific work devoted to Jurassic ammonites, F. A. Quenstedt's *Petrefactenkunde Deutschlands; Die Cephalopoden* (1845-49). This is still in many ways the clearest and most accurate morphological account available, just as the same author's *Ammoniten des Schwäbischen Jura* (1883-88), with its three folio volumes of sumptuous plates, is still the most comprehensive and accurate picture-book.

Quenstedt's pupil, A. Oppel, carried the subject beyond the descriptive stage in a classic which laid the foundations of modern zonal stratigraphy (1856–58),* and Oppel's pupil, W. Waagen, first attempted to trace a single evolutionary series or, as it is now called, lineage (Waagen, 1869). Later (1873–75) Waagen went on to do for the Indian Jurassic almost what Quenstedt was doing for the Swabian, describing and figuring the extensive collections of the Geological Survey of India from Cutch, and thus opening up new realms of knowledge.

Meanwhile E. Suess of Vienna, later more famous for his masterpiece *The Face of the Earth* (translated into English and into French), laid the foundations of the present classification of ammonites by separating and naming the two leiostracous or smooth-shelled genera (now families) *Phylloceras* and *Lytoceras* (1865). The fundamental importance of these two persistent stocks in the phylogeny of ammonites, and their special relation to the so-called trachyostraca or rough-shelled, *i.e.* ornamented families, has been emphasised by the progress of research.

Another pupil of Oppel's, M. Neumayr, produced a series of

* Works mentioned by author and date are listed on p. 415.

palæontological and stratigraphical monographs on Jurassic ammonites which for imaginative sweep, grasp of detail, and orderly presentation have never been surpassed. He took for his theme the Jurassic of the whole world as known in the '70's and '80's and with a sure hand marked out the palæogeography and dated the rocks in the most distant lands. One of many brilliant strokes was to date the marine Jurassic of West Australia to the Humphriesianum Zone on the strength of a single ammonite and two fragments: a conclusion since fully confirmed. Neumayr also dealt with such matters as explanations for the astonishingly world-wide distribution of genera of ammonites, the repeated sudden appearance of new types for which no satisfactory ancestors can be found, and the existence of faunal provinces and climatic zones in the Jurassic. His pupil V. Uhlig in turn pursued the same topics further in the light of new knowledge, especially of the Indian and Russian faunas. Like Waagen he was called in by the Geological Survey of India, and he monographed the ammonites of the Spiti Shales and studied the far-reaching problems to which they led. From Russia a great deal of new information drawn from an immense field came through the writings of S. Nikitin, who carried on lively polemics with Neumayr.

Uhlig's great works in the tradition of the Munich and Vienna schools carry us down to 1911. Since then there has been a vast accumulation of knowledge. New discoveries have illuminated large tracts of the earth left blank on Neumayr's maps. It begins to look as if we are in sight of gaining all the data that erosion and the nature of terrestrial geographies, past and present, have left in existence. Next it behoves us to consider what we can make of it all. Shall we ever be able to answer the old questions, let alone the new ones never dreamed of in Neumayr's time?

2. Ammonites and Zonal Stratigraphy

Oppel's pioneer zonal scheme has been greatly elaborated in the course of nearly a century, chiefly by the English amateur S. S. Buckman. Buckman, however, drove subdivision too far and some of his work has had to be done over again. The position now is that, from whatever part of the world Jurassic ammonites may be collected, it should be possible to assign them to one of about 55 zones. That is, the rocks to which they belong can be dated to within about one fifty-fifth part of the Jurassic system. Even in Buckman's day (he died in 1929) any estimate of the length of time involved was pure guesswork. "What is there to prevent giving to a hemera a length of time like a million years?"

he wrote in 1925; and according to his scheme the Jurassic represented 367 hemeræ. (A hemera was Buckman's time-unit, based on the time when a certain index-ammonite flourished and was dominant.)

A reliable, if still only approximate, absolute chronology has now been made available by the radio-active method of dating the rocks, of which the leading exponent is Professor Arthur Holmes. In Holmes' latest, revised time-scale, issued April 1947, the Jurassic period is assigned 25 million years, from about 127 to 152 million years ago. According to this the formation of each zone occupied on the average approximately half a million years. The Lower Lias, for which some 62 hemeræ have been determined, comprises 11 zones and so occupied in formation something like 5½ million years, or on the average about 88,000 years per hemera.

The stratigraphical work of determining the zonal sequence, and the descriptive-palæontological work of defining the ammonite content of each zone everywhere, together form the basis of all advances in knowledge of the Jurassic. Neither task is yet anything like complete. In the present century the impetus given to descriptive work by new discoveries in distant parts of the world, often backed by funds from exploration expeditions or colonial governments, has led to unbalance in our knowledge almost as great as that of the early days. Neumayr was handicapped in his inductions by the restriction of his detailed knowledge to Europe. Nowadays Europe in turn lags far behind. Detailed and up-to-date monographs are available on India, East Africa, Madagascar, the Malay Archipelago, South and Central America, Canada, Greenland, and parts of the Siberian Arctic, while for figures of some of the most important English ammonites we have to go back to Sowerby's *Mineral Conchology* of the first quarter of the nineteenth century. One good result of this eventually will be that when in time the faunas of the old classic European countries come to be reworked (as they are being, slowly), we shall profit by a wealth of comparative material from many parts of the world and a more complete picture will be forthcoming.

Apart from the fascination of the subject for its own sake, gradual perfection of a world stratigraphical standard or zonal scheme with detailed knowledge of the ammonite-content of each zone may be expected to lead to advances in three main directions:

(1) In the first place it affords the only means of discovering what the ammonites have to tell about the workings of evolution; for until it is certain what forms were evolved and in what order and where, speculations are vain.

(2) Secondly, it provides the only means of reconstructing with any approach to probability the geographies of the past: climatology, palæoecology, and such fundamental questions to geology as the nature, distribution and evolution of geosynclines. A separate picture must be constructed for each zone, otherwise we obtain only a confused and misleading result.

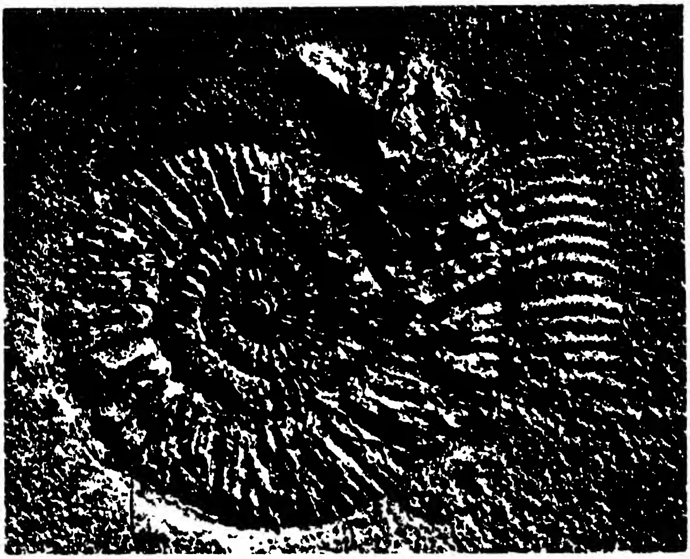
(3) Thirdly, it should provide a means of testing the hypothesis of Continental Drift.

3. Ammonites and Evolution

That so remarkable an extinct order, unique in diversity of characters within strictly defined and comparatively narrow limits, has much light to throw on the workings of evolution cannot be doubted. Shells that roll up within themselves their early ontogenetic stages and continually ring the changes on a limited number of easily appreciable characters must be valuable material for the evolutionist.

Yet owing to preconceptions and above all mistakes in stratigraphical sequence the evidence of the ammonites has been until recently misinterpreted and misunderstood. It happened that the first to publish theories of ammonite evolution, Alpheus Hyatt, was an inhabitant of North America, a sub-continent almost devoid of ammonite-bearing Jurassic rocks. Consequently Hyatt's theories were based on museum material brought over from Europe, reinforced by visits to European museums. On this basis Hyatt expounded the Law of Morphogenesis or Recapitulation (Haeckel's Biogenetic Law), according to which the life of the individual repeated in miniature the history of the race. He found that the inner whorls of some Liassic ammonites resembled the complete adult of others. Therefore new characters appeared on the outer whorls and gradually "pushed back" the older stages further in each generation. If satisfactory lineages could not be traced without omitting certain stages, it was assumed that the missing stages had been "skipped." In this way an impressive scheme was built up, enshrined in suitable jargon. For nearly half a century the universities lectured and examined on "tachygenesis," "bradygenesis," "lipopalingenesis," and a rigmarole of other "geneses," as set out in Hyatt's *Genesis of the Arietidae* and the works of his followers. Buckman swallowed the cult whole and it enabled him to pronounce with comfortable certainty on the course of evolution of the ammonite groups with which he dealt.

In 1901 A. P. Pavlow published the observation that in the genera *Kepplerites*, *Kosmoceras*, *Amœboceras* and *Simbirskites* it is



(*a*) A Perisphinctid in the Solnhofen Slate with impression of venter beside it
(*After* ROTHPLETZ.)

(*b*) Marston Marble, from the Lower Lias of Somerset, composed of *Promicroceras marstonense*. The population was apparently all killed at once and comprises five successive broods. (*Photo.* ALBERT BARLOW.)

the young ammonites that first show new characters of ribbing and whorl-shape, which only in later zones spread on to the adults. "The frequent repetition of this phenomenon in different groups of ammonites proves how important it is for ammonite phylogeny. We shall call it *phylogenic acceleration* or *precession of characters*." Because the inner whorls foretell the characters of the racial descendants he called them also the "prophetic phase," and he observed that this phase shows itself in Hyatt's neanic stage of development. Pavlow also pointed to instances of the same phenomenon among the belemnites, gastropods, and vertebrates, and concluded: "It is to be hoped that, under the influence of the facts, the limitations of the recapitulation hypothesis will soon be realised and that outside those limits the field will be left free for other interpretations." Nearly a quarter of a century later the prophetic phase was rediscovered in the west by Schindewolf (1925, 1929, 1936), using some of the same instances as Pavlow. Schindewolf coined the term proterogenesis for the gradual spreading to the adult in successive generations of a new character first appearing in the young (*i.e.* originating by cœnogenesis).

Allowing for the obvious fact that in the earliest (nepionic) stage all animals must be simple and so far repeat their simpler ancestors, the truth in many cases and the far-reaching importance of Pavlow's observations are now generally recognised, though the credit is never given him by non-Russian authors. Freed from the toils of the Recapitulation theory, research has made a new start in unravelling the pattern of evolution of the "pageant of ammonites". Thus a monograph on the Lower Lias family Liparoceratidæ by L. F. Spath (1938) completely reverses the previously accepted evolutionary order of these ammonites and concludes that Hyatt's *Genesis of the Arietidæ* was one huge fallacy. It must not be overlooked, however, that some new characters first appear on outer whorls, and some characters, such as uncoiling, are physically incapable of originating proterogenetically.

There have been palæontologists, among them no less an authority than G. Steinmann, who have held that many Jurassic families, even such as the Oppelidæ, which first appear in the Middle Jurassic, are descended from different Triassic stocks. Similarly, within the Jurassic, until recently it was generally held that all lineages were to be represented as running vertically from one similar form to another: oxycones were all filiated, *Oxynoticeras* being supposed to give rise to *Amaltheus*, and that to *Cardioceras*, and so on; or *Dactylioceras* to *Stephanoceras* and that to *Perisphinctes*. Even now the results of homœomorphy, although

realised, are often still baffling. Pompeckj's thesis (1895) is now generally accepted, however, that at the transition between Triassic and Jurassic the ammonoidea passed through a critical phase, in which all suffered extinction except the single family Phylloceratidæ, from which the whole of the Jurassic and Cretaceous ammonoidea must be descended.

Since there is already in the Lower Lias a considerable variety of forms, the viable Phylloceratidæ must have undergone rapid evolutionary radiation, for which the way was no doubt made clear by the extinction of all competitors. At the same time *Phylloceras* itself persisted with little change into the Cretaceous.

Side by side with Phylloceratidæ there persisted the other stock named by Suess in 1865, the Lytoceratidæ, which was descended from some late Triassic Phylloceratid, probably *Monophyllites*, and continued with only minor changes until the end of the Cretaceous. It was by way of early Lytoceratidæ that the earliest Liassic trachyostracous ammonites (*Psiloceras*) were evolved, and through *Psiloceras* in turn probably all the main Lower Liassic genera.

Using this history of the beginnings of Jurassic ammonites as a key to the rest of the story, H. Salfeld (1922 and 1924) announced the theory of Iterative or Repetitive Evolution. According to this theory all the innumerable throng of ammonites arose through repeated radiation of offshoots from the conservative stocks, mainly the Lytoceratidæ. "So it is certain that of all the families erected for the Jurassic and Cretaceous ammonites nothing remains."

Pompeckj, Nikitin, and others have maintained that the leiostraca or conservative fundamental families had a different mode of life and different habitat from other ammonites, probably being free swimmers in the open ocean. This might account for their surviving the general extinction at the end of the Trias and another period almost as fatal in the Barremian: conditions of life in the open ocean are more changeless than anywhere else on the earth. As Neumayr had shown, leiostraca periodically migrated from the Mediterranean region, where they always abounded, into the epicontinental seas of northern Europe. Salfeld held that the changes of environment encountered during these migrations, and consequent adaptations to different modes of life, might have given the spur to the repeated evolutionary radiations. Meanwhile the products of previous migrations and their transformed descendants lived on and continued to evolve in the epicontinental seas. In this way was produced the incredibly complex tangle of forms.

How far have modern researches borne out Salfeld's theory?

Perhaps the most convincing case of direct evolution of a family

from *Lytoceras* is that of the Upper Liassic Dactylioceratidæ, by way of "*Ammonites*" *davœi* of the uppermost Lower Lias. It is significant that Salfeld's pupil, H. Frebold, who propounded this case, called *Am. davœi* "*Paralytoceras*" (1922, a preoccupied generic name), and that L. F. Spath (1923) called it "*Prodactylioceras*". It is wellnigh perfectly intermediate in characters between *Lytoceras* and *Dactylioceras.*

Palæontologists have long accepted that in the Lower and again in the Upper Cretaceous the Lytoceratidæ underwent sudden modification in mode of coiling to produce the aberrant Macroscaphitidæ and Scaphitidæ, half uncoiled. Whether these changes denoted racial senescence as held by the Hyatt school, or virile adaptability to fresh environment and modes of life as held by Diener (1912, 1922), may be debatable, but it seems that the coiled conservative main stock of the Lytoceratidæ lasted as long as all its aberrant offshoots, and longer than most of them.

In the Jurassic there is a family of small, smooth, simple, rather nondescript forms, the Haploceratidæ, which appear in the Bajocian and persist to the end of the Jurassic. As long ago as 1884, H. Douvillé suggested that these forms were the ancestors of the Oppelidæ. It is even conceivable that Oppelids were derived more than once from Haploceratids. The Haploceratids remained, however, almost unchanged and increased in numbers to reach their acme in the uppermost Jurassic.

In the Cretaceous the place of the Haploceratidæ is taken by a more heterogeneous succession of similarly smooth and nondescript forms, grouped together for convenience as the family Desmoceratidæ. According to the theory of repetitive evolution, however, the Desmoceratidæ are not a true family but a succession of offshots from the Phylloceratidæ or Lytoceratidæ, endowed with a family resemblance because at a comparable stage of transition from a common ancestral stock to quite different end forms. Thus L. F. Spath has gone further than Salfeld and visualises the Hoplitidæ of the Gault as composed of at least seven lineages given off at successive periods via some Desmoceratid intermediary.

The consequences of the theory of iterative evolution to classification have been disastrous: a "pulvérisation à outrance" (Roman, 1938, p. 3) which bids fair to defeat most of the objects of nomenclature. Roman's systematic encyclopædia (1938) is a corrective and an indispensable reference work, though the serious student must check all its clues and form his own judgments. On the whole it must be said that in the Jurassic there are not nearly

enough transitional forms to prove the theory so generally valid as has been claimed by its enthusiastic advocates.

4. Mode of Life, and Sidelights on Palæogeography

After Willey and Bashford Dean at the beginning of this century described the anatomy and creeping mode of life of the pearly nautilus, it was generally assumed that the ammonites were bottom crawlers which seldom came to the surface voluntarily. Diener has pointed out, however (1912 and 1922), that the delicate hydrostatic apparatus constituted by the shell of the ammonites and nautilids would be useless to animals of predominantly crawling habits and would never have been evolved or, once evolved, retained for such a long period. He inferred, therefore, that the crawling of the living *Nautilus* is a habit contracted recently, but that the great majority of the ammonites had quite a different mode of life. Their shells are thinner and lighter than those of *Nautilus*; some equal *Lytoceras* and *Phylloceras* in being thin as paper and clear as glass, like modern nectonic gastropods; some, especially the oxycones, seem to be streamlined for rapid swimming; others have spines which probably aided in balancing; and most have ribs which represent strengthening to resist water pressure.

This matter of water pressure is important for the light that ammonites may throw on sea depths in past ages. If Willey (1902) and Bruun (1943) are right in thinking that the so-called air or gas chambers of *Nautilus* and *Spirula* always do contain gas (mainly nitrogen) and that its pressure cannot be appreciably varied by the animal, its purpose being merely "to render the shell buoyant once for all", it follows that ammonites cannot have lived at great depths or their shells would have been crushed. The pressure exerted by sea-water amounts to about a ton to the square inch at 1000 fathoms. It is remarkable enough that the nautilus is found occasionally down to depths of over 300 fathoms, at which the pressure is something like 800 lb. per square inch, as compared with 15 lb. at the surface. The optimum depth is said to be 100 to 200 fathoms, but accounts vary widely, and of course dead shells filled with water could sink to the bottom at any depth without injury. Bruun (1943) has made experiments with *Spirula* and concludes that the maximum depth physically possible without crushing of the shells is 1750 metres (966 fathoms), and that the animals normally live in the waters of the continental slope. (*Spirula* is much smaller than *Nautilus* and correspondingly resistant.)

On the other hand, the many authorities who have discussed

this matter seem to visualise extensive changes of pressure in the gas chambers as involving prodigious feats of pumping on the part of the animal. In reality, provided gas can pass at least slowly through the siphuncle, adjustment of pressure would be automatic. Since the fluids in the animal's body must be at the same pressure as the surrounding water, any gas generated at depth would be sucked into the shell almost as by a vacuum, until it reached approximately the same pressure as that common to the animal and its environment. The process would require no effort on the part of the animal beyond passive generation of gas.

If then ammonites were able to equalise the air pressure inside the cameræ to the pressure of the surrounding sea-water, the elaboration of the septal edges finds a ready explanation : it would have provided the perfect means of increasing the attachment of the septa to the shell walls to prevent bursting when the animal swam suddenly upwards. On this hypothesis simplification of suture lines could mean change of habitat to shallower waters, or sluggishness.

Uhlig (1910) objected to the notion that the majority of ammonites habitually crawled on the bottom, because their tracks are never found fossilised, as are those of many much rarer animals. Although F. Trusheim (1934) has figured peculiar tracks from the Solnhofen Slates which he attributes to ammonites, these are only the exception that proves the rule ; in normal deposits they are unknown.

Much light has been thrown on this matter by an entirely new line of investigation by A. E. Trueman (1941). By calculating the positions for the centre of gravity and centre of buoyancy in a number of genera, Trueman is able to show the attitude which the shell must have adopted when the animal was at rest. He finds that in the evolute, sepenticone, shells (such as *Dactylioceras*, *Skirroceras*, and many *Perisphinctes*) the centres of gravity and buoyancy are so close together that the animal could easily have changed its position considerably, though the normal position of rest would be with the aperture facing half upwards (*see* Figs. 1 and 2). The involute forms, however, must have been much more stable, and in them the aperture always faced almost completely upwards, as in the living *Nautilus*. The investigation shows that most ammonites probably did not habitually crawl, that some fairly easily could, and that others could only by holding on to the bottom. In the uncoiled forms, such as the Scaphitids, Trueman shows that the buoyant effect of the air chambers would have made crawling almost impossible. Probably, as Uhlig supposed, most ammonites

hovered and soared through the water not far above the bottom, resting near it without touching it, as does *Nautilus*.

Trueman's calculations are confirmed by impressions of Perisphinctids on the Solnhofen Slates. A. Rothpletz (1909) showed

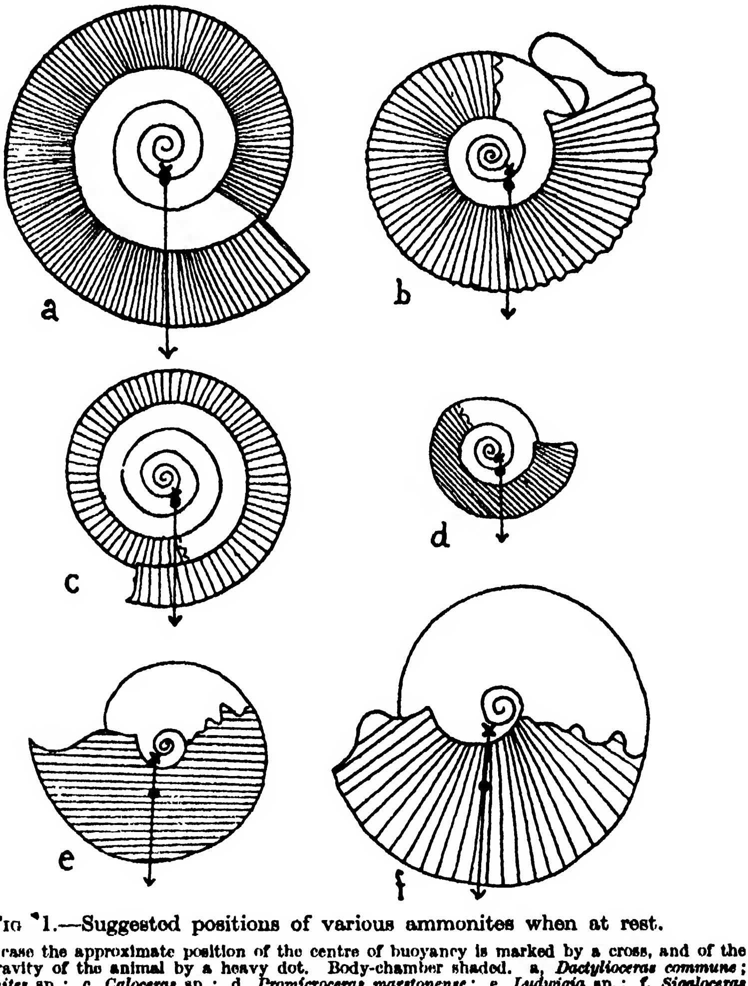


FIG. 1.—Suggested positions of various ammonites when at rest.

In each case the approximate position of the centre of buoyancy is marked by a cross, and of the centre of gravity of the animal by a heavy dot. Body-chamber shaded. a, *Dactylioceras commune*; b, *Normannites* sp.; c, *Caloceras* sp.; d, *Promicroceras marstonense*; e, *Ludwigia* sp.; f, *Sigaloceras micans*. e and f show marked stability in the attitudes illustrated, while a, b, and c are likely to have been able to adopt other positions. (*After* TRUEMAN.)

(*Reproduced by permission from "Quart. Journ. Geol. Soc.", XCVI, 1941, 372.*)

that some of the perisphinctids which lie on the bedding-planes at Solnhofen have beside them an impression of part of the venter (Plate I*a*). He pointed out that the only possible interpretation of these is that the ammonite floated in shallow water, presumably on a receding tide, with its spiral plane vertical, that it first touched

the bottom with its venter, and as the water continued to shallow the shell fell over on its side and was eventually left high and dry. In every case the impression of part of the venter is just where it should be according to Trueman's figures *a* and *c*. The impres-

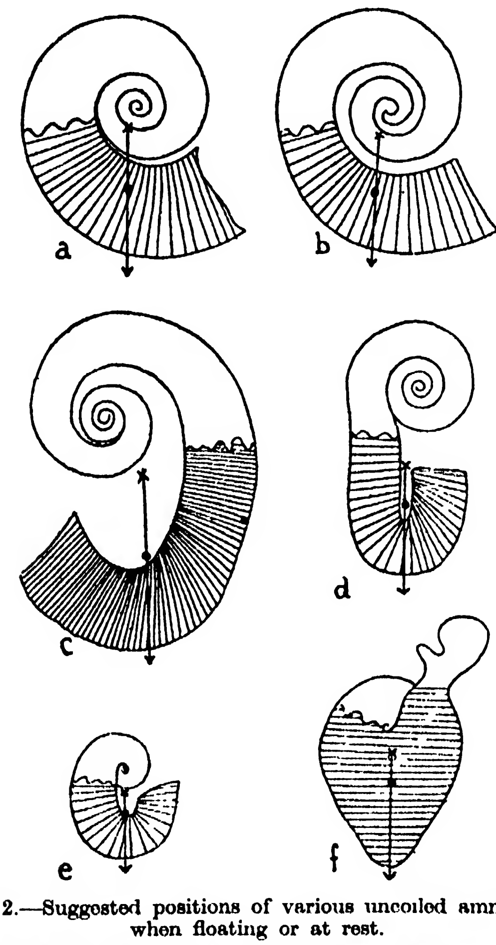


FIG. 2.—Suggested positions of various uncoiled ammonites when floating or at rest.

Approximate positions of centres of buoyancy and gravity are marked as in Fig. 1. a, *Crioceras fissicostatum*; b, *Crioceras duvali*; c, *Crioceras mulsanti*; d, *Macroschaphites ivanii*; e, *Scaphites æqualis*; f, *Œcoptychius* sp. (*After* TRUEMAN.)

(*Reproduced by permission from "Quart. Journ. Geol. Soc.", XCVI, 1941, 373.*)

sion on the mud is so clear that if any of the animal had protruded from the shell it too must have left impressions. The inference is that the animals were completely retracted within their shells, perhaps dead.

Some rays of light have recently illuminated also the reproductive processes and rate of growth of ammonites. By measuring

large numbers of the ammonites massed together in the Marston Marble, of the Somerset Lower Lias (Plate I*b*), Trueman (1941) found that the material fell into five size-groups. He interprets this as showing that all the ammonites were killed at once, and that five successive broods are represented. Whether the size-groups represent seasonal or more frequent broods is unknown. The supposed unborn brood found in the body chamber of an Oppelid (*Streblites steraspis*) from Solnhofen has been reinterpreted by M. Schwarzbach (1936) as the masticated remains of aptychi of smaller ammonites eaten cannibalistically.

The problem of the rate of growth has been attacked from another angle by Schindewolf (1934). He found that certain Lower Lias ammonites are completely encircled by a single large *Serpula* tube, which must have grown with the ammonites, in some kind of symbiotic association. From a number of the most closely related living Serpulids he estimates that the rate of growth of the ammonites is of the order of one whorl in ⅓ to 3 years. Those interested in the mathematics of the growth of the ammonite spiral and the changes it undergoes in all its parts should read the two papers by Ethel D. Currie (1942, 1944).

5. Ammonites and Earth Movements, Geosynclines, and Continental Drift

That the discontinuities in all ammonite lineages, the migrations from the Tethys into northern Europe, the sporadic appearance of new "cryptogenic" migrants (as Neumayr called them), are all related to tectonic movements and bound up with cyclic sedimentation cannot be doubted. The ammonites were more sensitive to these earth pulsations than other invertebrates. At least twice in their long history they were nearly extinguished by some changes of apparently world-wide extent, at the end of the Trias and in the Barremian. Complete extinction was only avoided on these occasions by survival of the persistent leiostraca and some of their immediate offshoots; at the last crisis, at the end of the Senonian, these hardy stocks also succumbed.

The final disaster, therefore, may have been due to coincidence of two distinct factors. The extinction of the leiostraca and "heteromorphs" in the open ocean might have been brought about by enemies, perhaps giant cuttlefish or sharks, or rays, such as *Ptychodus*. The general wiping out of the trachyostraca, however, had happened before. It is reasonable to connect it in each case with reduction of favourable habitat by shrinkage and stagnation of the epicontinental seas, with disappearance or diminution

of those conditions of recurring transgression under the stimulus of which the ammonites so greatly flourished, spread, and evolved.

Application of Oppel's zonal methods to the Mesozoic strata of the Alpine geosyncline proved that the sedimentary record is just as discontinuous and changeable there as in the ordinary epicontinental or shelf seas elsewhere. The Alpine region can show cyclic sedimentation, non-sequences, and condensed deposits (for instance the famous metre-thick Aalenian and Lower-Middle Bajocian marble of Cape St. Vigilio on Lake Garda), just like those in the Jurassic elsewhere. Detailed analysis, as Deecke (1912) clearly showed, reveals only one distinguishing feature of importance, and that is greater mobility in the geosynclinal tracts. The Alpine region was one of ever-shifting shallows, islands, troughs, and deeps. Extension of the same methods of faunal analysis by means of ammonites to other geosynclinal tracts, as by Renz in the Caucasus, and Uhlig and lately Spath (1934) in the Himalayas, has produced abundant confirmation. The oft-quoted "geosynclinal" Spiti Shales of the Himalaya are similar to, but only one-third as thick as, and much less homogeneous than the Kimeridge Clay of Kimeridge, and their lithological and palæontological connections with the underlying Jurassics show much less continuity of subsidence than the contemporary deposits on the Dorset coast.

Here an understanding of the ammonites has provided an instrument to clear away entrenched misunderstanding about the nature of geosynclines,—features which have been called "a fundamental concept in geology."

Finally, the world-wide distribution of ammonite genera makes them a useful means of investigating the past distribution of seas and continents. By following chosen zones or small groups of zones all over the world and plotting their occurrences on a map (*e.g.* Fig. 3) we obtain a picture, not of the distribution of former oceans, but of the relatively small areas of epicontinental floodings over tracts now land. Nevertheless, the extent of habitat of, for instance *Dactylioceras*, *Stephanoceras* (Fig. 3), *Macrocephalites*, *Perisphinctes*, and other genera, is astonishing. Such world-wide distribution seems to demand a more compact arrangement of the land-masses in the Jurassic.

According to Wegener and Du Toit the Jurassic was the last geological epoch before the break-up of the two great adjacent continental masses, Laurasia and Gondwana. Between them stretched the broad and on the whole shallow mobile belt of Tethys, open at both ends to the ocean. If we re-plot the ammonite occurrences for each zone on maps of the world as re-arranged by the

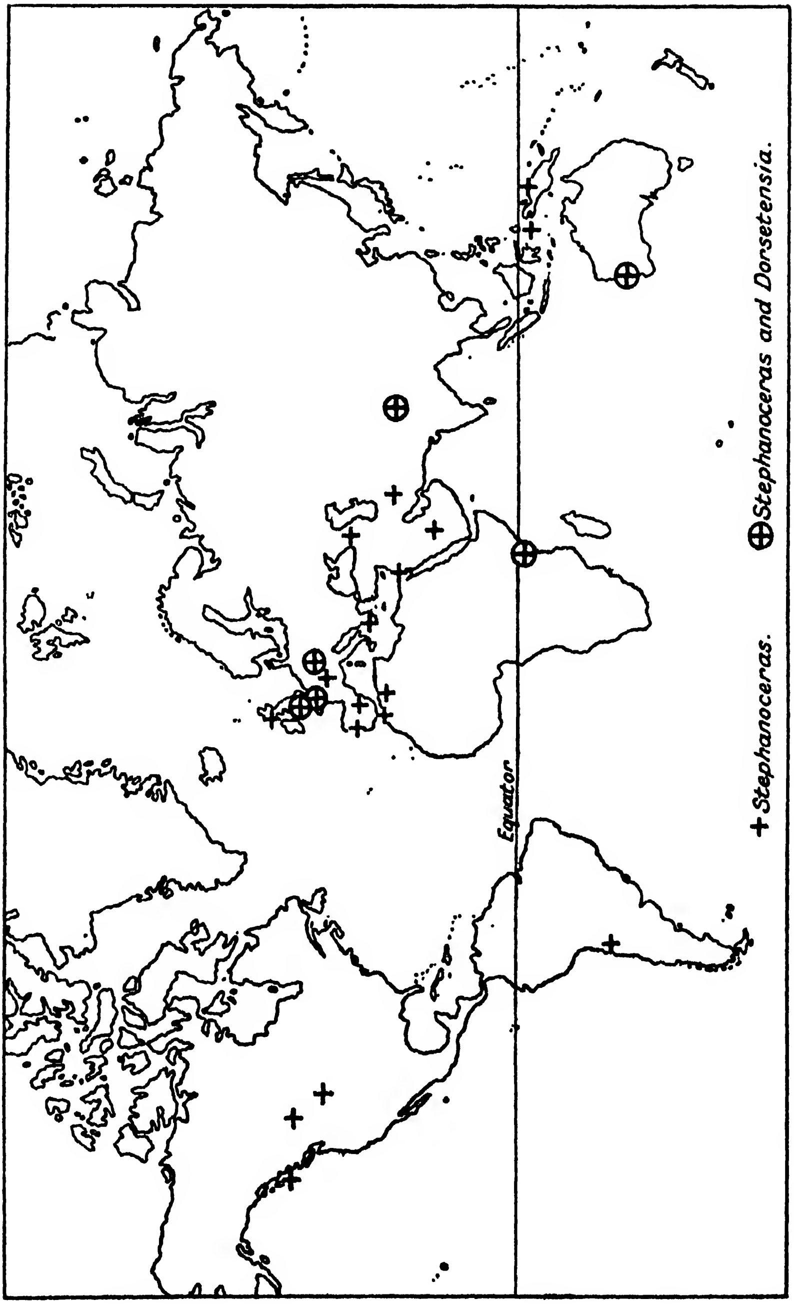


FIG. 3.—Map on Mercator's Projection showing distribution of two contemporary unrelated ammonite genera in the Middle Bajocian (Humphriesianum Zone).

advocates of continental drift (Fig. 4), there emerge striking simplifications of particular problems, such as the close affinity between the Bajocian ammonites of Europe and the western cordillera of South America. Moreover, in general it stands out that the land-masses and deep ocean as so arranged offer far less obstructions to migration of marine organisms inhabiting the epicontinental seas, and the global distribution of so many ammonite genera becomes more understandable.

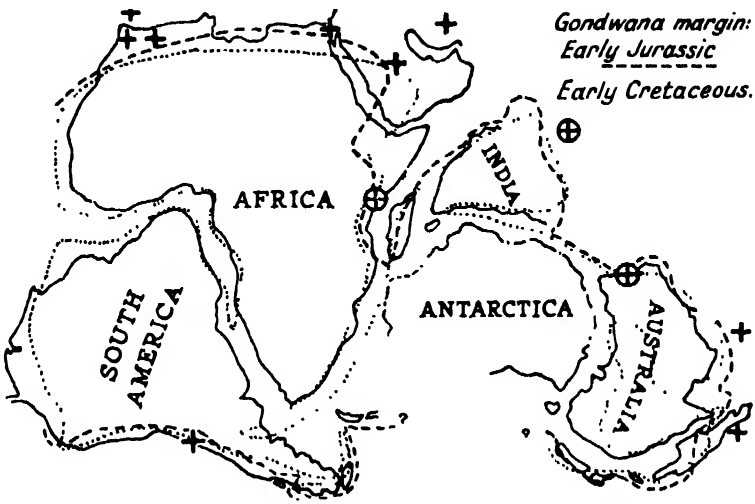


FIG. 4—Occurrences of Stephanoceras and Dorsetensia (*cf.* Fig 3) plotted on Du Toit's map of Gondwana.

Margins of Gondwana in Early Jurassic and Early Cretaceous after Du Toit (modified on the east coast of India).

REFERENCES

BRUUN, A. F. (1943), "The biology of *Spirula spirula* (L)," *Dana Report 24.*

BUCKMAN, S. S. (1909–30), *Type Ammonites*, Vols. I–VII (London).

CURRIE, E. D. (1942), "Growth stages in the ammonite *Promicroceras marstonense* Spath," *Proc. Roy. Soc. Edin.* (Sect. B), LXI, 344.

—— (1944), "Growth stages in some Jurassic ammonites," *Trans. Roy. Soc. Edin.*, LXI, 171.

DEAN, BASHFORD (1901), "Notes on the living Nautilus," *Amer. Naturalist*, XXXV, 819.

DEECKE, W. (1912), "Die Alpine Geosynklinale," *Neues Jahrb. für Min.*, Beil.-Bd. XXXIII, 831.

DIENER, C. (1912), "Lebensweise und Verbreitung der Ammoniten," *Neues Jahrb. für Min.*, 1912, ii, 67.

—— (1922), "A critical phase in the history of ammonites," *Amer. Journ. Sci.*, (N.S.), IV, 120.

DOUVILLÉ, H. (1884), "Sur quelques fossiles de la zone à Amm. sowerbyi des environs de Toulon," *Bull. Soc. géol. France*, (3), XIII, 12.

Du Toit, A. L. (1937), *Our Wandering Continents*, (London).

Frebold, H. (1922), "Phylogenie und Biostratigraphie der Amaltheen im mittleren Lias von Nordwestdeutschland," *Jahresber. Niedersächs. geol. Vereins*, (Hannover).

Hyatt, A. (1889), "Genesis of the Arietidæ," *Smithsonian Contributions to Knowledge*, No. 673, (Washington).

Neumayr, M. (1878), "Über unvermittelt auftretende Cephalopodentypen im Jura Mittel-Europas," *Jahrb. k.k. Geol. Reichsanst., Wien*, XXVIII, 37.

—— (1885), "Die geographische Verbreitung der Juraformation," *Denkschr. k. Akad. Wiss. Wien*, I, 57.

Nikitin, S. (1886), "Über die Beziehungen zwischen der russischen und der westeuropäischen Juraformation," *Neues Jahrb. fur Min.*, 1886, ii, 205. (Neumayr's reply, 1887, i, 70.)

Oppel, A. (1856–58), "Die Juraformation Englands, Frankreichs und des sudwestlichen Deutschlands," *Wurttemb. naturwissen. Jahresh.*, XII–XIV, (Stuttgart).

Pavlow, A. P. (1901), "Le Crétacé inférieur de la Russie, et sa faune," *Mém. Soc. Nat. Moscou*, XVI.

Pompeckj, J. F. (1895), "Die Ammoniten des Rhät," *Neues Jahrb. fur Min.*, 1895, ii, 1.

Quenstedt, F. A. (1845–49), *Petrefactenkunde Deutschlands*, Part 1: *Die Cephalopoden*, (Tubingen).

—— (1883–88), *Die Ammoniten des Schwäbischen Jura*, 3 vols., + 3 vols. Atlas (Stuttgart).

Roman, F. (1938), *Les ammonites jurassiques et crétacées*, (Paris).

Rothpletz, A. (1909), "Über die Einbettung von Ammoniten in die Solnhofener Schichten," *Abh. kgl. Bayer. Akad. Wiss. Munchen* (2 kl.), XXIV, 313.

Salfeld, H. (1922), "Formänderung und Vererbung bei fossilen Evertebraten," *Pal. Zeitschr.* IV, 107.

—— (1924), *Die Bedeutung der Konservativstämme fur die Stammesentwicklung der Ammonoideen*, (Leipzig).

Schindewolf, O. H (1925), "Entwurf einer Systematik der Perisphincten," *Neues Jahrb. fur Min.*, Beil.-Bd., LII, 309.

—— (1929), "Ontogenie und Phylogenie," *Pal. Zeitschr.*, XI, 54.

—— (1934), "Über Epöken auf Cephalopoden-Gehäusen," *Pal. Zeitschr.* XVI, 15.

—— (1936), *Paläontologie, Entwicklungslehre und Genetik*, (Borntraeger, Berlin).

Schwarzbach, M. (1936), "Zur Lebensweise der Ammoniten," *Natur und Volk*, LXVI, 8.

Spath, L.F. (1923–43), "A monograph of the Ammonoidea of the Gault" (Pal. Soc., London).

—— (1934), "The Jurassic and Cretaceous ammonites and belemnites of the Attock district," *Pal. Indica*, (N.S.), XX, Mem. No. 4.

—— (1938), "The ammonites of the Liassic family Liparoceratidae," *Cat. Brit. Mus. (Nat. Hist)*. (London).

Steinmann, G. (1927), "Die Methoden der Ammoniten-Phylogenie," *Pal. Zeitschr.*, IX, 187.

Suess, E. (1865–70), "Über Ammoniten," *Sitzungsb. k. Akad. Wiss. Wien*, LII, (30 June 1865); Part 2, *ibid.* (March 1870).

TRUEMAN, A. E. (1941), "The ammonite body-chamber, with special reference to the buoyancy and mode of life of the living ammonite," *Quart. Journ. Geol. Soc.*, XCVI, 339.

TRUSHEIM, F. (1934), "Eine neue Lebensspur aus den lithographischen Schiefern Süddeutschlands," *Pal. Zeitschr.*, XVI, 1.

UHLIG, V. (1903–10), "The Fauna of the Spiti Shales," *Pal. Indica*, (XV) IV, Fascs. 1–3.

—— (1910), "Die Fauna der Spiti-Schiefer des Himalaya, ihr geologisches Alter und ihre Weltstellung," *Denkschr. M.-N. Klasse k. Akad. Wiss. Wien*, LXXXV, 1.

—— (1911), "Die marinen Reiche des Jura und der Unterkreide," *Mitt. Geol. Gesellsch. Wien*, IV, 329.

WAAGEN, W. (1869), "Die Formenreihe des Ammonites subradiatus," *Geognostisch.-Palaont. Beiträge*, II, Heft 2, 179.

—— (1873–75), "Jurassic Fauna of Kutch. The Cephalopoda," *Pal. Indica*, I, Pts. 1–4. (Revised by L. F. Spath, 1927–33, *Pal. Indica*, (N.S.), IX, Mem. 2).

WILLEY, A. (1902), "Contribution to the natural history of the pearly nautilus," *Zool. Results Materials New Britain, New Guinea, Loyalty Islands, etc.*, VI, 691 (Cambridge Univ. Press).

THE CHEMICAL AND BIOLOGICAL PROPERTIES OF ACRIDINES

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1. Introduction

When Graebe and Caro (1871) were examining the higher-boiling distillates of coal-tar, one particular fraction was obtained which discomforted the investigators by causing sneezing and making the lips and eyes smart. Graebe gave the name "acridine" to the constituent that caused these effects and soon succeeded in isolating it in a pure crystalline state.

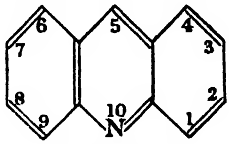
It was later found that the amino-derivatives of acridine were not irritating and several attempts were made in the nineteenth century to find uses for them in the treatment of disease. Although these endeavours were not successful, an amino-derivative of 5-phenylacridine, known as chrysaniline or phosphine, was introduced as a synthetic dye in 1862, actually long before its structure was known. Once the constitution of chrysaniline was established (in 1884) the search for other worth-while dyes in the acridine series began and two very useful series of commercial dyestuffs were found (see section 6). The first useful acridine drugs were the antibacterials (Browning & Gilmour, 1913) and these were followed by the antimalarials (Mauss & Mietzsch, 1933).

In addition, numbers of miscellaneous uses for acridines have become established. Although the acridines do not represent a large fraction of the output of the fine chemicals industry, they have made and are still making important theoretical and practical contributions to both science and technology.

2. The Chemical Nature of Acridine

Acridine (I) is a three-ring heterocyclic substance, in which a central pyridine ring is flanked by two benzene rings. The system of numbering given in (I) is used throughout the British Empire, but alternative systems are used in some foreign countries. The molecule is conjugated throughout, the resonance energy being

106 Kgm. cal./mole (Albert & Willis, 1946). It is a perfectly flat molecule, about 11 Å long, 7 Å wide and presumably, like benzene, 3·7 Å in thickness. In all these properties acridine closely resembles anthracene, and the spectra of these two substances are almost identical (Radulescu & Ostrogovitch, 1931).



Acridine

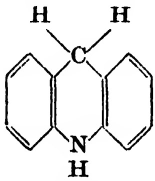
(I)

Acridine crystallises from about 5 parts of alcohol or benzene, on cooling, in the form of very pale yellow crystals melting slightly below 111° C. It is soluble in about 20,000 parts of cold water and somewhat volatile in steam. Acridine is a moderately weak base, of about the same strength as aniline, pyridine and quinoline. The salts with common acids are freely soluble in water. The salts of acridine are more yellow than acridine itself because the long-wave absorption peak of the latter spreads out further, from the ultra-violet into the visible, upon ionisation. Aromatic amino-groups behave in the opposite manner when ionised and this knowledge has been used to prove spectrographically that the aminoacridines form their (mono-) salts by adding a proton to the ring-nitrogen and not to the $-NH_2$ groups (Turnbull, 1945; Craig & Short, 1945). Solutions of acridine salts show a green fluorescence when moderately dilute; but, when diluted further, hydrolysis occurs and the violet fluorescence of free acridine is seen.

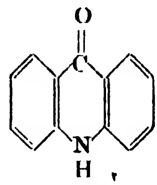
Acridine is a stable chemical which has resisted all attempts to sulphonate it. It is recovered unchanged when heated to 280° C. with either potassium hydroxide or concentrated hydrochloric acid. However, it can be nitrated, below 100°, to give a mixture of mono- and di-nitroacridines and -nitroacridones which are difficult to separate, 3-nitroacridine being the principal product. The direct halogenation of acridine leads merely to the addition of two atoms of the halogen in the 5- and 10- positions. Such products, *e.g.* 5:10-dibromoacridan, revert to acridine when allowed to stand in the air. Hydrocyanic acid similarly gives 5-cyanoacridan and sodium bisulphite gives the sodium salt of acridan-5-sulphonic acid (Lehmstedt & Wirth, 1928).

Acridine is not easily oxidised. The best-known method for converting it to acridone (5-hydroxyacridine, III) is by heating it

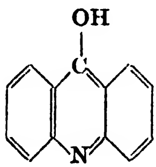
with sulphur in a sealed tube at 190°. This gives thioacridone which can be hydrolysed to acridone by stirring with sodium hypochlorite (Edinger & Arnold, 1901). Acridine is quite easily reduced to acridan (5:10-dihydroacridine, II) by catalytic hydrogenation over Raney-nickel at atmospheric temperature and pressure (Albert & Ritchie, 1943). Alternatively, reduction with sodium amalgam or with zinc and hydrochloric acid may be used (Bernthsen & Bender, 1883). Hydrogenation under increasing pressure leads to a mixture of polyhydroacridines until finally, at 240° and 275 atmospheres, in the presence of Raney-nickel, an 80 per cent. yield of the completely hydrogenated product, tetradecahydroacridine, is produced (Adkins & Coonradt, 1941). These higher hydrogenated products resemble aliphatic amines and have not proved of particular interest.



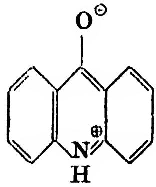
Acridan
(II)



Acridone
(III, *a*)



(III, *b*)



(III, *c*)

3. The Three Levels of Oxidation

In preparative chemistry, which is a very interesting topic but beyond the scope of this article, three common levels of oxidation are often encountered in the acridine series. These levels are respectively the acridan (II), and true acridine (I) and the acridone (III) levels. In choosing a preparative method, the oxidative level at which it yields its product is usually a matter of indifference because of the excellent procedures which now exist for passing from one level to another. These are fully dealt with in a forthcoming publication and, being technical, need not be enlarged upon here. (Albert, in the press).

Acridan (II) resembles diphenylamine rather than acridine. It is almost devoid of basic properties and does not fluoresce.

The molecule is not flat and there is no conjugation between the two benzene rings (v. Auwers & Kraul, 1925). Acridan is easily oxidised to acridine, *e.g.* by chromic acid or even boiling ferric chloride solution. Many of the aminoacridans autoxidise in the air but acridan may safely be dried in air at 120° C.

Acridone (III) is discussed in the next section.

4. Substituted Acridines

When a hydrogen atom in the acridine nucleus is replaced by another atom or group, the general effect on the properties resembles very much that brought about by similar substitution in the aromatic bases. For example, the introduction of a methyl- group into acridine makes it more oleophilic, slightly more basic, and shifts the foot of the ultra-violet absorption peak slightly further into the visible. Chlorine atoms have much the same effect except that they are base-weakening.

Nevertheless, a few categories of substituents, and these only when in particular positions, produce changes of an altogether different order and give rise to substances with quite exceptional properties. It may fairly be claimed that the chief interest of the acridine series resides in these unusual properties, either directly or indirectly. The following is a summary.

The Chloroacridines

Whereas the chlorine atom in 1-, 2-, 3- and 4-chloroacridines is very firmly bound, as in chlorobenzene, that in 5-chloroacridine is distinguished by its great lability (Graebe & Lagodzinski, 1893). In the presence of a trace of acid, it is rapidly hydrolysed to acridone (III) and solutions of 5-chloroacridine hydrochloride in cold water remain clear for only a few minutes. When 5-chloroacridine is warmed with phenol at about 100°, 5-phenoxyacridine is obtained quantitatively or, if ammonia or ammonium carbonate are simultaneously present, 5-aminoacridine is formed in about 90 per cent. yield (Albert & Ritchie, 1942). The chlorine in 4-chloroquinoline, which is the lower benzologue of 5-chloroacridine, is much less labile and requires a temperature of 160° to be converted to 4-aminoquinoline under the above conditions (Albert, Brown & Duewell, 1948).

The Hydroxyacridines

The substitution of a hydroxy-group in the 3-position of acridine introduces no unusual features apart from decreasing the solubility through intramolecular hydrogen bonding with the ring-

nitrogen (almost all hydroxy- and aminoacridines are less soluble than acridine). The only unusual feature of 1-hydroxyacridine is its ability to chelate with heavy metal ions in much the same way as the analogously substituted 8-hydroxyquinoline.

On the other hand, 5-hydroxyacridine (III, *b*) is a substance of highly individual character. Unlike its four isomerides, which are as basic as acridine and as acidic as phenol, it is remarkably deficient in acidic and basic properties so that it has long borne the special name "acridone" and been given a special formula (III, *a*). Yet, although it is rather deficient in phenolic properties, it is completely lacking in ketonic ones: hence formula (III, *a*) does not adequately express its properties. The very poor solubility of acridone in common organic solvents and the exceptionally high melting-point compared with those of its isomerides, suggests that the molecule may have a certain amount of zwitterionic character. Its properties are possibly best explained by considering it as a resonance hybrid of the traditional acridone structure (III, *a*) and the zwitterionic (III, *c*) form of the phenol (III, *b*). Acid amides are known to be resonance hybrids of this kind and the properties of acridone well qualify it for consideration as a vinylogous acid amide.*

The existence of two tautomers has never been demonstrated for acridone. On the other hand, the remaining hydroxyacridines, namely the 2- and 4- isomerides (Albert & Short, 1945) can, by small changes in the nature of the solvents, be made to give spectra characteristic of either the ketonic or the phenolic forms, corresponding to (III, *a* and III, *b*) respectively. For example, 4-hydroxyacridine gives a yellow solution in absolute alcohol, a green solution in 50 per cent. alcohol and a blue solution in 10 per cent. alcohol, whereas the kation is orange and the anion scarlet. There is nothing to suggest that either 2- or 4-hydroxyacridine is a resonance hybrid involving a structure analogous to (III, *c*).

THE AMINOACRIDINES

It is in the aminoacridines that the most unusual and interesting properties are encountered. It is interesting to note that almost all acridines which have been used as dyes or drugs contain an amino-substituent. The ions of the aminoacridines exhibit a surprising individuality of colour, ranging from the very pale yellow of 5-aminoacridine salts, through the orange-yellow of the

* A vinylogue is an analogue in which portions of the characteristic group are separated by one or more —CH=CH— groups (Fuson, 1935).

2- and the scarlet of the 1- and 3-aminoacridine salts, to the violet of 4-aminoacridine salts. The free bases show considerable evidence of chemical individuality. For example, under identical conditions the 2-, 3- and 4- isomerides methylate on the ring-nitrogen, the 5- isomeride on the primary amino-group, whereas the 1- isomeride does not methylate at all. 5-Aminoacridine is the only isomeride which does not diazotise and couple with β-naphthol under ordinary conditions and it does not form anils with aldehydes (Albert & Ritchie, 1943).

However, much more striking differences between the various mono-aminoacridines have been found in their strengths as bases. As this property has such a profound effect on their behaviour as dyes and (more particularly) as antibacterials, it will be discussed fully in the next section.

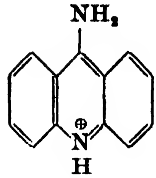
5. The Basic Strength of the Aminoacridines

Basic strengths are conveniently expressed in $p\mathrm{K}_a$ units, which are the negative logarithms of the acidity constants (K_a) in the following equation,

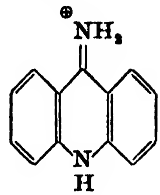
$$\mathrm{K}_a = \frac{[\mathrm{B}][\mathrm{H}^{\oplus}]}{[\mathrm{BH}^{\oplus}]}$$

From this it follows that stronger bases have higher $p\mathrm{K}_a$ values than weaker bases. It is readily seen that a difference of 3 between two $p\mathrm{K}_a$ values corresponds to a thousandfold (10^3) difference in basic strengths. The $p\mathrm{K}_a$ values of 120 acridines have been determined, most of them by potentiometric titration with the glass electrode (Albert & Goldacre, 1943; 1946).

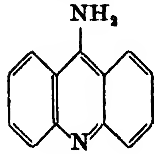
It is evident from Table I that the five isomeric mono-amino-acridines cover an unusually wide range of $p\mathrm{K}_a$ values. They fall into two distinct classes, (*a*) those that are of about the same strength as acridine (*i.e.* the 1-, 3- and 4- isomerides), and (*b*) those that are very considerably stronger bases (the 2- and 5- isomerides). The increased strength of these two isomerides has been traced to the phenomenon of ionic resonance. That is to say, the resonance energy of the ion is so much greater than that of the base that the equilibrium favours the ion much more than in the case of the other isomerides where this is not possible (Albert & Goldacre, 1943; 1946). This extra ionic resonance occurs between the structures (IV, *a* and *b*) derived from the two possible tautomeric forms of the non-ionised molecules (V, *a* and *b*). The original papers should be consulted for further details.



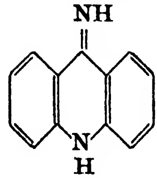
(IV, *a*)



(IV, *b*)



(V, *a*)



(V, *b*)

It has been found that the insertion of a second amino-group influences the pK_a of a mono-aminoacridine to much the same extent that it would influence that of acridine if inserted into the same position. For example, a 1-amino-group is base-weakening in acridine and hence the insertion of another 1-amino-group into 1-aminoacridine (giving 1:9-diaminoacridine) depresses the pK_a still further. On the other hand, a 2-amino-group is considerably strengthening in acridine and hence exerts a similar effect on 5-aminoacridine (see 2:5-diaminoacridine). Further examples of this effect can be seen in Table I. It may be claimed that the information brought to light in the above work, of which it has been possible to recount only a portion here, enables the prediction of the basic strength of hitherto unknown acridines to be made with an accuracy of about $\pm$ 0·5 unit of pK_a. The importance of being able to predict basic strength in advance of synthesis will be seen in the next two sections, particularly section 7.

6. The Acridine Dyestuffs

The two main classes of acridine dyestuffs are the *basic dyes* and the *vat dyes*.

The *basic dyes* are simple diaminoacridines, of which the most important are probably acridine orange (2:8-*bis*dimethylamino-acridine), chrysaniline (2-amino-5-*p*-aminophenylacridine) and Diamond Phosphine GF (2:8-diamino-3:7-dimethyl-10-methylacridinium chloride). These dyes have a high affinity for animal fibres. They are not sufficiently light-resistant to come in for consideration in wool- and silk-dyeing, but are much used for dyeing leather in various shades of yellow-brown and orange because of their durability on this material. Other important outlets for these basic

TABLE I

CONNECTION BETWEEN IONISATION AND BACTERIOSTASIS AMONG ACRIDINES

-acridine.	pK_a in Water 37° C.	Percentage ionised at pH 7·3 (37° C.).	Minimal Bacteriostatic Concentration for *Streptococcus pyogenes* (48 hours' incubation in 10 per cent serum-broth at 37° C. and pH 7·3).
(Unsubstituted).	5·4	1	1 in 5000
1-Amino-	4·2	<1	„ „ 5000
2-Amino-	7·7	72	„ „ 80,000
3-Amino-	5·6	2	„ „ 10,000
4-Amino-	5·7	2	„ „ 10,000
5-Amino-	9·6	99	„ „ 160,000
1:5-Diamino-	9·0	98	„ „ 80,000
1:9-Diamino-	3·8	<1	„ „ <5000
2:5-Diamino-	11·1	100	„ „ 160,000
2:7-Diamino-	7·8	76	„ „ 160,000
2:8-Diamino-	9·3	99	„ „ 160,000
3:7-Diamino-	5·9	4	„ „ 20,000
1-Amino-9-methyl-	3·6	<1	„ „ <5000
3-Amino-5-methyl-	5·8	3	„ „ 20,000
5-Amino-1-methyl-	9·8	99	„ „ 320,000
5-Amino-2-methyl-	9·8	99	„ „ 160,000
2-Amino-5-chloro-	6·4	11	„ „ <5000
2-Amino-8-chloro-	7·0	33	„ „ 10,000
5-Amino-3-chloro-	8·5	94	„ „ 160,000
5-Amino-4-chloro-	8·1	86	„ „ 160,000

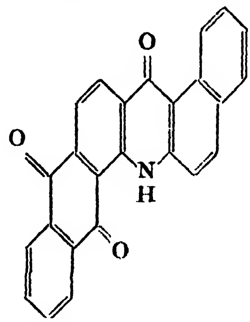
dyes are in the dyeing of crude cellulosic material such as jute, hemp, raffia, straw, sisal and coir. The more refined of these fibres require to be previously mordanted, *e.g.* with antimony tannate. It would seem that dyeing properties are practically lacking in acridines whose pK_a values are low (Albert & Bird, 1943; Bird, 1948).

These and other basic dyes are often laked with phosphotungstic acid to obtain pigments with a high degree of light-resistance which are used in paints, distempers and opaque inks.

Although the acridine dyes of commerce are confined to yellow and orange hues, every other possible colour is represented in the acridine series, including violet (*e.g.* 4-aminoacridine hydrochloride), blue (4-phenylaminoacridine), bluish-red (2-dimethylamino-7-aminoacridine hydrochloride) and green (5-p-dimethylaminostyryl-N-methylacridinium chloride).

Vat dyes are those which are insoluble in water but become soluble when stirred with an alkaline solution of sodium hydrosulphite (dithionite). Cotton is immersed in such solutions at about 50° C. and allowed to oxidise in the air. In this way dyeings of

the highest degree of light resistance are obtained and the general durability is good. The acridone vat dyes, of which (VI) is an example, were discovered by Ullmann in 1909. They are mainly red or reddish-purple in colour and, although they constitute only one small section of the large class of vat dyes, they fill an important gap because the colour-range is otherwise lacking in reddish hues.



Indanthrene Red RK
(VI)

7. The Acridine Antibacterials

Large quantities of aminoacridines are used in the prevention and treatment of sepsis in wounds, usually as a 1 in 1000 solution. For the treatment of infection existing in the blood-stream no acridines are known which are in any way comparable with penicillin and sulphonamides, but for external application (in large wounds or on mucous membranes) the acridines are often preferred.

In Table II, some of the more important biological properties of penicillin, aminacrine (*i.e.* 5-aminoacridine, one of the best of the acridine antibacterials) and sulphathiazole (a typical sulphonamide drug) may be compared. All three drugs are official in the British Pharmacopœia. It can be seen that penicillin is by far the most potent against those bacteria which are classified as "Gram-positive organisms," but has no useful activity against typical Gram-negative bacilli such as *B. coli* and *Proteus* which are common wound contaminants. 5-Aminoacridine, whilst less active against the Gram-positive organisms, maintains good activity against the Gram-negative types and hence has greater all-round usefulness. It has proved remarkably effective in cases of long-standing sepsis, particularly those heavily contaminated with Gram-negative organisms (Poate, 1944; Turnbull, 1944). It has also been most successful in preventing sepsis in the severer cases of civil casualties, particularly in children (Arden, 1945). It will be seen from Table II that sulphathiazole scores very poorly in

this standard test.* Sulphathiazole shows better activity in a special test using very few organisms (on the other hand, the activities of penicillin and 5-aminoacridine remain almost constant, regardless of the number of organisms). Hence, sulphathiazole can deal with a moderately infected wound, but not with one in which the number of organisms is really large.

TABLE II

BIOLOGICAL PROPERTIES OF IMPORTANT ANTIBACTERIALS

Substance.	Minimal Bacteriostatic Concentration (48 hours' incubation in 10 per cent. serum-broth at 37° C. and pH 7·3). The inoculum is one million organisms unless otherwise indicated. Bacteria.					Lethal Average Dose (L.D. 50, mouse, by Subcutaneous Injection), gm /Kg.
	Cl. welchii.	*Strept. hæm.*	*Staph. aur.*	*B. coli.*	*Proteus.*	
Penicillin . .	1 in: 1,500,000	1 in: 1,000,000	1 in: 1,000,000	1 in: <5000	1 in: <5000	No lethal dose
Aminacrine (5-aminoacridine)	320,000	160,000	40,000	40,000	40,000	0·08
Sulphathiazole	<5000	5000	<5000	<5000	<5000	1·75
Sulphathiazole*		50,000	50,000	50,000		

* With smaller inoculum (50,000 organisms) and carried out in a special medium favouring the test (lysed horse red cells on agar plate).

(The figures in this table were kindly furnished by Professor S. D. Rubbo, Melbourne University.)

It can also be seen from the last column of Table II that aminacrine is 21 times as toxic as sulphathiazole and that penicillin is not toxic at all. However, aminacrine is not used internally and, in the form used in wounds (a 1:1000 solution), it would not approach its toxic concentration unless several pints were absorbed, a manifest impossibility.

Sulphonamides are being used less and less in dressing wounds because sensitivity to these drugs is easily acquired when they are used externally, thus making it impossible to prescribe the drug later for a septicæmia. Quite apart from this, the free use of penicillin and sulphonamides in treating minor injuries is considered dangerous, because it has been found to produce drug-resistant strains of *staphylococci* which later cannot be controlled by these drugs. However, aminacrine does not produce drug-resistance of this kind (McIntosh & Selbie, 1943; Selbie & Simon, in the press).

It is interesting to compare all the modern antibacterials in

* Sulphanilamide is still less effective.

Table II with the older antiseptics such as phenol and chloroxylenol, which have little activity at dilutions greater than 1 in 100, especially if serum is present.

Besides aminacrine, another good non-staining acridine antibacterial is available. This is salacrin (1-methyl-5-aminoacridine), which has the great advantage of being compatible with isotonic saline solution. Other acridine antibacterials which are still in use are proflavine (2:8-diaminoacridine) and acriflavine (a crude mixture). These cause deep yellow stains on skin and clothing which are hard to remove. Of the two, proflavine is to be preferred because it is less toxic and has recently been marketed in a neutral condition, whereas the acriflavine of the British Pharmacopœia gives a 1 : 1000 solution with a *p*H of 2, a degree of acidity which is highly irritating to tender surfaces. In contrast to aminacrine, these yellow staining acridines can produce resistant strains of staphylococci.

Aminacrine, given orally, has been successful in cases of cystitis which have resisted other forms of therapy (Prof. S. D. Rubbo, personal communication). All the acridine antibacterials, at present on the market, are very rapidly shed from the blood-stream and hence are unlikely to be useful in treating septicæmias (*cf.* Keogh & Bentley, 1948). Nevertheless, the long persistence of atebrin in the blood-stream encourages the hope that acridine antibacterials could fairly readily be found which would also have a good persistence.

It may be of interest to consider how the acridine antibacterials came to be discovered and how their action is exerted.

Proflavine and acriflavine (v.s.) were prepared by Benda (1912) for Ehrlich to test in the experimental chemotherapy of trypanosomiasis in mice. Acriflavine proved successful in curing this disease in mice, but was of no value in man. Browning, who had been working in Ehrlich's laboratories upon the phenomenon of drug-resistance, took these acridine compounds back to Britain with him and later found that they had antibacterial powers which surpassed those of any substance then known. Not only were they active against a wide range of organisms at great dilution, but they were relatively non-toxic to man and their efficacy was unimpaired in the presence of blood-serum. They injured bacteria at so great a dilution that leucocytes, the blood's natural scavengers, were unharmed (Browning & Gilmour, 1913). Following upon the work of Garrod (1940), Berry (1941) and others, the superiority of proflavine over acriflavine was beginning to be realised when non-staining acridines were introduced (Rubbo, Albert & Maxwell,

1942) and these are now considered superior for reasons given above.

The first important clue as to the mode of action of the acridine antibacterials was obtained about this time (Albert, Rubbo & Goldacre, 1941) and was consolidated four years later, when a strong positive correlation was demonstrated between antibacterial action in this series and the percentage ionisation at the physiological pH value (7·3) (Albert, Rubbo, Goldacre, Davey & Stone, 1945). This conclusion is illustrated in Table I, which contains a representative selection of these authors' results which were further confirmed by tests on 22 bacterial species. It is evident that the presence or absence of amino-, methyl- or chloro-, etc., groups has no direct effect on the antibacterial action, but exerts an indirect effect by the usual inductive influence on ionisation.

The necessity for a high degree of ionisation was demonstrated not only by the use, at pH 7·3, of a number of bases of varying base-strength but also by working with a single substance at different pH values, using *B. coli* as the test organism. In these experiments, even weak bases became as antibacterial as strong bases, provided the solutions were made sufficiently acid to give complete ionisation (strong bases were completely ionised over the full range studied, *viz.* pH 5 to 9). When the minimal concentration of acridine kations required to effect bacteriostasis was plotted against the concentration of hydrogen ions present, a straight-line relationship was found (see Fig. 1). This clearly shows that acridine kations are competing against hydrogen ions for some vitally important anion on the bacterial surface. This could only be a weakly acidic group, such as a hydroxyl-group in the purine or pyrimidine constituent of a nucleic acid.

Apart from its intense theoretical interest, this demonstration of competition between acridine ions and hydrogen ions had clinical repercussions, because it showed that the acridines would act most beneficially when wounds were not allowed to become acid. It was actually found in the Australian Army that wounds made their best response to acridine therapy after a prior lavage with sodium bicarbonate solution.

The powerful antibacterial activity of aminoacridines is not found in the corresponding aminopyridines or aminoquinolines. This has been attributed to the necessity for a minimal flat area of contact between antibacterials (of the heterocyclic kation class) and the vital group with which they react, upon the bacterial surface. It is believed that this area of common contact provides sufficient van der Waals' bonds to overcome the tendency of the

molecule to become desorbed through its own kinetic energy. This explanation receives confirmation from the fact that the addition of a small flat area to the molecules of well-ionised quinolines or pyridines converts them into highly antibacterial substances (Albert, Rubbo & Burvill, in the press).

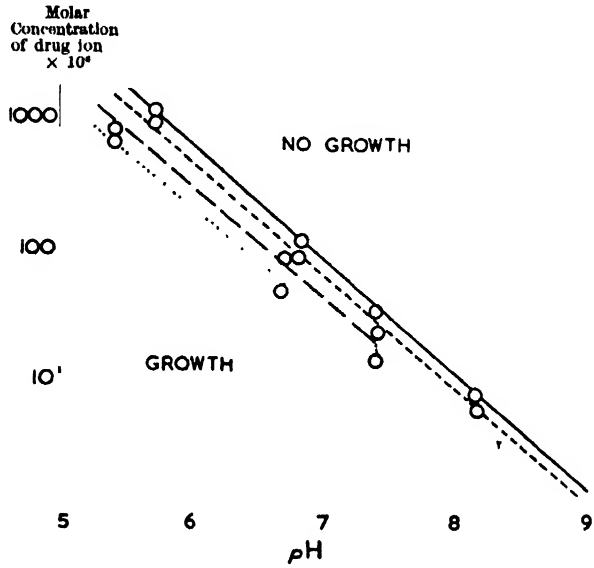


FIG. 1.—Competition between Hydrogen Ions and Aminoacridine Ions
B. coli.

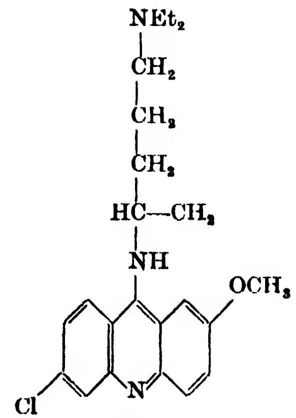
— 5-*Aminoacridine*.
···· 3-*Aminoacridine*.
- - 2:7-*Diaminoacridine*.
— 3:7-*Diaminoacridine*.

8. The Acridine Antimalarials

The first and most important of the acridine antimalarials was "Atebrin" (Mepacrine, B.P.; Quinacrine, U.S.P.), which was discovered in 1930 by Mauss & Mietzsch of the I.G. Farbenindustrie, Elberfeld (Mauss & Mietzsch, 1933). These workers were searching for a substitute for quinine, but Atebrin has proved a more powerful and less toxic drug than quinine. The opportunity for showing this conclusively arose during the second World War, when Hamilton Fairley carried out his now classical experiments on a thousand human volunteers in Queensland (Fairley, 1945). Some of the most decisive battles of the second World War were fought in highly malarious areas. Hence, after the fall of Java in 1942, and with it the loss of the world's supply of quinine, the Allied Armies would have been in a desperate position had not this superior drug been available.

At the present time, Atebrin is being manufactured and used in large quantities, although its position has been challenged by two newer drugs, chloroquin (the corresponding quinoline analogue) and proguanyl (" Paludrine ") which is an aryl diguanide.

The formula of Atebrin is given below (VII). It is seen to consist essentially of 5-aminoacridine bearing three substituents, (*a*) a basic side-chain, (*b*) a chlorine atom, and (*c*) a methoxyl-group. The side-chain can be shortened or lengthened without greatly impairing the antimalarial action, but there is no action in its absence. The chlorine atom intensifies the antimalarial action, but it may be substituted by a methyl-group or even by hydrogen without complete loss of activity. The methoxyl-group does not influence antimalarial activity, but reduces the toxicity of the drug to the human subject who often has to take 0·1 gm. daily, for years at a time, as a " prophylactic " (*i.e.* suppressive). It is rare for any untoward effects to accompany the use of Atebrin in this way, although the skin becomes temporarily yellow through the presence of the drug there. For treating an attack of overt malaria, 0·3 to 0·9 gm. is given daily for three weeks. This treatment cures malignant tertian malaria, but benign tertian malaria is apt to relapse at intervals for about a year under all known forms of therapy.



Atebrin
(VII)

No intimate connection between structure and biological function has been worked out for the acridine antimalarials. The problem is a much more difficult one to solve than with the acridine antibacterials, because the malarial parasites cannot yet be cultivated well enough *in vitro* to make satisfactory test-objects. Hence the experimental work is done mainly in birds using, of sad necessity, strains of malarial parasites which do not infect man. The drug, in these tests, is usually given orally and must have physico-chemical properties which will enable it to be absorbed well from the gut, persist for a long time in the blood-stream and be able to penetrate the red cells. It is easily appreciated that under the conditions of these tests it has not been easy to sort out which physical and chemical properties of the Atebrin molecule are responsible for these various desirable biological properties. How-

ever, it is now clear that the action of Atebrin does not consist in attacking the respiration of the parasites (Albert & Marshall, 1948). Those who seek data on the effect of varying the substituents in the molecule upon antimalarial action will find these references useful (Maghidson & Travin, 1936; Mietzsch & Mauss, 1934; Mietzsch & Klarer, 1942; Wiselogle, 1946).

9. Miscellaneous Properties and Uses of Acridines

(*a*) *In Medicine.*—"Tetrophan," a dihydrobenzacridine carboxylic acid, is used as a stimulant of the central nervous system in tabes dorsalis and in paresis. "Acranil," which is similar in structure to Atebrin, is the most potent chemotherapeutic agent against *Giardia*, the protozoon which causes the intestinal disease known as lambliasis. "Rivanol" is useful for controlling the bacterial infection which accompanies and prolongs amœbic dysentery; in addition, it has a sedative effect on the bowel in this disease. Both Rivanol and acriflavine are sometimes given orally to stimulate the flow of the bile and, possibly, sterilise it.

(*b*) *Other Biological Properties.*—Anticarcinogenic properties of a mild character have been attributed to many of the simpler acridines, whereas some of the benzacridines are mildly carcinogenic. Acriflavine induces, in yeast, a small-colony mutation which lacks normal cytochrome respiration (Ephrussi, private communication). Several simple aminoacridines interfere with mitosis in tissue-culture (Lasnitzki & Wilkinson, 1948).

Acridine orange has been shown to be a valuable stain for living cells. Acridine yellow is commonly used in fluorescence microscopy for the routine examination of tubercular sputum (Hughes, 1946). Rough and smooth strains of Gram-negative bacteria are commonly differentiated by Pampana's test with neutral acriflavine (Pampana, 1931).

(*c*) *Biochemistry.*—Acridine is the standard reagent for separating and identifying the adenosine phosphoric acids (Wagner-Jauregg, 1936; Baddiley & Todd, 1947). It is also used for the purification of such sulphate esters as sodium indoxyl sulphate and for the purification of sulphatases (Egami, 1939; Soda & Egami, 1940).

(*d*) *Analytical.*—Streptomycin is often determined by measuring the amount of fluorescent hydrazone produced when it reacts with acridyl-5-hydrazine (Boxer & Jellinek, 1947). Many heavy metals, particularly traces of zinc, which are so hard to pick up spectroscopically, are commonly determined with acridine thiocyanate (Langer, 1938).

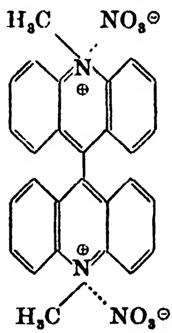
Very many acridines fluoresce strongly and several of them are

used as dicators when dilute or murky solutions have to be titrated, the operation being carried out under an ultra-violet lamp.

Ligninsulphonic acid, a common adulterant of soap, is easily detectable with various acridine dyes (Noll, 1938).

(*e*) *Industrial.*—5:5-Dimethylacridan (" D.M.A.") is used as a rubber antioxidant in America. Acridine yellow is one of the most potent of the organic bases used for preventing the oxidation of copper (Lawrence & Walton, 1942). Proflavine is employed in the sulphite-pulp section of the paper industry to inhibit anaerobic bacteria which cause corrosion to the plant (Bruce, 1941), and acridine yellow is similarly used for protecting pipes and cables from the action of hydrogen-sulphide generated by sulphate-reducing bacteria in water and soil (Rogers, 1940; Rogers & Barton-Wright, 1945).

(*f*) *Chemiluminescence.*—Very few pure chemicals show the phenomenon of chemiluminescence, *i.e.* the evolution of cold light during a chemical reaction. N:N′-Dimethyl-diacridylium nitrate (VIII) is an easily prepared compound, which is claimed to be the most strongly luminescent substance known (Decker & Petsch, 1935). It is called " Luzigenin " and has been known since 1909. A strong green light is emitted when it is gently oxidised with hydrogen peroxide.



Luzigenin
(VIII)

REFERENCES

ADKINS & COONRADT (1941), *J. Amer. Chem. Soc.*, **63**, 1563.
ALBERT (in the press), *The Acridines*, London, Edward Arnold & Co.
ALBERT & BIRD (1943), *J. Soc. Dyers Colour.*, **59**, 74.
ALBERT, BROWN & DUEWELL (1948), *J. Chem. Soc.*, p. 1284.
ALBERT & GOLDACRE (1943), *J. Chem. Soc.*, p. 454.
—— (1946), *J. Chem. Soc.*, p. 706.
ALBERT & MARSHALL (1948), *Nature*, **161**, 1008.
ALBERT & RITCHIE (1942), *Organic Syntheses*, **22**, 5.
—— (1943), *J. Chem. Soc.*, p. 458.
ALBERT, RUBBO & GOLDACRE (1941), *Nature*, **147**, 332.

ALBERT, RUBBO, GOLDACRE, DAVEY & STONE (1945), *Brit. J. Exper. Path.*, **26**, 160.
ALBERT & SHORT (1945), *J. Chem. Soc.*, p. 760.
ALBERT & WILLIS (1946), *Nature*, **157**, 341.
ARDEN (1945), *Med. J. Aust.*, ii, 486.
v. AUWERS & KRAUL (1925), *Zeits. Physik. Chem.*, **116**, 438.
BADDILEY & TODD (1947), *J. Chem. Soc.*, p. 648.
BENDA (1912), *Berichte*, **45**, 1787.
BERNTHSEN & BENDER (1883), *Berichte*, **16**, 1802, 1971.
BERRY (1941), *Quart. J. Pharm. Pharmacol.*, **14**, 363.
BIRD (1948), *J. Soc. Dyers Colour.*, **64**, 357.
BOXER & JELLINEK (1947), *J. Biol. Chem.*, **170**, 491.
BROWNING & GILMOUR (1913), *J. Path. Bact.*, **18**, 144.
BRUCE (1941), *J. Soc. Dyers Colour.*, **57**, 310.
CRAIG & SHORT (1945), *J. Chem. Soc.*, p. 419.
DECKER & PETSCH (1935), *J. Prakt. Chem.*, **143**, 211.
EDINGER & ARNOLD (1901), *J. Prakt. Chem.*, **64**, pp. 182, 471.
EGAMI (1939), *J. Chem. Soc. Japan*, **60**, 849, per *Chem. Abs.*, 1942, 6180.
FAIRLEY (1945), *Trans. Roy. Soc. Trop. Med. Hyg.*, **38**, 311.
FUSON (1935), *Chem. Reviews*, **16**, 1.
GARROD (1940), *Lancet*, i, 798 and 845.
GRAEBE & CARO (1871), *Annalen*, **158**, 265.
GRAEBE & LAGODZINSKI (1893), *Annalen*, **276**, 35.
HUGHES (1946), *Tubercle*, **27**, 91.
KEOGH & BENTLEY (1948), *Aust. J. Science*, **11**, 98.
LANGER (1938), *Mikrochem.*, **25**, 71, per *Chem Abs.*, 1939, 1629.
LASNITZKI, I., & WILKINSON, J. H. (1948), *Brit. J. Cancer*, **2**, 369.
LAWRENCE & WALTON (1942), *J. Phys. Chem.*, **46**, 609.
LEHMSTEDT & WIRTH (1928), *Berichte*, **61**, 2044.
MAGHIDSON & TRAVIN (1936), *Berichte*, **69**, 537 ; *J. Gen. Chem. Russia*, **6**, 909.
MAUSS & MIETZSCH (1933), *Klin. Woch.*, **12**, 1276.
McINTOSH & SELBIE (1943), *Brit. J. Exper. Path.*, **24**, 246.
MIETZSCH & KLARER (1942), *Medizin und Chemie*, **4**, 73 ; Leverkusen : Bayer.
MIETZSCH & MAUSS (1934), *Angew. Chem.*, **47**, 633.
NOLL (1938), *Seifensieder Zeitung*, **65**, 35.
PAMPANA (1931), *Ann. Igiene Sper.*, **41**, 537.
POATE (1944), *Med. J. Aust.*, i, 242 ; *Lancet*, ii, 238.
RADULESCU & OSTROGOVITCH (1931), *Berichte*, **64**, 2233.
ROGERS (1940), *J. Soc. Chem. Ind.*, **59**, 34 ; *cf.* Brit. Pat. 497,829 (1938).
—— & BARTON-WRIGHT (1945), *J. Soc. Chem. Ind.*, **64**, 292.
RUBBO, ALBERT & MAXWELL (1942), *Brit. J. Exper. Path.*, **23**, 69.
SODA & EGAMI (1941), *J. Chem. Soc. Japan*, **62**, 256, per *Chem. Abs.*, 1943, 4084.
TURNBULL (1944), *Aust. & N.Z. J. Surg.*, **14**, 3.
—— (1945), *J. Chem. Soc.*, p. 441.
WAGNER-JAUREGG (1936), *Zeits. Physiol. Chem.*, **239**, 188.
WISELOGLE (1946), *Survey of Antimalarial Drugs*, U.S. Office of Scientific Research & Development ; Ann Arbor : J. W. EDWARDS ; Vol. II, Part II, pp. 1321–78.

SEMICONDUCTORS

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The term "semiconductor" evokes quite naturally a picture of a substance which is neither a metallic conductor nor an insulator, but something between—a sort of poor insulator or highly resistive conductor. In one sense this idea is not entirely wrong, for the specific conductivity of semiconductors does lie in the intermediate zone of magnitudes. On the other hand, it is now quite clearly recognised that these substances are not to be placed in the neglected fringes of the other two groups, but have characteristic properties of their own. The most illuminating of these are probably to be found in the temperature variations of the electrical properties. Thus, whereas in a pure metal the conductivity diminishes as the temperature rises, in semiconductors it increases, at a rate which augments as the temperature rises. This striking behaviour can be demonstrated without difficulty by joining the ends of a short glass rod to (say) the 230 volt a.c. mains. At room temperatures, the glass is to all intents an insulator; but if it is warmed with a bunsen flame it begins to conduct, so that when the flame is removed the power dissipated by the current in the glass is sufficient to maintain the action, and the glass will eventually melt. A similar experiment may be performed with a stick made of a compressed semiconducting oxide powder, although the melting-point—usually very high—may not be attained. This method has indeed been applied to the production of small single crystals of semiconducting substances. It is also at the root of the stabilising action produced in an electric circuit when a semiconducting element is suitably introduced into it. A sudden surge of current in the circuit raises the temperature and reduces the resistance of the element, and if the element has been placed in parallel with an instrument it will take the lion's share of the surge. A great many semiconductors are also distinguished from metals by another peculiarity. Continued purification of a metal reduces its residual resistance, so that its conductivity in the purest state is a maximum. For most semiconductors, on the other hand, continued purification

reduces the conductivity; so much so indeed as to suggest that the special properties of semiconductors are functions of the impurities and not of the bulk material, which would presumably be an insulator in the perfectly pure state. It is this peculiarity which makes the detailed study of semiconductors much more baffling and difficult, and frequently leads to flat contradictions between results from different experiments. It has been possible, notwithstanding, to unravel a considerable length of the theoretical skein, and a good deal of order is now visible.

It is now clearly recognised that, while some semiconductors conduct by means of free electrons or positive charges in something of the manner described in the old Drude theory, in others the process is electrolytic, a migration of the ions through the solid material having been detected and measured in some instances. This latter type are frequently called "ionic" conductors, and some authorities prefer not to apply the term "semiconductor" to them. Conduction in glass at high temperatures is due to such an electrolytic or ionic process. Those semiconductors which depend on the motion of free electrons are known as "electronic" semiconductors. Some materials are known to conduct partly by both processes and it is not always a simple matter to distinguish between the two. While, in this article, both types will be referred to as semiconductors, most attention will be paid to the electronic variety. One of the first things to be done in investigating a new material is an investigation of the character of the conduction process, whether electronic, ionic, or a mixture of both. Various phenomena give some indication of ionic conduction—polarisation, for example, which may result in apparent departure from Ohm's law, as in Cr_2O_3 [1], or the actual transfer of measurable quantities of one of the ions, as in Ag_2S [2]; and there are some phenomena, such as the Hall and thermoelectric effects, which are thought to be characteristic of electron conduction only, the low mobility of the ions in electrolytic conduction making any measurable contribution from them extremely unlikely. However, the matter is not always simple, owing partly to complications of an experimental character, such as the presence of contact resistances, and partly to the difficulty of interpreting apparently simple results. The classic instance is that of αAg_2S, a substance to which Faraday devoted some attention in 1834. This material, investigated by Tubandt, was proved to be an electrolytic conductor, at least in part, by experiments on direct deposition, and Faraday's law of electrolysis was found to be obeyed. This last fact led Tubandt to conclude that it was an almost completely ionic conductor.

On the other hand αAg_2S shows readily measurable Hall and thermoelectric effects, and the mobility of the Ag ions, calculated on the assumption of purely ionic conduction, turns out to be far greater than the value obtained experimentally for Ag ions by experiments on diffusion. A long controversy on these somewhat contradictory results terminated in general agreement that αAg_2S is electronic as to 99 per cent. of its conducting power, and only very slightly ionic, a result drawn not from the conductivity results alone, but from a variety of different experiments [3].

Some of the principal results of experiments with semiconductors may now be recounted, with the preliminary comment that the magnitudes involved vary so much from one semiconductor to another that no general method of experiment can be laid down. Almost every new material studied has to be given special consideration and often investigated by a special method. This does not lead to easy correlation of results. Moreover, small differences in the method of preparation, heat treatment or purity, produce unfortunately large variations in the experimental results. An easy and correct inference from this is that, while conclusions about the general trend of results may well be justified, the significance of detailed numerical values ought not as a rule to be overemphasised. We may begin with the results for conductivity, upon which more experiments have been made than upon any other electrical effect. In a good many instances of experiments on compressed-powder specimens of metallic oxides, or of coherent strips made by oxidising the pure metal, or even in a few cases on single crystals, it is found that the logarithm of the conductivity σ is a linear function of $1/T$, T being the absolute temperature. If the range of temperature is large enough, it not infrequently happens that the graph of $\log \sigma$ against $1/T$ consists of two rectilinear parts as in Fig. 1, the part corresponding to higher values of T having the steeper slope. When different samples of the same material are tried, it is often possible to obtain a set of graphs like that of Fig. 2 (which is for NiO). Here, it will be observed, the high-temperature lines lie much closer together than the low-temperature portions, although the lines are substantially parallel. These results, obtained by R. W. Wright at Queen Mary College, closely resemble results obtained for cuprous oxide by Jusé and Kurtschatow [4] and are typical of a number of substances. The samples A, B and C were formed from the same original metallic nickel, and differed only in their preliminary heat treatment, an observation of great interest. The original nickel, though of high purity, is known to have contained a few traces of metallic impurity, and

it is conceivable that the final concentration of each impurity may depend on the duration and temperature of the preliminary heat treatment, some impurities being more volatile than others. It is also well established that, when an oxide is heated in an atmosphere of air or oxygen, a kind of chemical equilibrium is established between ions in the solid lattice and the oxygen outside, which leads to a slight deviation from the exact stoichiometric proportions of the component elements inside the solid. In order to accommodate

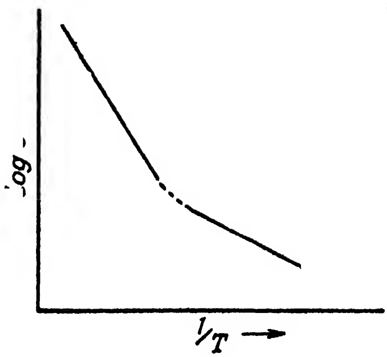

FIG. 1.

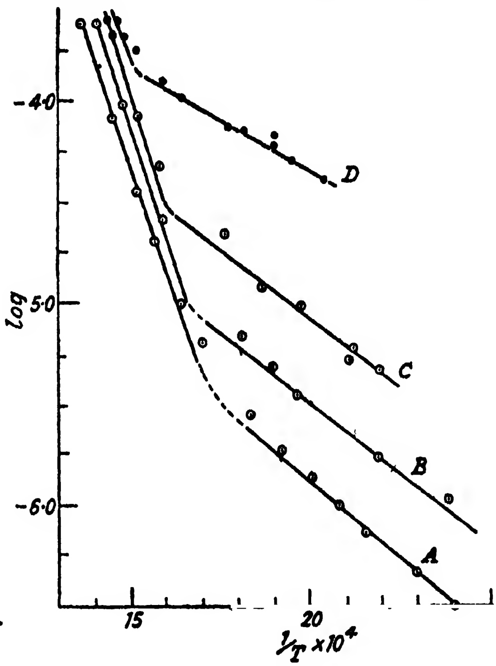

FIG. 2.—NiO.

itself to this small excess of one or other of the ions, the crystalline lattice must allow slight distortions and adjustments of the same character as those required to accommodate foreign impurities. In addition to the possible effects of heat treatment on the impurity concentration, another possibility arises which could occur in a chemically pure crystal. There is good reason for believing that an ion in a crystalline lattice may occasionally become untethered and wander from its proper position, being finally recaptured in a false interstitial position, and leaving behind it a vacant lattice site. This kind of fault is known as a Frenkel defect. Alternatively, the wandering ion may find a resting place on the surface of the crystal, as part of a new crystal layer. This fault, which also involves a vacant lattice site, is a Schottky defect. It is

clear that these defects must also produce the kind of readjustments necessary for the accommodation of impurities. Moreover, in order to produce a defect in the perfect crystal, a certain amount of work must be done—it is therefore associated with an activation energy. Now the most likely source of energy for the formation of the defects is the thermal oscillation of the lattice, so that the concentration of the defects should be a function of the temperature. In fact it may be shown that if W, W′ are the amounts of work required for the formation of a Frenkel and a Schottky defect respectively, then the concentration of Frenkel defects is proportional to $e^{-W/2kT}$, and the concentration of Schottky defects is proportional to $e^{-W'/kT}$, k being Boltzmann's constant. When one recalls, firstly, that according to the simple electron theory of conduction the conductivity σ is proportional to the number of charge carriers, and secondly that the experimental results already referred to show that $\sigma = \sigma_0 e^{-\Delta/T}$ for a semiconductor, the possible correlation of conductivity and defect concentration becomes evident. Moreover, when a solid is allowed to attain thermal equilibrium at a high temperature and then cooled rapidly, the lattice does not have time to readjust itself to contain the much smaller number of defects which it can maintain in thermal equilibrium at the lower temperature, and considerable numbers are, as it were, frozen in. Different rates of cooling will leave different concentrations of "frozen" defects, so that we arrive at another means by which the heat treatment can affect the properties of the solid.

Reverting now to Fig. 2, it appears that these considerations apply most aptly to the low-temperature sections, where decided differences are found between the specimens, but not so clearly to the high-temperature portions, where, in any case it must appear that some new mechanism enters to give rise to the line of steeper slope. The probable significance of the two parts of the graphs will be referred to later.

Meanwhile we may recall the important application to the measurement and control of temperature, as in the *thermistor* and allied devices, of the rapid change of resistance with temperature which occurs in many semiconductors.

In a growing proportion of investigations, not only the conductivity, but other related electrical properties, particularly the Hall and thermoelectric effects, have been measured. One important reason for these last two measurements is that the effects depend upon the first power of the electronic charge, or the charge of the "carrier," whatever its nature, responsible for conduction—

whereas conductivity depends upon the square of the charge. From the direction of the thermoelectric or Hall effect, then, conclusions may be drawn about the apparent sign of the carrier charge—a principle equally applicable to metals, some of which (Bi, Pd, etc.) behave as though they contained free positive charges, while many others have negative carriers. The same dichotomy can be made for electronic semiconductors. Many (*e.g.* ZnO, CdO, etc.) appear to employ negative carriers, while others, (*e.g.* NiO, Cu_2O, etc.) appear to use positive carriers. There are indeed some substances known (*e.g.* PbO) in which the sign of the carrier appears to be negative at some temperatures and positive at others [5]. Not infrequently it is difficult to prove that carriers of only one sign are operative—and indeed there are good theoretical reasons for believing that conduction can take place by means of both kinds of carrier at the same time and in different degree. Those electronic semiconductors in which electrons are considered to predominate are often called *excess* or *normal* or *n-type*, while those with a balance of positive carriers are called *defect*, or *abnormal* or *p-type*.

Any explanation of semiconduction must form a part of the general theory of solids, an account of which was given by E. E. Schneider in a recent number of SCIENCE PROGRESS [6]. Reference may be made to that article for detail, and only a brief recapitulation of essential parts need be given here. In some respects the modern theory of conduction departs considerably from the early theory of Drude. In Drude's theory conduction was supposed to be due to the drift imposed on the atmosphere of free electrons assumed to permeate conducting solids, when an electric field was superposed. An insulator was a substance with few or no free electrons. No explanation of semiconductors, or indeed of apparently positive carriers of charge as suggested by the sign of Hall and thermoelectric effects, is to be found in the theory. For a more adequate account it is necessary to pass to the "band" theory of solids. The idea of a free or gaseous atom as consisting of a nucleus surrounded by shells of electrons is now familiar, and the electrons, it is well-known, are found only with certain discrete values of energy or, in other words, at certain energy levels, values of energy between these levels not being available to the electron. When the atom is imprisoned in the lattice of a crystalline solid, however, the system of sharply defined energy levels is modified by the proximity of all the other atoms in the lattice. In broad terms it may be said that each energy level of the free atom is replaced by a set or *band* of closely related energy levels, with very

small energy differences between the constituent levels of a band. The bands continue to be separated in general by forbidden regions, though the widths of the prohibited regions are not the same as before, and overlapping of successive bands may even occur. To each of the levels in this band system the Pauli exclusion principle applies, namely, that no more than two electrons having opposite spins can be found in any one level. When every level in a band is " occupied " by its pair of electrons, the band is said to be *filled*, a condition most likely in the bands deriving from the inner orbits of the free atom. Now the electrons in such a filled band are unable to take part in conduction, for when an electric field is exerted an electron can only acquire more kinetic energy by rising to a higher energy level, which must therefore be unoccupied. Any increase of velocity of one electron must consequently be exactly counterbalanced by an equal decrease of velocity in another, the two interchanging their energy levels and leaving the total energy unchanged, and with no resultant drift of charge. For similar reasons the electrons cannot contribute to the specific heat. If we can imagine a material in which all the energy bands are completely filled, therefore, we have a picture of an insulator. In practice there is one escape from this curious situation, afforded by the existence of another band, entirely or partly denuded of electrons, and separated from the top of the highest filled band by a forbidden zone. A good many real insulators may be considered to possess, at the absolute zero of temperature, such a system of filled bands, separated from a completely empty band in the manner described. At higher temperatures it may become possible for electrons from the filled band to acquire enough thermal energy to lift them right over the forbidden zone into the empty band. Once there, they are practically free, at any rate while the number of levels greatly exceeds the number of excited electrons, and they can therefore give rise to conduction, and, incidentally, contribute to the specific heat. According to well-known statistical principles, if the width of the forbidden zone is W units of energy (usually electron volts) the equilibrium concentration of electrons in the upper band will be proportional to $e^{-W/kT}$, and the resulting conductivity will also be proportional to this quantity. Every electron which succeeds in bridging the gap, however, leaves behind it a vacant level in the filled band, into which other electrons can be excited, so that the uppermost filled band now takes some part in the conduction. Conditions are different however. It has been shown, for example, that the effective mass of the electron moving among the few vacancies of the filled band can differ very markedly

from that of a free electron, and indeed may appear to be negative. If the movements of the few vacancies or "holes" in the band are studied, it turns out that the effect is as though *positive* charges, with positive masses usually exceeding the electronic mass, are moving freely among the energy levels of the lower band. This property of the *positive holes* is invoked elsewhere in the theory to account for the apparent positive sign of the carrier. Meanwhile, it may be noted that we have arrived at a model in which conduction is due to both positive and negative charges, and in which the temperature variation of conductivity is of the type found in semiconductors. Owing to the compensating effect of the opposite charges, a material behaving like this would be expected to show only a small thermoelectric or Hall effect. For classification purposes this is called an *intrinsic* semiconductor. Since in known materials the energy gap is often of considerable width, appreciable conduction of this kind will generally be looked for only at quite high temperatures. The steeper, high-temperature parts of the conductivity graphs, like those of Fig. 1, are often ascribed to intrinsic conduction, and in those instances where the high temperature results appear to be independent of heat treatment or impurity content, this may be true. Intrinsic conduction should clearly be a property of the bulk material, not of the impurities or defects primarily. Jusé and Kurtschatow have given reasons for considering the high temperature conductivity of cuprous oxide to be intrinsic, and silicon and germanium are other examples. Intrinsic conduction is not the only explanation of the high-temperature sections of the conductivity graphs, so that authenticated examples of intrinsic conductors are few.

Consider now what happens in an energy band which is partially filled at absolute zero, the number of levels and the number of electrons both being large. At all temperatures conduction is possible, because the electrons of greatest energy are within easy reach of unoccupied levels. On the other hand, at all accessible temperatures the electrons of low energy will be surrounded only by filled levels, and will therefore be in an unreceptive condition, unable to take part in such processes as conduction, which will therefore be governed by the density of electrons in the uppermost bands. This state of affairs is the condition of degeneracy characteristic of metals, and leads to a satisfactory explanation of their properties. If, however, the total number of levels in the band greatly exceeds the number of electrons, all the electrons can take part, and degeneracy is absent. The characteristics of the electron motion are then practically identical with those in the Drude

theory, a simplification which evidently applies to intrinsic conductors.

On the other hand, many of the most interesting and industrially most important of the properties of semiconductors are more characteristic of the impurities or defects than of the bulk material, and the theory just outlined offers no clue to this peculiarity. The modifications required to account for impurity semiconductors were made by A. H. Wilson [7]. Let us suppose that an atom of a foreign impurity is present in the lattice of an otherwise pure material. The maladjustment of the ions of the bulk material in its neighbourhood will in general introduce some corresponding change in the system of energy levels. Wilson suggests that a new energy level appears somewhere in the forbidden region between filled and empty bands. Two possibilities are considered, as illustrated in Fig. 3. In both cases it is assumed that the lower

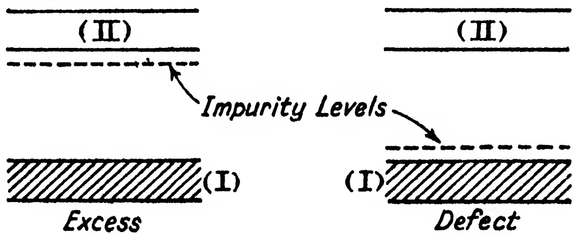

FIG. 3.

band (I) is completely filled at absolute zero, while the upper band (II) is completely empty at absolute zero. In the first instance the new energy level, or narrow set of levels, is to be found just below the band II, and all the new levels are supposed filled at absolute zero. At any higher temperature some of the electrons from the impurity levels will be excited into the empty, or conduction, band, the activation energy required for this operation being much less than that required for excitation from band I, as in an intrinsic conductor. Consequently, excitation from impurity levels will dominate the action at the lower temperatures, and conduction will be due to the motion of electrons only, in the conduction band. We thus arrive at a model of the excess type of impurity semiconductor. In the other instance, the narrow band of new levels is supposed to be found near the top of the filled band I, and in this case the impurity levels are supposed to be completely unoccupied at absolute zero. At higher temperatures electrons are excited from the filled band to the impurity levels, leaving mobile positive holes in the lower band. This gives a model of the defect type of impurity semiconductor, and shows how the conduction

takes place by means of positive holes only. Now in either of these revised models, the rate of increase of the conductivity is governed by the activation energy, or the interval on the energy diagram between the impurity levels and the appropriate band, and it is this activation energy which differs for different kinds of impurity. It will thus appear that the electrical properties, at any rate at the lower temperatures, will appear to be characteristic of the impurity rather than of the bulk material, although it would be more exact to say that they are characteristic of a particular impurity in a particular bulk material. It is not difficult to extend these ideas to the case of a material containing two or more kinds of impurity each giving rise to its own impurity levels, and each associated with a characteristic activation energy. In such a material the low-temperature conduction would be governed by the impurity of lower activation energy, while at higher temperatures the impurity of higher activation would be likely to take charge. In this way it is possible to account for the two rectilinear sections of the log σ graphs of Fig. 1, without invoking intrinsic conduction. The complete experimental analysis of the electrical properties of a material possessing this order of complexity is naturally difficult, and has rarely been accomplished fully.

An impurity conductor of this type is clearly a substance whose electrical properties are subject to artificial control and modification by the deliberate addition of impurities. Advantage has been taken of this possibility in important technical applications. Thus, in the construction of germanium rectifiers, it is possible, by adding up to 0·5 per cent. of antimony as an impurity, to produce an excess type of semiconductor, whereas the addition to the pure germanium of 0·1 atomic per cent. of tin produces a defect semiconductor. It has even been found possible merely by a suitable heat treatment to convert defect germanium to excess. A less-spectacular but equally important example of artificial control is to be found in the effect on the properties of oxide semiconductors produced by varying the pressure of the oxygen in the surrounding atmosphere, an effect to which reference was made earlier. Similar effects have been found with sulphide semiconductors in an atmosphere of sulphur, and in cuprous iodide in iodine. The mechanism of this effect may be illustrated from the behaviour of the excess conductor CdO, investigated by Hogarth [8]. When this material is placed in an atmosphere of oxygen the ions in the crystal lattice tend to dissociate and settle down to a quasi-chemical equilibrium with the surrounding atmosphere. The dissociation may be represented thus:

$$2CdO \rightleftharpoons 2Cd^{++} + 2O''$$

If the pressure of oxygen in the surrounding atmosphere is now reduced, some of the oxygen ions are evolved as oxygen gas, leaving behind them their electrons ($2O'' \rightarrow O_2 + 4e$), and, of course, a stoichiometric excess of Cd ions. In consequence the electrical conductivity increases somewhat at lower oxygen pressures, while the thermoelectric power and Hall effects, which depend inversely on the concentration of free electrons, are reduced. In a material like NiO which is a defect conductor, on the other hand, the electrical conductivity is smaller at reduced oxygen pressure, and higher at the high pressures. For this reason electronic semiconductors are sometimes subdivided into "reduction" (excess) or "oxidation" (defect) types.

In order to follow the electronic processes which underlie the electrical properties of semiconductors, it will be seen then that a knowledge of the concentration of free electrons or positive holes is demanded. This in turn requires a knowledge of the impurity (or lattice defect) concentration, and of the activation energies involved. In order to obtain these data it is necessary to perform experiments on more than one of the electrical properties. This may be seen from the formulæ derived from the theory for the measurable electrical quantities. Thus we have for excess conductors, for the conductivity

$$\sigma = \frac{4}{3} \cdot \frac{e^2 ln}{(2\pi mkT)^{\frac{1}{2}}}$$

for the Hall effect

$$R = -\frac{3\pi}{8} \cdot \frac{1}{en}$$

where e is the electronic charge, m the mass of the electron, l the mean free path of the electrons, k Boltzmann's constant, T the temperature and n the concentration of free electrons. The formula for thermoelectric power P is somewhat more involved, viz. :—$P = \frac{k}{e}\left(\frac{5}{4} - \mu\right)$ where μ, the reduced chemical potential of the electrons, is a function of the impurity concentration, the activation energy, and the temperature. The nature of the function is known, from the work of Fowler [9] and of Shifrin [10]. The importance of measurements of the Hall effect R is at once obvious from the fact that they give n immediately. Ironically, however, it turns out that the most immediately useful measurement (of R) is commonly the most difficult to make, while the easiest (of P)

is often the most difficult to interpret; so that it is becoming more customary to carry out measurements of all three, if possible simultaneously and at various temperatures.

From a physical standpoint the interest of semiconductors no doubt derives from their importance in the theory of solids, but much of the widespread interest taken in them at the present time is related to their important practical applications. One of the best known of these is the rectifying action of the interface between a metal and a semiconductor placed in contact with each other, although, strictly speaking, this is an application in which the use of a semiconductor seems to be a practical rather than a theoretical necessity. Any brief account of the complex process of rectification is certain to be over-simplified, but fortunately several authoritative works are now available for detailed reference [11, 12]. Let us suppose that a block of metal is brought into

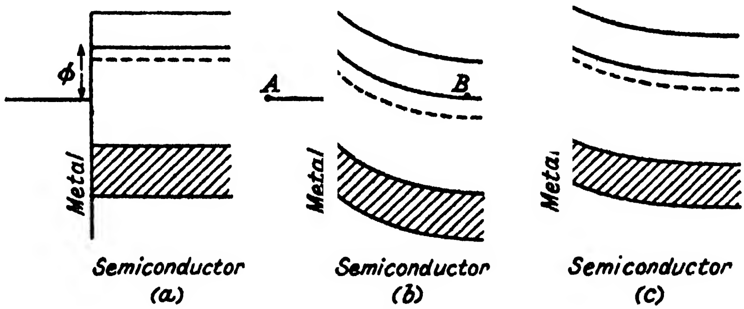

Fig. 4.

intimate contact with a block of (say) an excess semiconductor, then at the instant of contact the energy levels will appear somewhat as in Fig. 4 (*a*).

In this diagram energies are plotted vertically and distances normal to the interface, horizontally. The level indicated in the metal is the "Fermi level," which at absolute zero separates a region of completely filled from a region of empty levels in the metal. According to this picture, while an electron in the metal would have to surmount a potential barrier of height ϕ before entering the conduction band of the semiconductor, any electrons already in the conduction band can readily pass to the empty levels of the metal. Accordingly negative charge is immediately transferred to the metal, leaving a positive space charge in the semiconductor, a condition leading to the retention of electrons in the semiconductor, and a depression of the levels as shown in (*b*), which represents the equilibrium state. Electrons at A or B,

moving toward the interface, will be presented with potential barriers of much the same height. But if an external field is applied in the direction tending to urge electrons from the semiconductor to the metal, the semiconductor levels are once more raised, as in (c), and it becomes easier for electrons to cross to the metal. A reverse field, however, while it lowers the semiconductor levels, does not affect the height of the barrier as seen from the metal. In this way it appears that the resistance to the flow of electrons from semiconductor to metal must be less than for electron flow from metal to semiconductor. In a semiconductor dependent on hole conduction, similar arguments apply, and the easy direction is that in which positive charges flow from semiconductor to metal, as in the cuprous oxide rectifier.

Quite recently a dramatic extension of these experiments on rectification has led to the invention of a device, known as the *transistor* or *crystal triode* [13], closely analogous in its behaviour

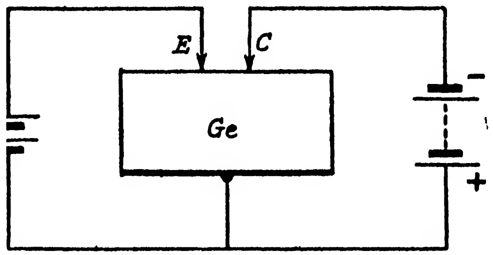

FIG. 5.

to the triode valve. The transistor consists of a small block of germanium (or silicon) to which electrodes are fixed as in Fig. 5, in the manner of the cat-whisker of early wireless days, the two whiskers being set very close together—sometimes as near as 0·002 cm. It is found that when a small positive bias is applied to the *emitter* E, and a suitable negative bias to the *collector* C, then a small change of emitter current results in a larger change of the collector current, an effect which in suitable circuits can be used to produce voltage amplification, or sustained oscillation, as in a triode valve. In explanation the inventors have suggested that the bulk of the germanium, which in this case is an excess conductor, differs from the surface layer which conducts by positive holes, and that the control effect is due to the passage of positive holes through the semiconductor from E to C. Whatever may be the explanation, however, the facts are quite clear, and already a complete radio receiver has been constructed with transistors replacing the valves.

Large numbers of semiconductors are known which become luminescent when stimulated by suitable radiation, whether this be ultra-violet or X-rays, alpha rays or cathode rays. The study of these phosphors with a view to their technical applications has led to an enormous output of experimental results, for the most part of great complexity. Any adequate account here is quite impossible, but reference to one or two aspects may be made. The vast majority of phosphors have to be "activated," that is to say small quantities of chosen impurities must be added to the pure materials before they exhibit luminescence, and the colour of the light then emitted under suitable stimulation depends on the nature of the impurity. Thus, when zinc sulphide is activated by a small quantity of copper, the emitted light is green, but when activated by zinc or silver in correct proportions it is blue. In some degree this action can be understood, if the insertion of the impurity atoms into the lattice establishes new energy levels in the forbidden zone, as in the ordinary impurity semiconductor. If at any stage in the process of stimulation an electron is caused to pass from an impurity level to the conduction band, it may at a subsequent instant drop back, perhaps remaining for a short time on the way in an excited state. The energy it loses in returning to its original state is no doubt dissipated as heat very often, but under some circumstances it may be surrendered as a quantum of light whose frequency is then determined by the position of the impurity level. Now a feature common to all phosphors is the decay of the luminescence. After the removal of the exciting radiation the luminescence dies away, sometimes very rapidly, say in a millionth of a second, and sometimes over longer periods perhaps extending to several minutes. This implies that the electrons raised to the conduction band by the exciting radiation have been prevented from returning immediately. It is supposed that the conduction electrons, in wandering about the crystal, encounter singularities at which they are trapped. They remain imprisoned in the traps until they can acquire enough energy from the thermal movements of the lattice to escape. Clearly, the phosphors of long delay—the phosphorescent materials—must have deep traps from which the electrons cannot readily escape. In illustration it may be observed that, when a zinc sulphide phosphor is illuminated at the temperature of liquid air by a radiation which would excite luminescence at room temperature, the expected luminosity does not appear, but when the phosphor is warmed up a burst of luminescence is observed, showing that, while at low temperatures very few of the trapped electrons acquired enough energy to release

them, at suitably high temperatures they were able to escape in large numbers.

Not unrelated to these effects is the phenomenon of photoconduction, a name for the increase of conductivity shown by some semiconductors when illuminated by appropriate radiation. A curious feature of the phenomenon is that it occurs only in semiconductors which have cations with closed shells of electrons, as in Cu_2O or Ag_2S, but at bottom the effect must be an example of the result of raising electrons from a lower level up to the conduction band—an alternative, as it were, to the thermal excitation already considered. In photoconduction, however, on the removal of the exciting radiation, the electrons are free to drop back, so that the extra conduction rapidly ceases. Light-sensitive devices based on this effect are now common.

In this article we have brushed the fringes of a number of the more important properties of semiconductors, and attempted light sketches of the explanations. Some selection has been necessary, and whole ranges of phenomena connected with the ionic type of semiconductor have been neglected, in addition to a number of electrical, thermal and optical properties of electronic semiconductors. References to the well-known works of A. H. Wilson, Mott and Gurney [14], and of Seitz [15] may be made for further information on many of these.

REFERENCES

1. J. Jaffray and M. F. Beaufort, *Comptes Rendus*, **277**, 1345, 1948.
2. C. Tubandt, *Handbuch der Experimentalphysik*, Bd. XII/1.
3. B. Gudden, *Ergeb. der exakt. Naturwiss.*, **13**, 223, 1934.
4. W. Jusé and B. W. Kurtschatow, *Phys. Zeits. Sowjet*, **2**, 453, 1932.
5. C. Bidwell, *Phys. Rev.*, **3**, 204, 1914.
6. E. E. Schneider, Science Progress, **36**, 614, 1948.
7. A. H. Wilson, *Proc. Roy. Soc.*, **A136**, 487, 1932.
8. C. A. Hogarth and J. P. Andrews, *Phil. Mag.*, **40**, 273, 1949.
9. R. H. Fowler, *Proc. Roy. Soc.*, **A141**, 56, 1933.
10. K. Shifrin, *Jour. Phys. U.S.S.R.*, **8**, 243, 1944.
11. Torrey and Whitmer, *Crystal Rectifiers*, McGraw-Hill, 1948.
12. H. K. Henisch, *Metal Rectifiers*, Oxford, 1949.
13. J. Bardeen and W. H. Brattain, *Phys. Rev.*, **74**, 230, 1948.
14. Mott and Gurney, *Electronic Processes in Ionic Crystals*, Oxford, 1948.
15. F. Seitz, *Modern Theory of Solids*, McGraw-Hill, 1940.

SOME ASPECTS OF THE BIOCHEMISTRY OF SULPHUR

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SINCE the isolation of methionine from casein, egg-albumin and wool by Mueller in 1923 [1] an ever-increasing interest in the natural occurrence and biochemical significance of organic compounds of sulphur has been exhibited by chemists. This is evidenced by the work in England and America on penicillin and on the biological methylation and demethylation of sulphur compounds [2], and in America and Switzerland on the sulphoxides occurring in garlic and radishes.

This review will deal with the work of Dr. Margaret Simpson and the author on the occurrence of a dimethylsulphonium compound in a marine alga, its possible implications, and the manner in which it has been shown to be closely related to the experiments of du Vigneaud and his colleagues on transmethylation in animals carried out at Cornell University Medical School. The important work on sulphoxides will also be reviewed. This establishes for the first time the occurrence of such compounds in nature and emphasises their close structural similarity with other longer-known natural products—and the possible biological relations which this similarity suggests.

The work now to be discussed owes much to the fundamental and long-continued researches of P. Haas on the ingredients of certain marine algæ. Thus, in 1921 he established the occurrence of " ethereal sulphates " in carrageen (*Chondrus crispus*) [3]. He showed that these consisted of potassium and calcium salts of the acid sulphuric esters of polysaccharides. Compounds of this type, already known to occur in animals, *e.g.* in cartilage, had not previously been detected in plants. The *iso*thiocyanate-glucosides which yield potassium hydrogen sulphate on hydrolysis have, as shown by Schneider *et al.* [4], an entirely different type of structure.

Neuberg and Ohle [5] then reported the presence of polysaccharide sulphuric esters in agar, obtained from certain marine algæ. Haas [6] amplified this work and in 1931 he isolated an octapeptide

of glutamic acid [7] from the red alga *Polysiphonia fastigiata*, and studied with Miss Bird [8] the constituents of the cell-wall of *Laminaria*. He was particularly interested in the red algæ and in 1933 with Miss Russell-Wells [9] showed that *P. fastigiata* also contains a polysaccharide sulphate. In 1935 he decided to investigate the nature of the odorous product evolved by this seaweed when exposed to air. He found [10] that, on aspirating air over the weed through bromine in carbon tetrachloride, lemon-yellow crystals were deposited which were identical with authentic dimethyl sulphide dibromide. The volatile matter was also passed through mercuric chloride and potassium chloroplatinite K_2PtCl_4, and the mercurichloride and platinochloride were shown to be identical with those of dimethyl sulphide. *Polysiphonia nigrescens* also evolved this sulphide. Haas pointed out that other natural sources of dimethyl sulphide are the seeds of *Brassica juncea* (Indian mustard), the first runnings of American peppermint oil and certain fractions of petroleum. It has also been reported in Réunion and African oil of geranium [11].

Shortly afterwards the author and Blackburn found [12] that, in cultures of the mould *S. brevicaulis*, dimethyl disulphide gave rise to methylthiol CH_3SH and dimethyl sulphide, in agreement with the reducing and methylating action exerted by this organism [13] on other dialkyl disulphides.

In 1942 Birkinshaw, Findlay and Webb reported that the wood-destroying fungus *Schizophyllum commune* grown on a glucose medium containing inorganic salts converts ammonium sulphate to methylthiol [14]. The author and Charlton then showed [15] that dimethyl sulphide and traces of dimethyl disulphide were also present.

There is evidence, however, that dimethyl sulphide may have a biological significance in animals, as Pfiffner and North [16] found the corresponding sulphone in the adrenal glands of cattle, and Ruzicka, Goldberg and Meister [17] describe the isolation of dimethyl sulphone from the dried blood of cattle. The quantities were small and putrefaction, leading to the decomposition of some precursor (see p. 452), was possibly not excluded. Smythe [18] considers that biological methylation of hydrogen sulphide arising from cystine followed by oxidation may explain these observations [19].

On account of the frequency with which biological methylation is encountered, as indicated by the instances cited here and in an earlier review [2] it seemed possible that the dimethyl sulphide evolved by the alga might arise by methylation of the polysaccharide

sulphates of the weed. This would involve hydrolysis, reduction and further methylation; such a process would resemble the metabolism of *S. commune* which has already been mentioned. Dr. Haas kindly agreed that an investigation of the mechanism of the process should be carried out in Leeds and it was undertaken by Miss M. I. Simpson [20]. It was found, however, that the alga exerts no methylating action on sodium arsenite, sodium selenite, or potassium tellurite when suspended in artificial sea-water containing 0·2 and 0·5 per cent. of these compounds. The dimethyl sulphide which was evolved contained no trace of trimethylarsine, dimethyl selenide or dimethyl telluride. When 1 per cent. and 10 per cent. sodium ethyl sulphate was used in the "sea-water," methyl ethyl or diethyl sulphides were absent from the dimethyl sulphide. These experiments provided, therefore, no support for the suggestion outlined above.

It appeared unlikely that the dimethyl sulphide was stored as such in the alga, and the possibility of its occurrence as a sulphonium compound suggested itself. It was, therefore, decided to attempt the identification of the precursor from which it appeared probable (*see* below) that the sulphide was eliminated by enzyme action. The alga, when received, was still attached to its host *Ascophyllum nodosum*, but was removed before investigation.

When the volatile products from the alga were aspirated through (*a*) mercuric cyanide and (*b*) mercuric chloride, no precipitate was formed in (*a*), indicating the absence of methylthiol, a conclusion in agreement with the relatively pleasant odour which is observed. The deposit in (*b*) consisted solely of the mercurichloride of dimethyl sulphide; mercurated fission products of dimethyl disulphide were absent [*see* 13, 12]. The volatile sulphur compound was, therefore, homogeneous. The dimethyl sulphide was further characterised as benzyldimethylsulphonium picrate. Addition of cold sodium hydroxide to the alga accelerated the elimination of dimethyl sulphide.

The question arises whether the sulphide evolution is an essential life-process of the alga or whether it is a sign of death or damage. The well-known occurrence of trimethylamine, probably arising from choline, in the living flowers of many trees and plants may be cited. Experiments carried out at Robin Hood's Bay, showed that when the alga, in this instance still attached to its host, was removed from the rock-pools and immediately re-immersed in a jar of sea-water, no dimethyl sulphide was evolved for at least 24 hours, though after this time a very slow production occurred, possibly due to gradual damage under abnormal conditions. On the other

hand the alga, detached from its host and immersed in tap or distilled water, evolved the sulphide in a few hours. When in the rock-pools the alga had no sulphide odour. The production of dimethyl sulphide would seem, therefore, to be an abnormal phenomenon.

Several observations suggest that an enzyme is concerned with the process. Grinding the weed with sand or immersing it in distilled water, whereby the cells are ruptured, facilitated the evolution. Haas [10] states that immersion in boiling water inhibits the production of the odour. Weed so treated and allowed to cool evolved no dimethyl sulphide in an air stream, but long boiling with water or addition of sodium hydroxide (purely chemical hydrolysis) caused a fresh evolution. When the alga was left in alcohol for a few hours, and then suspended in water, air removed no dimethyl sulphide, but addition of alkali caused a ready evolution.

Haas [10] considered it unlikely that bacterial action was involved in view of the short interval elapsing between the time of collection of the weed and the appearance of the odour, and this conclusion would appear to be confirmed by the unpublished observation of Dr. Simpson and Miss P. Fothergill that dimethyl sulphide formation still occurs when the weed is placed in water containing toluene and well shaken. In all experiments the use of a stream of nitrogen and air-free water made no difference to the result.

Identification of the Precursor of the Dimethyl Sulphide

The alga was left in alcohol for 2–3 weeks, and the extract concentrated to a syrup which yielded a brown solid on rubbing with alcohol. This, with cold sodium hydroxide, evolved dimethyl sulphide, characterised as the mercurichloride [12, 15]. The solid gave a precipitate with aqueous ammonium reineckate, $NH_4[Cr(NH_3)_2(SCN)_4]$. The reineckate ion was removed by silver sulphate, and the sulphate with barium chloride. Evaporation gave a residue which was converted into the platinichloride. This, with metallic silver, gave silver chloride and platinum, and, on evaporation as before, a solid chloride. This yielded a picrate which was carefully purified by recrystallisation from alcohol. Decomposition with hydrochloric acid finally yielded a chloride, $C_5H_{11}O_2ClS$. This was optically inactive, evolved dimethyl sulphide with cold sodium hydroxide, and was again converted into the picrate and also into the bromide, chloroplatinate, and styphnate.

Throughout this work the evolution of dimethyl sulphide on addition of sodium hydroxide was a valuable check on the purification process.

The acidity of the chloride and its non-reactivity with 2:4-dinitrophenylhydrazine excluded such constitutions as

$$(CH_3)_2\overset{+}{S}(\overset{-}{Cl}).CH_2.CO.CH_2.OH \quad \text{or} \quad (CH_3)_2\overset{+}{S}(\overset{-}{Cl}).CH(OH).CH_2.CHO.$$

Two structural formulæ appeared probable according to which the chloride would be either an α- or a β-thetin derivative (I) or (II) (*see* Brown and Letts [20*a*]).

$$\underset{(I)}{(CH_3)_2\overset{+}{S}(\overset{-}{Cl}).CHMe.CO_2H} \qquad \underset{(II)}{(CH_3)_2\overset{+}{S}(\overset{-}{Cl}).CH_2.CH_2.CO_2H}$$

The corresponding bromides (Carrara [21]) were converted into the chlorides, picrates, styphnates and platinichlorides. Mixed m.p. determinations carried out with these compounds and with the chloride from the alga and its corresponding derivatives left no doubt that the algal product is the chloride of the β-propiothetin derivative, dimethyl-2-carboxyethylsulphonium chloride (II).

Holmberg [22] states that β-methylthiolpropionic acid $CH_3S.CH_2.CH_2.CO_2H$ with $2\frac{1}{2}$ equivs. of dimethyl sulphate in cold alkaline solution eliminates dimethyl sulphide giving sodium acrylate, characterised as the addition product with benzylsulphinic acid, $Ph.CH_2.SO_2.CH_2.CH_2.CO_2H$. The same reaction occurred with the free dimethyl-2-carboxyethylsulphonium bromide.

With sodium hydroxide and toluene-*p*-sulphinic acid the synthetic β-thetin bromide (II), the chloride from the alga, and authentic acrylic acid gave the same product, *p-tolyl-2-carboxyethyl sulphone*, $CH_3.C_6H_4.SO_2.CH_2.CH_2.CO_2H$. The α-thetin bromide is more stable and only evolves dimethyl sulphide when heated with sodium hydroxide.

The carboxysulphonium chloride isolated from the alga possibly arises from a more complex derivative, which, during the process of separation, may undergo hydrolysis. The carboxyl group may, in the living alga, exist as a peptide or ester linkage and the anion may be a polysaccharide sulphate ion.

Only one other instance of the detection of a sulphonium compound in nature is recorded. Neuberg and Grosser [23] state that the precursor of the diethyl sulphide evolved on warming the urine of dogs with alkali is methyldiethylsulphonium hydroxide. They consider that this hydroxide is formed on feeding diethyl sulphide to dogs. Experimental details are very scanty and no analyses are given [*see* 20].

It is difficult to regard a methyldiethylsulphonium salt as the parent of the diethyl sulphide, because the iodide with sodium

hydroxide at 100° gives methyl ethyl sulphide, characterised as the mercurichloride.

The occurrence of sulphoxides in nature (*see* above) will be considered later. On account of their basic properties these compounds may be regarded as of sulphonium type

$$RR'SO + HX = RR'\overset{+}{S}(OH)\overset{-}{X}$$

The occurrence of a thetin in seaweed is of particular interest on account of the similarity in structure, and in many of their properties, which exists between the thetins, *e.g.* $(CH_3)_2\overset{+}{S}.\overset{\alpha}{C}H_2CO.\overset{-}{O}$, $(CH_3)_2\overset{+}{S}.\overset{\beta}{C}H_2CH_2CO.\overset{-}{O}$ and their nitrogen analogues, the betaines, *e.g.* $(CH_3)_3\overset{+}{N}.\overset{\alpha}{C}H_2CO.\overset{-}{O}$, $(CH_3)_3\overset{+}{N}\overset{\beta}{C}H_2CH_2CO.\overset{-}{O}$, etc. The α-betaines are very frequently found in plants and animals, in fact almost every natural α-amino-acid has its natural α-betaine as a counterpart, usually in the same organism. β-Amino-acids and β-betaines on the other hand are more rarely encountered among natural products. Conversely an α-thetin has not yet been detected in plants or animals.

The work of du Vigneaud and his colleagues on transmethylation in animals [*see* 2, 24] has shown that a methyl group of betaine $(CH_3)_3\overset{+}{N}CH_2CO.\overset{-}{O}$, of choline $(CH_3)_3\overset{+}{N}.(\overset{-}{O}H).CH_2CH_2OH$ and of methionine $CH_3SCH_2CH_2CH(NH_2)COOH$ is mobile and can in the case of the first two compounds be transferred to homocysteine $HS.CH_2CH_2CH(NH_2)COOH$ (thus enabling rats to maintain growth on a choline- and methionine-free diet containing homocysteine), and in the case of methionine to a choline precursor which is presumably aminoethanol $NH_2CH_2CH_2OH$. The process is enzymic [25]. Numerous reactions of betaine illustrate this mobility, *e.g.* its reversible conversion to the methyl ester of dimethylamino-acetic acid $(CH_3)_3\overset{+}{N}.CH_2CO\overset{-}{O} \rightleftharpoons (CH_3)_2N.CH_2COOCH_3$ on heating alone (Willstätter [26]) and the formation of N-alkylarylamines, *e.g.* $C_6H_5NHCH_3$, and of dimethyl sulphide, selenide and telluride $M(CH_3)_2$ on heating with aniline, sodium sulphite, selenite and tellurite (Challenger, Taylor and Taylor [27]).

The occurrence of the methyl ester of β-methylthiolpropionic acid $CH_3S.CH_2CH_2COOCH_3$ was detected in pineapple juice by Haagen-Smit *et al.* [28]. This compound is isomeric with the seaweed thetin $(CH_3)_2\overset{+}{S}CH_2CH_2CO\overset{-}{O}$ and the possibility that the two compounds are related biologically, as are betaine and methyl dimethylamino-acetate, is an attractive one [29, 15]. Attempts to

convert the ester to the thetin by heat were unsuccessful. The betaine ⇌ ester change, however, has only been observed with α-betaines. Betaines of the β-type lose trimethylamine and form acrylic acid, CH_2:CHCOOH [26]. The seaweed thetin is a β-derivative and its chloride gives acrylic acid and dimethyl sulphide with alkali, and probably also on heating. On grounds of strict analogy, therefore, the pineapple ester-seaweed thetin inter-conversion was not necessarily to be expected.

A Possible Biological Importance of the Thetins

We must now consider a rather remarkable instance of the convergence of two apparently dissimilar lines of work. When du Vigneaud discovered the mobility of the methyl group in choline, methionine and betaine he tested many other methyl derivatives, but only the rather out-of-the-way compound dimethylthetin chloride $(CH_3)_2\overset{+}{S}.(\overset{-}{Cl})CH_2COOH$ (which has not, as yet, been detected in nature) exhibited methyl mobility. Toennies [30] and Toennies and Kolb [31] suggested that sulphonium compounds, *e.g.* derivatives of methionine, might be found to play a part in biological phenomena. When the isolation of dimethyl-β-carboxyethylsulphonium chloride (β-propiothetin chloride) $(CH_3)_2\overset{+}{S}.(\overset{-}{Cl}).CH_2CH_2COOH$ from seaweed was announced, du Vigneaud and his colleagues confirmed their earlier experiments with dimethylthetin chloride and, with Dr. G. Maw [32], also showed that the seaweed thetin chloride has a mobile methyl group and will support the growth of rats on a methionine-free diet containing homocystine. These thetin chlorides are, in fact, more active than betaine or choline in this respect. Methylethylthetin chloride is slightly active; the diethyl derivative is inactive.

At the same time Dubnoff and Borsook, working in the California Institute of Technology at Pasadena, demonstrated the capacity of the two thetin chlorides to methylate homocysteine to methionine using liver and kidney preparations of rats, guinea-pigs and hogs [33].

The enzyme "dimethylthetin transmethylase," which was partially purified by fractional precipitation with alcohol, is absent from preparations of muscle, pancreas and spleen. It is distinguished and separable from the accompanying "betaine transmethylase," as the latter is destroyed at *p*H 4. The transmethylation is independent of oxygen and is not inhibited by azide, cyanide, arsenate or arsenite. Only one methyl group is transferred from dimethylthetin when homocysteine is in excess; in

agreement with this finding, methylthiolacetic acid CH_3SCH_2COOH is inactive. Dimethylthetin will not methylate glycocyamine. It is 10–20 times more effective than betaine in transmethylation; the seaweed thetin is also more effective.

Dimethylthetin could not be detected in various animal tissues. Liver and kidney preparations were freed from the betaine transmethylase, leaving the thetin enzyme unaffected. No increase in methionine formation occurred on adding homocysteine, thus suggesting the absence of dimethylthetin from the tissues. It would be interesting to know whether the enzyme can catalyse the reaction

$$(CH_3)_2\overset{+}{S}.CH_2CO\overset{-}{O} \rightarrow CH_3S.CH_2COOCH_3.$$

Thetin Salts and Mould Cultures

From a consideration of these results it would appear almost certain that the breakdown of the dimethyl-β-propiothetin salt in *Polysiphonia fastigiata* is due to enzyme action. It therefore became of interest to study the behaviour of this and other thetin salts with different organisms [33*a*] and *S. brevicaulis* and *Penicillium notatum* were chosen. The seaweed thetin chloride and bromide were found to be almost unchanged in bread cultures of the first mould, only a 1–2 per cent. yield of dimethyl sulphide being obtained. With *P. notatum* and the bromide, however, the yield was 36 per cent. The isomeric dimethyl-α-propiothetin bromide and chloride $(CH_3)_2\overset{+}{S}(\overset{-}{X})CH(CH_3)COOH$ gave no dimethyl sulphide in cultures of *S. brevicaulis* and *P. notatum* respectively. Dimethylthetin bromide was also inert to both moulds, but diethylthetin bromide $(C_2H_5)_2\overset{+}{S}.(\overset{-}{Br}).CH_2COOH$ rapidly evolved diethyl sulphide, the yield being 25 per cent. The sulphides were characterised and weighed as the mercurichloride in each case. In blank experiments it was conclusively shown that, where a sulphide was evolved, this was not due to the culture becoming alkaline by formation of ammonia. Moreover, the only thetin salts which were found to yield di-alkyl sulphide with cold 2N-alkali were those of the seaweed thetin. These results are difficult to interpret, and the experiments are being continued along with an investigation of the behaviour of alkylthiol-acids $RS(CH_2)_xCOOH$ in the cultures. β-Methylthiolpropionic acid gives both methylthiol and dimethyl sulphide in cultures of *S. brevicaulis* but not with *P. notatum*.

Possible Origins of the Thetin in Seaweed

The seaweed thetin could arise from methionine

$$CH_3S.CH_2CH_2CH(NH_2)COOH$$

by oxidative deamination, loss of one carbon atom and further methylation. Cysteine has the same carbon chain and might yield the thetin through such stages as $CH_3S.CH_2CH(NH_2)COOH$, $CH_3SCH:CH.COOH$ and $CH_3SCH_2CH_2COOH$. Maw and du Vigneaud [32] found, however, no evidence of the conversion of methylthiolacetic acid to the aceto-thetin in the animal body as, unlike the thetin, it was found to be unable to support the growth of rats on a diet deficient in methionine but containing homocystine. The same possible objection applies to the suggestion made by a colleague of the author that the betaine of β-alanine $(CH_3)_3\overset{+}{N}.CH_2CH_2CO\overset{-}{O}$ might lose trimethylamine, giving acrylic acid $CH_2:CHCOOH$, which by addition of methylthiol and further methylation could yield the seaweed thetin.

Mobility of a Methyl Group in Dimethylthetin

As the mobility of the methyl group in betaine can be demonstrated both by chemical and biochemical evidence it became of interest to determine whether the same is true of the thetins and this has now been established for the aceto-thetin by some recent experiments of Miss P. Fothergill in the author's laboratory. When the "anhydrous" thetin $(CH_3)_2\overset{+}{S}.CH_2CO\overset{-}{O}$ is heated with aniline, *p*-toluidine or β-naphthylamine, dimethyl sulphide is evolved and the N-methyl derivatives of these bases are formed as in the betaine experiments [27]. These secondary amines were characterised as acetyl-N-methylaniline, N-nitroso-N-methyl-*p*-toluidine and N-nitroso-N-methyl-β-naphthylamine. In the case of aniline and *p*-toluidine, diphenylurea and di-*p*-tolylurea were also isolated. These reactions are still under investigation.

n-Propylthiol, a New Ingredient of Onions

From the work described so far it is clear that a closer investigation of sulphur compounds of natural occurrence might produce interesting results. A preliminary investigation of the onion by the author and Mr. D. Greenwood [34] has shown that *n*-propylthiol $CH_3CH_2CH_2SH$ is present in the volatile products from freshly chopped bulbs.

Three separate batches, 5–6 kilograms in all, were peeled and chopped immediately before use. Purified and sterile air was passed through the onions and through aniline to remove any allyl *iso*thiocyanate as a thiourea derivative, and through dilute hydrochloric acid to eliminate aniline. The air then entered two tubes of 4 per cent. aqueous mercuric cyanide to remove thiols,

and finally two tubes of 3 per cent. aqueous mercuric chloride which absorbed the sulphides and disulphides which pass unchanged through mercuric cyanide. A precipitate in the cyanide formed during 2–3 days. After recrystallisation from alcohol, this was found to be pure mercury di-*n*-propylthiol. It depressed the m.p. of mercury di-*n*-allylthiol.

n-Propylthiol has not until now been detected in nature, although Challenger and Rawlings [13] observed its formation from di-*n*-propyl disulphide in cultures of *S. brevicaulis*. It may be identical with a very volatile compound from onions mentioned by Walker, Lindegren and Bachmann [35]. Kohmann [36] reports the presence of propionaldehyde in onions from evidence based on the melting-points and analyses of the 4-nitro- and 2:4-dinitrophenylhydrazones. It seems possible that, during the distillation of the onions under reduced pressure in presence of water at 50° which Kohmann employed, propionaldehyde might have arisen from propylthiol by the following series of reactions:

$$CH_3.CH_2.CH_2.SH \xrightarrow{O_2} (CH_3.CH_2.CH_2S—)_2 \xrightarrow{H_2O}$$

$$CH_3.CH_2.CH_2SH + \underset{\text{propylsulphenic acid.}}{CH_3.CH_2.CH_2.SOH} \xrightarrow{—H_2S} CH_3CH_2CHO$$

This type of reaction was observed by Schöberl [37] with many disulphides under mild alkaline conditions and by Challenger and Rawlings [13] with diethyl disulphide at 210°.

The literature contains no mention of the occurrence of alkylthiols in oil of onions, but Semmler [38] states that traces of a thiol accompany the divinyl sulphide which he isolated, after treatment with metallic potassium, from the oil of *Allium ursinum*, the crow onion or ramsons.

Professor Meirion Thomas, F.R.S., kindly informed the authors that in 1924 he examined the volatile product obtained by the distillation of onions in steam after storing (*a*) in air and (*b*) in absence of air. He treated the distillate with "dimedone" (3:3-dimethylcyclohexane-1:5-dione) obtaining crystalline derivatives. We examined these specimens which, when combined and recrystallised, were found by m.p. and mixed m.p. determinations to be identical with the "dimedone" derivative of propionaldehyde, in confirmation of the work of Kohmann.

Diallyl Disulphide and Cultures of *Scopulariopsis brevicaulis*

In view of the interest attaching to the origin of the diallyl disulphide in garlic it seemed desirable to examine the metabolism

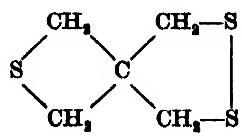
of this compound under biological conditions where chemical change could readily be detected. Consequently a study of its fate in cultures of the mould *S. brevicaulis* was undertaken.

The disulphide was found to undergo fission with formation of allylthiol characterised as the mercury, lead and silver derivatives. Methylation also occurred and methyl allyl sulphide was produced and characterised as the mercuri- and platinichlorides. This is in agreement with the behaviour of saturated disulphides R_2S_2 (where R is methyl to *n*-amyl) in cultures of *S. brevicaulis*, which yield the alkylthiol RSH and the alkyl methyl sulphide $R.S.CH_3$. Challenger and Ellis [39] found that allylarsonic acid in cultures of the mould gives dimethylallylarsine, thus showing that the allyl group when attached to arsenic is not reduced under these conditions. The allyl group in diallyl disulphide is also stable.

It appeared possible that the methylative fission of the disulphide group, now thoroughly established in the case of the mould *S. brevicaulis*, might have its counterpart in the metabolism of the higher plants and animals. Consequently in organisms where an alkyl disulphide has been recognised the corresponding alkyl methyl sulphide might be expected to occur, *e.g.* methyl allyl sulphide in members of the genus *allium*. So far, however, such derivatives have not been detected, except in the case of more complicated compounds such as methionine and the sulphones, sulphoxides and *iso*thiocyanates which are related to it or to homomethionine (*see* p. 464).

Structure of Diallyl Disulphide

The structure of diallyl disulphide is, like that of most simple disulphides, regarded as of the type R—S—S—R. The fact that Semmler [40] obtained diallyl sulphide $(CH_2{:}CHCH_2)_2S$ by heating the disulphide with zinc dust, a reaction which Greenwood has confirmed, might suggest, however, that the structure was of the type $R_2S{:}S$. Compounds such as



have been described by Backer and Evenhuis [41] and their structure, which is an unusual one, established beyond doubt.

If diallyl disulphide possessed the $R_2S{:}S$ structure it should be inert in cultures of *S. brevicaulis* or yield hydrogen sulphide and allyl sulphide. The hydrogen sulphide might be methylated to

methylthiol or to dimethyl sulphide, neither of which could escape detection. (Experiments in which hydrogen sulphide water was added to bread cultures of *S. brevicaulis* gave no evidence of such methylation.) The symmetrical structure has now been established by Greenwood [42] who found that diallyl disulphide and cysteine hydrochloride in neutral aqueous solution in a slow stream of nitrogen yield cystine, which was deposited, and allylthiol $CH_2{:}CHCH_2SH$ which was carried forward into mercuric cyanide and characterised as mercury diallylthiol. The yields of cystine and of thiol were over 95 per cent. in each case. On the unsymmetrical formula hydrogen sulphide and allyl sulphide might conceivably arise but, even assuming the subsequent reductive fission of the allyl sulphide, not more than a 50 per cent. yield of allylthiol calculated on the disulphide could have been obtained. No hydrogen sulphide was observed. Dimethyl disulphide readily reacts in a similar manner with cysteine and so do the diethyl and di-*n*-butyl derivatives, but with these two disulphides the reaction requires heat, is much slower and is consequently accompanied by some decomposition of the cysteine, hydrogen sulphide being evolved.

The symmetrical structure of diallyl disulphide receives further support by the formation of allicin on oxidation of diallyl disulphide with hydrogen peroxide or perbenzoic acid (*see* p. 462). Greenwood has shown that several other reactions of diallyl disulphide, while explicable on either of the possible formulæ, are seen in an interesting light if it be assumed that the disulphide readily dissociates (in a manner already established by Schönberg for diphenyl disulphide [43]) giving the free radical $CH_2{:}CHCH_2S—$. Thus, with zinc dust at 130°, zinc sulphide and allyl sulphide are formed, triphenylphosphine gives triphenylphosphine sulphide and phenyl*iso*cyanide gives phenyl*iso*thiocyanate, characterised as diphenylthiourea. These reactions may be represented thus where $R = CH_2{:}CHCH_2—$

(*a*) $RS - SR + Zn \rightarrow (RS)_2Zn \rightarrow R_2S + ZnS$

(*b*) $RS - SR + Ph_3P \rightarrow Ph_3P(SR)_2 \rightarrow Ph_3PS + R_2S$

(*c*) $RS - SR + Ph.N{=}C \rightarrow PhN{=}C(SR)_2 \rightarrow PhN : C : S + R_2S$

The zinc compound of allylthiol decomposes at 100° according to (*a*). Reaction (*c*) was carried out in consultation with Professor A. Schönberg of Cairo.

Possible Origin of the Allyl Group in Nature

Compounds containing the allyl group frequently occur in plants of the family *Cruciferæ* and the genus *allium*. It is interesting to attempt to relate the presence of diallyl disulphide in the onion and the garlic, and of allyl *iso*thiocyanate (as a glucoside sinigrin)

in the seeds of black mustard, with the amino-acids cystine and methionine.

Borek and Waelsch [56] refer to "the formation of hydrogen sulphide from homocysteine as from cysteine by the action of tissue slices." Smythe [18] has also established the presence of an enzyme in liver which eliminates hydrogen sulphide from cysteine. Allyl *iso*thiocyanate may be formed [15] by elimination of hydrogen sulphide from homocysteine (arising from methionine) followed by decarboxylation and interaction of the resulting amine with hydrogen sulphide and carbon dioxide:

$$HS.CH_2.CH_2.CH(NH_2).CO_2H \xrightarrow{-H_2S} CH_2{:}CH.CH(NH_2).CO_2H \xrightarrow{-CO_2}$$

$$CH_2{:}CH.CH_2.NH_2 \xrightarrow[H_2S]{CO_2} CH_2{:}CH.CH_2.NCS + 2H_2O$$

The relation existing between the *iso*thiocyanates of the *Cruciferæ* (*see* Armstrong and Armstrong [44]) and the corresponding amino-acids has been pointed out by Barger and Coyne [45] with reference to methionine and cheirolin, $CH_3SO_2.CH_2.CH_2.CH_2.NCS$, which occurs as the glycoside glucocheirolin in wallflower seeds, and also to phenylethyl *iso*thiocyanate $C_6H_5CH_2CH_2N{:}C{:}S$ (from the glycoside gluconasturtiin, in the garden nasturtium) and phenylalanine, $C_6H_5CH_2.CH(NH_2)COOH$.

The existence of erysolin, $CH_3SO_2.CH_2.CH_2.CH_2.CH_2.NCS$ (found as glucoerysolin in the seeds of the bright-orange wallflower), suggests the advisability of a search for homomethionine $CH_3SCH_2CH_2CH_2CH(NH_2)COOH$ amongst the products of protein hydrolysis.

Diallyl disulphide, which occurs in oil of garlic [40], might be formed from homocystine by loss of ammonia and carbon dioxide [15], a suggestion supported by the well-known enzymic equilibrium existing between fumaric and aspartic acids,

$$[CO_2H.CH(NH_2).CH_2CH_2.S]_2 \rightleftharpoons 2CO_2 + 2NH_3 + (CH_2{:}CH.CH_2.S)_2$$

The Precursors of Diallyl Disulphide in Garlic

In 1944 Cavallito *et al.* [46, 47, 48] isolated an antibacterial substance from garlic cloves which had the composition $C_6H_{10}OS_2$ and was named allicin. The cloves were extracted with aqueous alcohol and the extract distilled with steam under diminished pressure. The authors were unable to decide between the two formulæ

$$\underset{\substack{\downarrow\\ O}}{CH_2{:}CH.CH_2S}\text{—}S.CH_2.CH{:}CH_2 \quad \text{and} \quad (CH_2{:}CH.CH_2.S)_2O$$

but preferred the first. No other compound of sulphoxide type had hitherto been isolated from a natural product with the possible exception of $\beta\beta'$-dihydroxydiethyl sulphoxide (Reichstein [54]). Stoll and Seebeck [59] then showed that oxidation of diallyl disulphide with hydrogen peroxide in glacial acetic acid gave an oil having the chemical and antibacterial properties of allicin, which should therefore be assigned the sulphoxide structure. These authors then isolated an amino-acid from garlic which they named alliin and which they showed to be a precursor of allicin. This was isolated by a fairly simple process. Garlic cloves were mixed with solid carbon dioxide to prevent enzyme action, thoroughly ground and extracted with methyl or ethyl alcohol containing about 10–15 per cent. of water. The alcohol was removed *in vacuo* and the aqueous residue freed from fat by ether. Other material such as carbohydrate was removed by repeated fractional precipitation of the aqueous solution with methyl alcohol, when the alliin was obtained as optically active crystals. Potentiometric titration, the ninhydrin reaction and a van Slyke determination established that alliin was an amino-acid. Catalytic hydrogenation showed the presence of a double bond. Cysteine was oxidised to cystine and hydrogen sulphide to sulphur. Sodium metabisulphite $Na_2S_2O_5$ removed an atom of oxygen forming desoxo-alliin, which was shown by its reactions, and by synthesis from cysteine and allyl bromide in presence of mercuric chloride, to be S-allylcysteine. Alliin itself is therefore S-allylcysteine sulphoxide.

Alliin has no antibacterial properties. In a later communication [60] the authors describe the isolation and properties of an enzyme from garlic which they name alliinase. With alliin it yields allicin, pyruvic acid and ammonia thus:

$$2CH_2{:}CH.CH_2\underset{\substack{\downarrow\\ O}}{S}.CH_2CH(NH_2)COOH + H_2O$$

$$CH_2{:}CH.CH_2.\underset{\substack{\downarrow\\ O}}{S}\text{—}S.CH_2CH{:}CH_2 + 2CH_3COCOOH + 2NH_3$$

It is suggested that alliin first breaks down to give (*a*) allylsulphenic acid $CH_2{:}CH.CH_2S.OH$, (*b*) the corresponding sulphoxide

$$CH_2{:}CH.CH_2\underset{\substack{\downarrow\\ O}}{S}.H$$

and (*c*) two molecules of α-amino-acrylic acid $CH_2{:}C(NH_2)COOH$. Interaction of (*a*) and (*b*) is believed to give allicin and water, while (*c*) with water gives pyruvic acid and ammonia. Finally allicin was shown on distillation with steam to yield diallyl disulphide.

The oxygen acceptor in this reaction has not yet been identified. Minced fresh garlic behaves in a similar manner. The following scheme is tentatively, and without insistence on details, suggested by the author as a possible explanation of the origin of alliin in garlic and involves the formation of cystathionine:

$$\underset{\text{Methionine}}{CH_3S.CH_2.CH_2.CH(NH_2)COOH} \longrightarrow \underset{\text{Homocysteine}}{HSCH_2CH_2CH(NH_2)COOH}$$

$$+ \underset{\text{Serine}}{HOCH_2CH(NH_2)COOH} \xrightarrow[\text{(see p. 459)—HOH}]{\text{as in rat liver extract}}$$

$$\underset{\text{Cystathionine}}{COOH.CH(NH_2)\,CH_2SCH_2CH_2CHNH_2COOH} \xrightarrow{-NH_3}$$

$$COOHCH(NH_2)CH_2SCH_2CH{=}CH.COOH \xrightarrow{-CO_2}$$

$$COOHCH(NH_2)CH_2SCH_2CH{=}CH_2 \xrightarrow{O} \underset{\text{Alliin}}{COOHCH(NH_2)CH_2\overset{}{\underset{\downarrow \atop O}{S}}CH_2CH{=}CH_2}$$

As an alternative cystathionine might yield a keto-acid COOH.CH(NH$_2$)CH$_2$.S.CH$_2$.CH$_2$COCOOH which by decarboxylation, reduction, loss of water and oxidation could yield alliin.

New Compounds of Sulphoxide Type in Radish Seeds

The suggestion [15] mentioned on p. 462 that homomethionine might occur in nature is of interest on account of the recent isolation by Schmid and Karrer [49–51] of two more derivatives of this amino-acid from the seeds of the radish (*Raphanus sativus*). These have the same C—S—C . . . C chain as erysolin, but are sulphoxides instead of sulphones and contain a C=C bond in the α-position to the S → O group. One is an *iso*thiocyanate and the other the corresponding nitrile. The two sulphoxides are optically active, owing to the presence of R.R′.S → O link, and afford the first recorded instance of a natural product of which the optical activity is solely due to such a linkage.

Homomethionine:	$CH_3SCH_2CH_2CH_2CH(NH_2)COOH$
Erysolin:	$CH_3SO_2CH_2CH_2CH_2CH_2N{:}C{:}S$
Sulphoraphene:	$CH_3\underset{\downarrow \atop O}{S}.CH{:}CH.CH_2CH_2N{:}C{:}S$
The nitrile:	$CH_3\underset{\downarrow \atop O}{S}.CH{:}CH.CH_2CH_2CN$

The fresh seeds were finely ground and extracted three times with aqueous acetone, the acetone evaporated and the residue freed

from fat by extraction with light petroleum. The aqueous solution was then treated with silver nitrate when a silver derivative was precipitated. Removal of silver with sodium thiosulphate, extraction with ether, evaporation, and distillation of the residue in a high vacuum gave an oil which, after partition chromatography using a silica gel column and chloroform-ether and water as solvents, yielded sulphoraphene. The nitrile remained in the column and was removed by methyl alcohol.

The *iso*thiocyanate, sulphoraphene, occurs in the seeds as a glucoside of the usual type.

$$RN{=}C \begin{matrix} -OSO_2OK \\ \searrow S.C_6H_{11}O_5 \end{matrix}$$

where $R = CH_3SO.CH{:}CHCH_2CH_2{-}$; the silver compound is presumably

$$RN{=}C \begin{matrix} -OSO_2OAg. \\ \searrow SAg \end{matrix}$$

The hydrolysis of such glucosides to an *iso*thiocyanate, glucose and potassium hydrogen sulphate is well known. The authors consider that the nitrile also arises by decomposition or transformation of this glucoside; the mechanism is obscure. They point out that cases are already known in which *iso*thiocyanates and nitriles have been isolated from the same plant. Thus steam distillation of *Lepidium sativum* and *Tropæolum majus* yields benzyl*iso*thiocyanate and benzyl cyanide and from *Nasturtium officinale* phenylethyl*iso*thiocyanate and phenylethyl cyanide are obtained by the same process. Using the thoroughly disintegrated plant the *iso*thiocyanate predominates, otherwise the nitrile is the chief product. This would suggest the normal enzymic decomposition as a source of the *iso*thiocyanate, whereas steam distillation of the more or less intact plant may decompose the enzyme before it can react with the glucoside, which may then break down in a different manner. The simple conditions under which the nitrile and *iso*thiocyanate are produced in these three cases would suggest that the rather more drastic method of formation and decomposition of a silver salt in the experiments of Schmid and Karrer [50] was not the cause of nitrile formation. The observations on these three plants date from 1874 and 1889 and the matter might repay further investigation. A direct transformation of *iso*thiocyanate by loss of sulphur and isomerisation of the resulting *iso*nitrile seems improbable, although *iso*nitriles are known to yield nitriles on heating.

Dent [52, 53] has reported the detection of methionine sulphoxide

in urine by means of paper chromatography. It is however not clear whether this compound is present as such because, at the great dilutions employed in this method, methionine can undergo atmospheric oxidation to the sulphoxide.

Reichstein [54] found $\beta\beta'$-dihydroxydiethylsulphoxide

$$(HOCH_2CH_2)_2S \rightarrow O$$

among the products of hydrolysis of the fat of the suprarenal cortex of oxen, but considers that its formation during the isolation is not excluded.

The Fission of the Monosulphide Link by Moulds and in the Animal Body

The fission of the disulphide —S—S— link by moulds [12, 13] was discussed in an earlier review [2] and has been mentioned on p. 459.

During an investigation by Dr. P. T. Charlton of the mechanism of the formation of methylthiol from inorganic sulphate by *Schizophyllum commune* [15] he studied the behaviour of methionine in cultures of this fungus and also of *S. brevicaulis*. In the first case only traces of methylthiol were evolved using well-grown wort cultures, but in the second case, using bread cultures, both methylthiol and dimethyl sulphide were evolved and detected as $(CH_3S)_2Hg$ and $2(CH_3)_2S.3HgCl_2$ respectively. Under identical conditions S-methyl-, S-ethyl- and S-*n*-propylcysteines

$$R.S.CH_2CH(NH_2)COOH$$

gave the corresponding alkylthiol RSH and methyl alkyl sulphide $RSCH_3$, which were detected in a similar manner. With S-methylcysteine Mr. A. G. Lowther finds that the same fission occurs in a liquid (Czapek-Dox) medium containing glucose and inorganic salts [55], but that methionine is hardly affected, though the addition of a sterile aqueous extract of bread crumbs causes some reaction.

The mechanism of this fission may be reductive, giving homoalanine (from methionine) or alanine (from the alkylcysteines), or hydrolytic when homoserine or serine would be formed respectively, *e.g.*

$$CH_3S.CH_2CHNH_2COOH \rightarrow (a)\ CH_3SH + \underset{\text{Alanine}}{CH_3CH(NH_2)COOH}$$

or

$$(b)\ CH_3SH + \underset{\text{Serine}}{HO.CH_2CH(NH_2)COOH}.$$

There is, however, another alternative-deamination to the α-keto acid, thus:

$$CH_3S.CH_2CH_2CH(NH_2)COOH \rightarrow CH_3S.CH_2CH_2COCOOH \quad . \quad . \quad (A)$$
$$CH_3S.CH_2CH(NH_2)COOH \rightarrow CH_3S.CH_2COCOOH \quad . \quad . \quad . \quad . \quad (B)$$

The keto-acid (A) is known to be formed from methionine by the action of liver or kidney slices [56] or in the rat [57] and (A) gives methylthiol with acids or alkalis, though (B) is stable. In cultures of *S. brevicaulis* Dr. Y. C. Liu [33*a*] finds that both (A) and (B) give methylthiol and with (B) dimethyl sulphide is also formed. Owing to the possibility that the keto-acids may yield the original amino-acids in the cultures by transamination [58], no conclusion as to mechanism can be drawn from these experiments.

In consultation with Dr. C. E. Dent and Mr. Crumpler of University College Hospital Medical School, Mr. A. G. Lowther, using two-dimensional paper chromatography, has studied the mechanism of the C—S fission of S-methylcysteine by *S. brevicaulis*. In glucose-Czapek-Dox solution containing no other added substrate the only amino-acid which could be recognised after 12 days was aspartic acid. When S-methylcysteine was added and the culture examined after the same time and under identical conditions aspartic acid was detected as before, leucine and valine in small quantity, alanine in fairly high concentration and, of course, much unchanged methylcysteine. No serine was found either in the methylcysteine cultures or the controls. This suggests that the alanine has arisen by reductive fission but the experiments were, of course, not quantitative. The alanine and the other amino-acids gradually disappear as the age of the culture increases.

Carbon-Sulphur Fission in Animals

From preliminary experiments by Mr. A. G. Lowther it would appear that homomethionine $CH_3S.CH_2.CH_2.CH_2.CH(NH_2)COOH$ does not undergo the C—S fission in bread cultures of *S. brevicaulis*. Ethionine $C_2H_5S.CH_2CH_2CH(NH_2)COOH$, however, gives some ethylthiol, characterised as the mercury derivative.

The fission of the C—SCH_3 link in methionine and the *S*-alkylcysteines has, the author believes, only one biological counterpart, namely the fission of cystathionine,

$$CO_2H.CH(NH_2).CH_2.S.CH_2.CH_2.CH(NH_2).CO_2H.$$

In presence of rat liver or its saline extracts or kidney slices, this gives cysteine and, it has been suggested, homoserine, $CH_2(OH).CH_2.CH(NH_2).CO_2H$, or its phosphoric ester. This fission is probably reversible and cystathionine plays an important part in the biological conversion of methionine into cystine in rats, a

change which has been followed by the use of cystathionine containing "labelled" sulphur and carbon. Only the sulphur appears in the cystine. References to the extensive work of du Vigneaud, Binkley, Stetten and their colleagues in this connection are given by the author and Charlton [15].

Occurrence of an Alkyl Compound of Sulphur in Quartz

Sjogrem [61] drew attention over 40 years ago to the unpleasant odour which is evolved on crushing the milk-white vein quartz found in the iron-ore fields of Salangen in Tromsö County, Norway. The smell was very penetrating, resembled that of seaweed and rapidly disappeared. It was ascribed to the inclusion of gas or a very volatile liquid in minute cavities in the quartz. Such inclusions were seen in thin sections under the microscope. Sjogrem considered that, from the appearance and properties of the inclusions (which are described), they were formed at the same time as the vein quartz itself and are thus primary in the same sense as the liquid and gas inclusions in mineral masses deposited from aqueous solutions described by Retgers [62]. A secondary origin by injection of solutions through capillary cracks brought about, for example, by mountain chain pressure seems less probable.

Sjogrem compared the odour (which was obviously that of an aliphatic compound of sulphur) with that of known substances and concluded that it was due to dimethyl disulphide or to ethylthiol. The methyl derivative seemed the more probable, owing to the frequent occurrence of methyl derivatives in nature.

In 1947 the author broke some milk-white quartz pebbles of several pounds weight which were found on the shore of Aberdaron Bay in Caernarvonshire. The geological origin of these was unknown to him, but the odour of an organic sulphur compound was most pronounced, though very transient.

From the author's experience of numerous thiols, sulphides and disulphides he identified the odour as that of either dimethyl or diethyl disulphide. Other colleagues also detected the odour. The phenomenon described by Sjogrem would therefore appear to be widespread.

Sjogrem recalls that dimethyl disulphide, or the methylthiol from which it is so easily produced on oxidation, are connected with the processes of decay. Its appearance as a primary inclusion in a quartz vein deposited from an aqueous solution in rocks of palæozoic age suggested that, even at that time, bacteria existed and caused decay of organic matter.

The author's geological colleagues have suggested that the

origin of the dimethyl disulphide or ethylthiol might be "purely chemical." Hydrocarbons such as acetylene or ethylene might react with hydrogen sulphide to give ethyl derivatives of sulphur, or carbon dioxide and hydrogen sulphide might yield dimethyl disulphide, under conditions of high temperature and pressure.

REFERENCES

1. J. H. Mueller, *J. Biol. Chem.*, 1923, **56**, 1157.
2. F. Challenger, Science Progress, 1947, **35**, 396.
3. P. Haas, *Biochem. J.*, 1921, **15**, 469.
4. W. Schneider, H. Fischer and W. Specht, *Ber.*, 1930, **63**, 2787.
5. C. Neuberg and H. Ohle, *Biochem. Zeit.*, 1921, **125**, 311.
6. P. Haas, and (Miss) B. Russell-Wells, *Biochem. J.*, 1929, **23**, 425.
7. P. Haas and T. G. Hill, *ibid.*, 1931, **25**, 1472.
8. P. Haas and (Miss) G. M. Bird, *ibid.*, 403.
9. P. Haas and (Miss) B. Russell-Wells, *ibid.*, 1923, **17**, 696.
10. P. Haas, *ibid.*, 1935, **29**, 1258.
11. *Schimmel's Reports* (Leipzig), 1896, October, 58; 1909, April, 56.
12. S. Blackburn and F. Challenger, *J. Chem. Soc.*, 1938, 1872.
13. F. Challenger and A. A. Rawlings, *ibid.*, 1937, 868.
14. J. H. Birkinshaw, W. P. K. Findlay and R. A. Webb, *Biochem. J.*, 1942, **36**, 526.
15. F. Challenger and P. T. Charlton, *J. Chem. Soc.*, 1947, 424.
16. J. J. Pfiffner and H. B. North, *J. Biol. Chem.*, 1940, **134**, 781.
17. L. Ruzicka, M. W. Goldberg and H. Meister, *Helv. chim. Acta.*, 1940, **23**, 559.
18. C. V. Smythe, *J. Biol. Chem.*, 1942, **142**, 387.
19. E. Borek and H. Waelsch, *ibid.*, 1941, **141**, 99.
20. F. Challenger and (Miss) M. I. Simpson, *J. Chem. Soc.*, 1948, 1591.

20a. A. C. Brown and E. A. Letts, *Trans. Roy. Soc. Edinburgh*, 1877–8, **28**, Pt. II, 571.

21. G. Carrara, *Gazz. chim. ital*, 1893, **23**, i, 506.
22. B. Holmberg, *Ark. Kemi Min. Geol.*, 1946, **21B**, (7), 1.
23. C. Neuberg and P. Grosser, *Centralbl. Physiol.*, 1905–06, **19**, 316.
24. F. Challenger, *Chemical Reviews*, 1945, **36**, 315; *Annual Reports of the Chemical Society*, 1946, 262.
25. H. Borsook and J. W. Dubnoff, *J. Biol. Chem.*, 1947, **169**, 247; 1948, **176**, 789; *Fed. Proc.*, March 1949, 195.
26. R Willstätter, *Ber.*, 1902, **35**, 584.
27. F. Challenger, P. Taylor and B. Taylor, *J. Chem. Soc.*, 1942, 48.
28. A. J. Haagen-Smit, J. G. Kirchner, (Miss) C. L. Deasy and A. N. Prater, *J. Amer. Chem. Soc.*, 1945, **67**, 1651.
29. F. Challenger and (Miss) M. I. Simpson, *Proc. Biochem. Soc.*, 1947, **41**, xl.
30. G. Toennies, *J. Biol. Chem.*, 1940, **132**, 455.
31. —— and J. J. Kolb, *J. Amer. Chem. Soc.*, 1945, **67**, 849.
32. G. A. Maw and V. du Vigneaud, *J. Biol. Chem.*, 1948, **174**, 381, 477; **176**, 1029, 1037.
33. J. Dubnoff and H. Borsook, *Fed. Proc.*, 1948, **7**, 152; *J. Biol. Chem.*, 1948, **176**, 789.

33a. F. Challenger and Y. C. Liu, *Proc. Biochem. Soc.*, 1948, **43**, xxxvii.
34. F. Challenger and D. Greenwood, *Biochem. J.*, 1949, **44**, 87.
35. J. C. Walker, C. C. Lindegren and F. M. Bachmann, *J. Agric. Res.*, 1925, **30**, 175.
36. E. F. Kohmann, *Science*, 1947, **106**, 625.
37. A. Schöberl, *Annalen*, 1933, **507**, 111; 1936, **522**, 97.
38. F. W. Semmler, *ibid.*, 1887, **241**, 90.
39. F. Challenger and L. Ellis, *J. Chem. Soc.*, 1935, 396.
40. F. W. Semmler, *Arch. Pharm.*, 1892, **230**, 434.
41. H. J. Backer and N. Evenhuis, *Rec. trav. chim.*, 1937, **56**, 129.
42. F. Challenger and D. Greenwood, forthcoming publication.
43. A. Schönberg, *Ber.*, 1935, **68**, 163.
44. E. F. Armstrong and K. F. Armstrong, *The Glycosides*, Longmans, 1931, 66.
45. G. Barger and F. P. Coyne, *Biochem. J.*, 1928, **22**, 1417.
46. C. J. Cavallito and J. H. Bailey, *J. Amer. Chem. Soc.*, 1944, **66**, 1950.
47. C. J. Cavallito, J. S. Buck and C. M. Suter, *ibid.*, 1952.
48. C. J. Cavallito, J. H. Bailey and J. S. Buck, *ibid.*, 1945, **67**, 1032.
49. H. Schmid and P. Karrer, *Helv. chim. Acta*, 1948, **31**, 1017.
50. —— *ibid.*, 1087.
51. —— *ibid.*, 1497.
52. C. E. Dent, *Science*, 1947, **105**, 335.
53. —— *Biochem. J.*, 1947, **41**, 240.
54. T. Reichstein, *Helv. chim. Acta*, 1936, **19**, 41, 401.
55. H. Raistrick, *Trans. Roy. Soc.*, 1931, **13**, **220**, 78.
56. E. Borek and H. Waelsch, *J. Biol. Chem.*, 1941, **141**, 99.
57. H. Waelsch, *ibid.*, 1941, **140**, 313.
58. E. Baldwin, *Dynamic Aspects of Biochemistry*, Cambridge University Press, 1948, 154, 217, 227.
59. A. Stoll and E. Seebeck, *Helv. chim. Acta*, 1948, **31**, 189.
60. —— *ibid.*, 1949, **32**, 197.
61. H. Sjogrem, *Geol. Föreningens Forhandlingar*, 1905, **27**, 113.
62. J. W. Retgers, *Z. Phys. Chem.*, 1892, **9**, 274.

THE CENTENARY OF JOHN HOPKINSON, A GREAT ELECTRICAL ENGINEER

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On July 27, 1849, there was born in Manchester one of the most famous electrical engineers of all time. John Hopkinson was the eldest of five remarkably able brothers, sons of Alderman Hopkinson who later became a partner in the firm of Wren & Bennett, Engineers, of Manchester. In this firm, John Hopkinson himself received his initial engineering training, in the vacation and after leaving Cambridge.

The main part of his schooltime was spent at Queenwood College in the New Forest, a school which early gave special prominence to Science, and which once had Tyndall and Frankland on its staff. At the age of 15½ Hopkinson entered Owens College, Manchester, of which his brother—as Sir Alfred Hopkinson—was 33 years later to become Principal. In 1867, however, Hopkinson won a Mathematical Scholarship to Trinity College, Cambridge, taking first place in the examination. Everything else followed: Senior Wrangler, Smith's Prizeman, and Fellow of Trinity in 1871. Hopkinson's son declared that his father's ambition at Cambridge had always been, simply, "to come out top." His subsequent brilliant career suggests that competitive examination is not always a false guide to ability.

Hopkinson was held by Cayley to be scarcely less capable in pure mathematics, in which his declared interest was small, than in the engineering applications of mathematics, where his heart lay. The influence which shaped the practical side of his nature, and directed the current of his life, was his family background and training. Those who think that a particular kind of technical education will produce a particular kind of human product might note that several of the most famous of all engineers were reared on the very pure diet of the Cambridge Mathematical Tripos. Minds of the power of John Hopkinson's require only stimulus and the stiffest discipline

to rise to their full stature, and when they have done so they may apply themselves in a variety of ways.

The tale of Hopkinson's career is soon told. In 1872, after a time in his father's works, he became Engineer and Manager of Chance Brothers' Lighthouse Works in Birmingham; in 1877 he set up in London in practice as a consulting engineer and as an expert witness in Patent cases; in 1881 he exhibited at the Paris Exhibition an A.C. generator of a new type, becoming consultant to the English Edison Company in this matter; in 1892 he was appointed Honorary Siemens Professor of Electrical Engineering in King's College, London. He was elected F.R.S. in 1877; most of his brilliant technical papers were published in the 1880's and early 1890's; and in 1890 and 1896 he was President of the Institution of Electrical Engineers.

The most striking feature of Hopkinson's career is that he was able simultaneously to make great contributions to electrical science and to carry on a large consulting practice on work of a very practical kind. He was consulting engineer to the Corporation of Manchester from 1891, and was there the first to introduce the now familiar type of "assessed-maximum-demand" electricity tariff. A fixed minimum charge is levied, based on installed plant, and a further charge is added, based on the electricity consumed. He also introduced the 3-wire system of electricity distribution. He was as well, at various times, consultant on electricity supply problems to local authorities in Leeds, Liverpool and St. Helens.

Hopkinson's most famous engineering paper was contributed to the *Transactions of the Royal Society* in 1886, and dealt fundamentally with the design of the field electromagnets which are part of every electrical machine. It also dealt with the important effect of the currents in the armature—the rotating part of a D.C. machine—on these electromagnets, and it introduced a method of determining the efficiency of machinery which—with some modifications—is regularly used to this day. In order to load a single large electrical generator directly, it is necessary to provide a suitable prime mover and to dispose of—usually to waste—the whole power generated. Hopkinson coupled two similar machines back-to-back, electrically and mechanically, the assembly being driven by an external drive corresponding only to the wasted power, and not to the full load power. All the experiments described in the 1886 paper were carried out at Salford Iron Works on machines made by Messrs. Mather & Platt. "Magnetic circuit: open circuit characteristic: armature reaction: regenerative efficiency tests." These phrases, now part of the stock-in-trade of all electrical machine

designers, were without clear meaning before Hopkinson had treated the subjects.

It is strange how the intellectual teasers of one generation often become the commonplaces of the next. In his paper " on the theory of alternating currents " (1884), Hopkinson says " it is a common impression that alternate-current machines cannot be worked together, and that it is almost a necessity to have one enormous machine to supply all the consumers drawing from one system of conductors." He then examined the matter and showed that a number of A.C. machines could be run in parallel without any difficulty. Forty years later most of the electrical generators in the country were " worked together " on the Grid.

In pure science, Hopkinson carried out important work on the dielectric constant and refractive index of glass, and on the residual charge of condensers with glass dielectrics. His interest in these subjects was clearly aroused by his early concern with glass as a material used in Chance Brothers' Lighthouse Works. He was also responsible for much research—mainly carried out at King's College, London—on magnetic materials, for which he was awarded a Royal Society medal. This interest in magnetic materials was, of course, allied to his interest in dynamo-electric machinery.

Electrical Engineering, of all industries, probably requires the highest average level of technical and scientific training. Hopkinson was always interested, like many other eminent electrical engineers, in engineering education. He advised the University of Cambridge about remodelling its Engineering School in 1890, and the " memorandum on engineering education " in which he did so has still a surprisingly modern flavour. On the electrical engineering curriculum in Universities the influence of Hopkinson has been, at least until recently, almost too strong. Exhaustive and specialised tests on a wide variety of electrical machinery have often been required of undergraduates, regardless of the fact that electrical machinery, though a very important branch of electrical engineering, does not comprise the whole of the subject. Students ought not to be required to carry out several forms of the Hopkinson back-to-back test, just because these experiments have the sanction of tradition. The difficulty nowadays is to compress sufficient into an undergraduate's three years. It is conceivable that in Hopkinson's day there was a dearth of developed engineering science suitable for university instruction.

One interest of Hopkinson's rouses an answering echo at the present time. He was impressed by the value of a technical corps of electrical engineers for national defence purposes, and it was his

influence which resulted in an Electrical Volunteer Corps. This first saw service in South Africa.

Hopkinson had a great passion for mountaineering, which increased its hold as he advanced in years. On August 27 1898, he was climbing with two daughters and a son at Arolla in the Alps, and a tragic fall resulted in the death of them all. Hopkinson was only 49, but he had achieved more in his short life than most men could do in any life, however long.

His eldest son, Bertram, followed in his father's footsteps, and eventually became "Professor of Mechanism and Applied Mechanics" in the University of Cambridge. By a tragic coincidence, he also was killed accidentally, in an experimental flight during the 1914–18 war. John Hopkinson is commemorated by a wing of the Engineering Laboratories at Cambridge, and by the Electrotechnical Laboratory at Manchester University, the construction of these being made possible by gifts from his family. But as long as the design of electrical machinery continues to be based on the same broad principles which Hopkinson established, so long will Hopkinson be most honoured by his reputation among electrical engineers, as one of the greatest of them all.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By MICHAEL W. OVENDEN, B.Sc., F.R.A.S., University Observatories, Cambridge.

THE last few years have seen the increasing application of the techniques of the radio engineer to astronomical research. The results already achieved show that a new and significant branch of astrophysics is in the making. I will deal in this note with two such results that seem to me to be of especial interest in their different fields, and which have brought their own problems to the observer and theoretician alike.

DISCRETE SOURCES OF RADIO EMISSION.—**1.**—In any astrophysical investigation, the observations are limited in the final analysis by the disturbing effects of the earth's atmosphere. Variations of refractive index spoil telescopic definition, scattered starlight and night sky emission fog long exposure plates, and absorption renders most of the electromagnetic spectrum useless—the astronomer surely "sees through a glass darkly." On the radiation frequency scale above 10^{15} cycles/sec. the absorption due to ozone is practically complete. Between 10^{15} and 10^{14} cycles/sec. there is a window in the atmospheric absorption, below which molecular absorptions again produce effective cut-off. Through this window passes the optical band of wavelengths and the ultra-violet and infra-red wavelengths to which astrophysical research has been confined for most of its history. At frequencies below 5×10^{10}, another window in the atmosphere appears, the "radio window," and the behaviour of radiations in this range from extra-terrestrial sources is now being studied.

2.—The possibility of receiving radiation of radio wavelengths from the sun and stars appeared very early in the literature of radio engineering. In fact, as early as 1894 Sir Oliver Lodge suggested looking for Hertzian waves from the sun. It was not until 1931 that any results were obtained, and it was from the galaxy, and not the sun, that Jansky first detected the so-called "galactic noise." That the observations were first made on the galaxy, and not the sun, was the first surprise for astrophysicists. While the radiation

in the visual range received from the sun is very much greater than that from the galaxy, the reverse is the case at radio wavelengths. Previous observations had strongly emphasised that visually the sun was a fairly typical star. It was therefore natural that the possibility of the sun being an insignificant star from the radio criterion was rejected in favour of an hypothesis invoking a mechanism of energy generation in interstellar matter. Possibilities were (*a*) that the electrons in space radiated as classical oscillators, (*b*) that the galactic noise was due to scattering by electrons in interstellar space of radiation from B-type stars, or (*c*) that it was due to free-free transitions of electrons in interstellar space, where the electrons describe open orbits round positively charged nuclei, the change in kinetic energy being radiated. This last hypothesis was generally accepted, and it is possible to interpret Jansky's results in terms of this mechanism. If such a mechanism is responsible for galactic noise, then the intensity measured in any direction will depend upon the depth of the column of interstellar gas and the transparency of the gas to the radiation concerned. When the thickness is greater than a certain critical value (depending upon the absorbing properties of the medium), then no information as to the real thickness can be obtained, but if the depth is less than this value the region will to some extent be transparent. As the transparency is a function of the frequency of the radiation, the gas being more transparent at high frequencies, the distribution of received signals at different wavelengths will give some information on the structure of the interstellar gas system. Working on 20·5 megacycles, Jansky found a preference for the galactic plane, but the poles of the galaxy were nearly as bright. The work of Sander at 60 megacycles gives a much greater galactic concentration. Recently Hey, Parsons and Phillips have examined the galactic noise at 64 megacycles with a greater resolving power than hitherto applied, and they noticed that a region in Cygnus showed rapid fluctuations of intensity in a rather irregular manner, cycles of variation occurring in less than a minute. Their aerials had a half-beam-width of 7°, and with this apparatus the fluctuations were of the order of 15 per cent. of the normal signal. It would seem impossible for a region of the sky of the order of 5° to fluctuate in a coherent manner. Hey therefore suggested that the variations might be due to a discrete, unresolved source superimposed upon the general galactic radiation.

3.—Investigation of this region was begun by Ryle in 1948, working on 200, 80 and 40 megacycles (approx.). The difficulty with such radio observations is to obtain an aerial system with sufficient resolution, while at the same time having sufficient mobility

to observe the position of the source at any part of the sky. Ryle overcame this difficulty by using an interference method. Two aerial systems, separated by a distance of the order of 100 wavelengths, give an interference pattern to the sensitivity, in which maximum sensitivity is concentrated in a few sharp lobes. The aerials are fixed, pointing to the zenith, and the character of the observed signal is examined as the sidereal time alters. If the radiation being received comes from a more or less homogeneous area larger than the angular spread of the sensitivity cone, then the signal is constant, or else slowly varies with time. If, however, a discrete source passes across the beam, periodic variations appear in the record as the source travels across the interference pattern. The time of the maximum signal gives the hour angle of the source, while the period of the variations gives the declination. By this means Ryle obtained effectively a directional aerial array with a resolution of 5–10 seconds of arc in right ascension and declination. With this apparatus, the fluctuations in the signal received from Cygnus was confirmed, and the contrast between the Cygnus signal and the general background increased, so that the amplitude of the variation was now as much as 100 per cent. A number of other "point sources" were discovered, of which about ten are comparable in intensity with the Cygnus one. Most of these show an almost constant intensity, the source in Cassiopeia being very constant.

4.—The first step in examining the physical nature of these sources was to attempt to identify them with known stars, but it proved impossible to make such identification with any object brighter than 9th magnitude. The resolution of the radio measurements is not sufficient to give an identification with any fainter object, in the absence of any criterion by which a radio-emitting body might be recognised on a photographic plate. It is not possible to examine the galactic concentration of these sources from the radio measurements. It is a peculiar limitation of the method that only isolated sources can be detected; in fact, the greater the density of the sources in a given area the less the likelihood of their being discovered. Suggestions have been put forward that the whole of the galactic noise might be due to unresolved point sources, rather than from distributed interstellar source, but Hey has expressed the opinion that his results do not favour either hypothesis, and so a combination of the two is probably nearer the truth. The distances of the objects cannot be given in absolute measure, as they are certainly greater than could be detected by parallax with present resolution. Some idea of the relative distances between one source and another is given by the frequencies

at which they make their appearance above the general galactic noise, for, as has been discussed, the transparency of space depends upon the frequency. From such measurements, it is found that the Cygnus source, visible on 60 megacycles, is much nearer the solar system than the Cassiopeia source, which does not appear at frequencies below 80 megacycles. The nature of the general galactic noise is so uncertain that more definite information on distances cannot yet be obtained. From the nature of the fluctuations of the Cygnus source, which have durations of the order of 20 seconds, and assuming that if the disturbance is due to a part of the emitting body it cannot travel across the body faster than light, Ryle has estimated that the source cannot be greater than 20 light-seconds in diameter. Apart from the uncertainties of such an estimate, the possibility cannot yet be ruled out that the variations of the Cygnus region originate outside the actual source, in the interstellar medium.

If the distance is taken to be that of the nearest fixed star, the intensity of the radiation indicates a temperature of 10^{14}°. The radiation can be compared with that of the sun in the same wavelength ranges; the radiation of the quiet sun seems to lie between about 200 and 300 times that to be expected from a black body at 6000° at 160 megacycles, but at lower frequencies observations indicate sudden enhancements of radiation to intensities corresponding to 10^{11}°, these sudden enhancements being reflected in the measurements at higher frequencies. It has been established that the enhanced radiation from the sun is connected with sun-spots, and the possibility arises that these point sources of radio emission may be faint visual stars which have the sunspot phenomenon to an exaggerated degree. The enhanced solar radiation being circularly polarised, this hypothesis could be checked by examining the polarisation of the radiation from the point sources. Ryle has found that the polarisation is random, and so could not originate in "star-spots."

5.—The problem of the generation of these radiations is a difficult one. Various suggested mechanisms, such as coherent oscillations of a large number of electrons, or electrical discharges, seem unsatisfactory, and one is led to the conclusion that the radiation must be thermal. In this case, in the absence of a magnetic field, the density at the source, at an electron temperature of 10^{14}°, cannot be greater than 10^8 particles/c.c. Conditions would be eased in the presence of an intense magnetic field, but the radiation would be circularly polarised. Further, a large discontinuity of electron density must occur in a distance of the order of the wave-

length, which would be difficult to envisage in an assembly where the mean free path is of the order of a kilometer.

Of those states known to astrophysical science, it would seem that the most likely source of radio emission would be high-temperature gaseous stellar envelopes with low visual opacity. Some support is given to this hypothesis by recent evidence that much of the enhanced solar radiation originates in the corona.

There the problem must rest, until further experimental evidence is available. The future study of the emission of radio waves from extra-terrestrial sources will bring to light new facts, and set new problems to supplement and extend the progress of visual astronomy.

Solar Flares and Long-Wave Radio Anomalies.—1.—Throughout the past decade, the attention of solar physicists has been increasingly devoted to the study of solar flares. These flares are limited regions of the chromosphere which show for short duration as a partial emission within some of the Fraunhofer absorption lines. The effect is most prominent in the Hα line, and flares are observed at this wavelength with the spectroheliоscope. Solar flares are of interest, not only to astronomers, but also to geophysicists, for flares emit, as well as radiation which causes ionisation of the earth's upper atmosphere, corpuscular beams which cause characteristic effects on the earth's magnetic field, and also possibly cosmic radiation. Solar physicists have always looked to ionospheric studies in their turn to contribute to the physical study of the flare mechanism. Recently, a new ionospheric effect of flares has been studied which is easily recognised, and which appears to bear a much more exact relation with flares than any previously known disturbance.

2.—Ionospheric studies in general are carried out in the frequency range of roughly 10 kc./s to 10,000 kc./s. Above 100 kc./s, sudden fade-outs on the reception of radio waves are found to be roughly correlated with the appearance on the sun of an intense flare. The fade-out usually recovers more quickly than the falling-off of intensity of the flare, and the correlation would not appear to be exact. Reception on frequencies below 100 kc./s. is not subject to these fade-outs, but atmospheric disturbances show marked increases at the same time as the fade-outs. Bureau has taken records of the enhancement of atmospherics over many years, and a fair correlation with intense flares is found, but, as many other factors influence the atmospherics, it is not always possible to recognise disturbances characteristic of a flare.

Before the war, while working on the reception at 16 kc./s. of

the signal from Rugby station, Budden and Ratcliffe noticed characteristic changes in the amplitude of the received signal which appeared to coincide in time in many instances with fade-outs on the shorter wavelengths. They have found that the effect is, in fact, not a change of amplitude but a change of phase. The normally received signal is compounded of two waves—one, the ground wave, being received direct from the transmitter, while the other, the sky-wave, is reflected from the D-region of the ionosphere at a height of about 75 km., where the electron density is of the order of $1 \cdot 10^6$ electrons/c.c. If the phase of the sky-wave undergoes a sudden change with respect to the ground wave, the effect on the received signal is a change of amplitude. The relative phase of the sky- and ground-wave was found to vary slowly but regularly throughout the day and night, in a repeatable manner, except for sudden changes of phase, called "sudden phase anomalies" (S.P.A.'s). These changes were equivalent to changes in height of the D-layer amounting to a few kilometres, and they had a characteristic development. They began as a very rapid rise to maximum in the course of a few minutes, followed by a much slower return to normal. The great advantage of these anomalies is that they are easily recognised, due to the quiescent behaviour of the normal D-layer. They found that, while many more S.P.A.'s were observed than fade-outs were reported, in all cases of a fade-out an anomaly of a marked character was seen. Attention therefore turned to the correlation of S.P.A.'s with solar flares, which has been examined recently by Bracewell and Straker.

3.—The correlation was found to be very high. Not only were nearly all observed flares, even faint ones, accompanied by an anomaly, but the sudden commencement of the anomaly coincided to within the accuracy of the astronomical observations with the sudden appearance or brightening of the flare. Further, the size of the anomaly was in good agreement with the intensity of the flare. The astronomical observations of flare intensity are difficult, and indeed there is great disagreement between one observatory and another on the classification of flares. Another criterion was therefore sought in examining the progress of an anomaly in the light of the development of the flare, and such a criterion was found in measures of the line-width of the flare. The original work on line-width measurements was carried out as a means of following visually the course of the flare in the absence of a satisfactory photometric method. The line-width is given by the separation in Ångströms between the wavelengths on the two sides of the maximum at which the contrast between the flare in emission and the

normal Fraunhofer line of Hα in absorption is lost. Although in its final analysis this quantity depends upon physiological powers of intensity discrimination, experienced observers do in fact give reasonably concordant results on line-width measurements. The line-width is also largely independent of atmospheric conditions. When line-widths of some bright flares were compared with the corresponding development curves for the S.P.A.'s, agreement was found to be excellent, it only requiring a change of scale to bring the two curves effectively into identity.

It therefore seemed probable that the radiation causing the anomaly was one of the lines of the same hydrogen atoms which gave rise to the Balmer series. The obvious choice was the Lyman α radiation, in the ultraviolet. However, there were few results for comparison, particularly for the beginning of a flare, which is difficult to observe in the spectrohelioscope. As evidence accumulated, it became clear that the development of the anomaly did not invariably follow exactly the line-width of the flare, the deviations being such that the phase recovered more slowly than the fading of the flare. It might be thought that some delay mechanism was operative in the D-layer, but if this were the case, the phenomenon should be more marked with flares whose intensity fell rapidly than with ones of longer duration. This is not so. Further, anomalies are registered whose total duration is less than the delay time that it would be necessary to invoke to explain extreme deviations. It would seem, therefore, that the discrepancy lies in the incident radiation. Search will be made for other lines in the flare spectrum that may show a better agreement, but it is quite possible that the relevant line is in the ultraviolet part of the spectrum absorbed by the total atmosphere of the earth. (In this connection, recent spectra of the sun taken at heights of up to 88 km. with V2 rockets are of interest. They show emission in the Mg II doublet near 2800 Å, and such radiation may be very markedly enhanced in flares.) If the anomaly-producing radiation does not originate in the same atoms as the radiation by which the flares are observed, there is the possibility that some anomalies may be produced without corresponding visual flares, and vice versa. There are a very small proportion of observed flares which do not appear to have produced recognisable anomalies, and the explanation may be along these lines. Of the reverse cases, less definite statements can be made, as the coverage of the sun by astronomical observations cannot be complete.

4.—The solar physicist hopes that this new approach will greatly add to knowledge of the sun. It should now be possible to use

observations of these S.P.A.'s to clarify the definition of a flare, for in many instances it is difficult to distinguish between a flare, and a region of bright hydrogen emission (hydrogen flocculus) with a line-width above the average, but with very little development or change. Independent of weather conditions, the phase anomalies should be invaluable in work on the statistics of flare occurrence, although they can give no indication of the position of the flare on the sun, limb flares being apparently just as effective in producing an anomaly as those having a central location. The examination of the flare radiation will lead to new developments in the physics of the flare regions, and of the chromosphere in general.

The radio physicist has already put his mark upon astronomical science, as these two examples serve to show. He has made a dramatic entry on to the stellar stage—and he is there to stay.

REFERENCES

(*a*) RADIO SOURCES:
RYLE, M., and SMITH, F. G., *Nature*, **162**, 462, 1948.
REBER, G., *Sky and Telescope*, VIII, 39, April 1949.

(*b*) S.P.A.'s:
BUDDEN, K. G., and RATCLIFFE, J. A., *Nature*, **140**, 1060, Dec. 1937.
BRACEWELL, R. N., and STRAKER, T. W., *Mon. Not. R. Astr. Soc.*, **109** (in press).

PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

THE THEORY OF THE PHOTOGRAPHIC PROCESS.—**1.**—The commercial importance and intrinsic interest of the processes leading to a developable image in silver halide emulsions have naturally stimulated much experimental work and many theories. A great deal of science and technology involved is in the fields of inorganic, organic and physical chemistry, but the fundamental processes are physical and are related to other phenomena of importance to the physicist, while the main theories put forward during the last ten years are due to physicists. The progress made, both experimental and theoretical, until 1948 has been admirably summarised by W. F. Berg (*Ann. Rep. Progr. Chem.*, **39**, 49, 1942; *Phys. Soc. Rep. Progr. Phys.*, **11**, 248, 1948). Very recently, however, there has appeared an important paper by J. W. Mitchell (*Phil. Mag.*, **40**, 249, March 1949) which modifies and extends the theory of R. W. Gurney and N. F. Mott (*Proc. Roy. Soc.*, **A.164**, 151, 1938) which has had so much influence on the work in the subsequent decade, so a review of the position as it now stands may be useful.

2.—A photographic emulsion for normal work consists of a layer of gelatine about 0·1 mm. thick containing between 10^9 and 10^{12} grains of silver halide per square cm., the grains being crystals between 0·1 and 10 μ across and on the average 1 μ apart. The absorption of only a few quanta of light in one grain causes that grain to become black on treatment with a suitable developer, the unexposed grains not being so affected. Before development an exposed grain looks no different from an unexposed one. The main problem is thus to explain how it is that the exposure of a crystal of silver halide to a few light quanta can cause it to go black when developed by a physico-chemical process which leaves the unexposed grains quite unaffected in this respect. The exposed but undeveloped grains are said to contain "latent images," and any successful theory must deal with the "photolytic process" forming the latent image, and the mechanism of subsequent development to make it visible. Our first clue is the "print-out effect" by which is meant the visible darkening of grains which have been given very prolonged exposure to light (over a million times normal). The darkening can be shown chemically to be due to metallic silver, halogen having been liberated. The blackening proceeds further if a halogen acceptor is present. As far as it has been possible to follow the process back to smaller and smaller exposures, the black specks have been found to be metallic silver, so it is natural to assume that the invisible latent image consists of minute particles of metallic silver within or on the surface of the grain.

There are many photographic "effects" (Dr. Berg lists over twenty) which any successful theory should seek to explain, but one only need be mentioned here, "reciprocity failure." The Reciprocity Law states that the quantity of reaction product resulting from the photochemical reaction of the photographic process depends only on the total quantity of light absorbed (intensity $\times$ time) and not on the individual factors, intensity and time. The product of light intensity, I and time t, is called the exposure E. For most plates, if the logarithm of the exposure, E, necessary to produce a given density of image on the plate when developed under given conditions is plotted against log I, a curve with a minimum is obtained, showing that the photographic sensitivity falls at the one extreme of very small I (long t) and the other of large I (short t). The highest sensitivity is normally for exposure times between about 0·1 and 0·01 sec., which is fortunate, since this is the range of times most commonly used. High-intensity reciprocity failure is most marked at low temperatures, and low-intensity failure at high temperatures.

Photographic emulsions are made more sensitive by a "ripening process," during which the larger grains grow at the expense of the smaller, and also, apparently, sensitivity specks are formed on the surfaces of the grains. Since the work of S. E. Sheppard and his colleagues (*Photogr. J.*, **65**, 380, 1925; *Rev. Mod. Phys.*, **14**, 303, 1942), these specks have been assumed to be silver sulphide, formed by reaction with sulphur compounds in the gelatine.

3.—Before giving the main features of the Gurney-Mott theory and of Dr. Mitchell's recent work, it will be convenient to recall some of the properties of imperfect ionic crystals. (*See* also N. F. Mott and R. W. Gurney, *Electronic Processes in Ionic Crystals*, 1940; E. E. Schneider, SCIENCE PROGRESS, **36**, 616, Oct. 1948). It must be remembered that in the very small silver halide grains involved in the photographic process, the surfaces are relatively much more important than in the larger crystals normally investigated. Most photographic emulsions contain mixed crystals, *e.g.* silver bromide with up to 7 per cent. of silver iodide, or silver bromide and chloride in various proportions. The pure halide or mixed crystals used crystallise in the cubic system, and for simplicity we will consider first a pure halide.

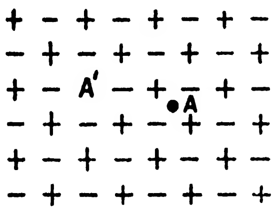


FIG 1 (*a*).—Frenkel defect.

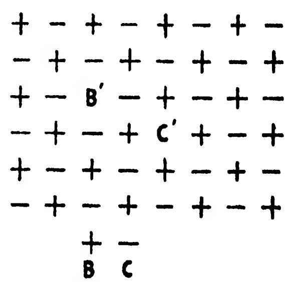


FIG. 1 (*b*).—Schottky defects.

A perfect silver halide cubic crystal consists of regular arrays of positive silver ions and negative bromide ions (for example) in alternate positions. Crystals as normally found, however, may contain "defects" of the kinds illustrated in Figs. 1 (*a*) and 1 (*b*). Fig. 1 (*a*) represents a Frenkel defect. A silver ion previously at A′ has moved to an interstitial position at A. Strictly speaking, A is half the lattice spacing above the plane of the diagram. The energy required to move an ion from a lattice site to a neighbouring interstitial position depends on the ion charges and sizes. If energy considerations permit, a halide ion may similarly move into an interstitial position. At any given absolute temperature, T, the

number of interstitial ions per unit volume in equilibrium with the lattice may be calculated by determining the conditions for minimum free energy, and is of the form

$$n = \text{Const. } (\text{NN}')^{\frac{1}{2}} \, e^{-\text{W}/2k\text{T}} \quad . \quad . \quad . \quad (1)$$

where W is the activation energy required to move an ion from a lattice site to a neighbouring interstitial position (strictly, at absolute zero), k, is Boltzmann's constant, N = number of atoms per unit volume, N′ the number of interstitial positions, and the multiplying constant is only slowly temperature dependent. Once in an interstitial position, an ion may migrate to a neighbouring one, the probability being proportional to a Boltzmann factor exp. $(-\text{W}_\text{D}/k\text{T})$, where W_D is another activation energy. This may give rise to the process of diffusion if there is a concentration gradient, and to ionic conductivity in an applied electric field. If a crystal is heated up (or, for example, prepared from a melt) and quickly cooled, Frenkel defects may be "frozen in" if W_D and W are high compared with kT at room temperature, and the number of defects actually present may therefore be much larger than given by equation 1.

Two Schottky defects are illustrated in Fig. 1 (*b*). Here, a silver ion has vacated a site at B′ and a corresponding ion is on the surface at B. Similarly there is a vacant halide ion site at C′ and an extra ion at C. There are no interstitial ions. Schottky defects originate at surfaces and the "lattice holes" diffuse inwards, giving rise to additional processes of ion diffusion and conduction. As for Frenkel defects, the number of Schottky defects in temperature equilibrium with the lattice may be calculated to be

$$n = \text{Const. N} . \, e^{-\text{W}_s/k\text{T}} \quad . \quad . \quad . \quad (2)$$

where W_s is the appropriate activation energy. To avoid internal space charges, there must normally be approximately equal numbers of Schottky defects of positive and negative ions.

For Frenkel defects, with interstitial silver ions, in AgCl and AgBr the activation energy W is 25,000 and 20,200 calories per gm. mol. respectively, so there will be a fair number of such defects in equilibrium in the crystal at room temperature, quite apart from those frozen in (equation 1). For diffusion of interstitial silver ions, the activation energy W_D is 6500 and 8200 cal. per gm. mol. respectively, indicating that the ions will be able to diffuse reasonably easily at room temperature.

4.—In their theory of the photographic process, Gurney and Mott assumed the existence of Frenkel defects, and based the

theory on the combined processes of photoconductivity and ionic conductivity. The steps in the argument are, briefly, as follows: (*a*) The absorption of a quantum of light, raising the energy of an electron attached to a halide ion so that it is in the conduction band of energies and free to move through the crystal. (*b*) In the comparatively small grain, the electron stands a good chance of reaching the surface and being captured by a sensitivity speck (*see* para. 2 above), acting as an "electron trap." (For further explanations of such terms as "conduction band," "electron trap," etc., *see* F. A. Vick, Science Progress, **34**, 562, 1946, or E. E. Schneider, *loc. cit.*) The sensitivity speck thus acquires a negative charge. (*c*) A nearby interstitial silver ion is attracted to the sensitivity speck, and combines with the electron to form a silver atom. (*d*) The process is repeated, with other electrons liberated by absorbed quanta. In this way a small speck of silver forms at the sensitivity speck, and when it is larger than a critical size is able to act as a latent image. (*e*) Each electron trapped by the sensitivity speck leaves behind a neutral halide atom. The electrons from a neighbouring halide ion may transfer its extra electron to the atom by tunnel effect, becoming an atom in turn, and so on until the atom in effect diffuses to the surface, where it may combine with a halogen acceptor in the gelatine. Thus the crystal as a whole keeps electrically neutral. (*f*) The allowed energies of electrons attached to a latent image silver speck varies with the size of the speck, and, as the number of silver atoms in it increases, the energies gradually broaden out into a narrow band. When the grain is placed in a developer which has an oxidation-reduction potential in the right range, the latent image speck may capture an electron from the developer, and so attract more silver ions from within the grain. At each sensitivity speck on the surface of the grain, silver will be pushed out from the surface of a "filament," and if the developer contains a halogen acceptor the whole grain may be converted to filaments of silver. The presence of such filaments has been confirmed by use of an electron microscope.

The Gurney-Mott theory is able to account for the breakdown of the reciprocity law. At high illuminations, the sensitivity specks will collect electrons too fast for the charges to be neutralised by an equal number of silver ions, and some electrons will be repelled by the negative charge on the speck, trapped elsewhere (*e.g.* by halide atoms) and lost as far as the photographic process is concerned. At low intensities of illumination, the electrons arrive very slowly, and it is suggested that the small "sub-image" is unstable, atoms of silver already attached having time to break away again before

further electrons and ions arrive (*see*, for example, J. H. Webb and C. H. Evans, *Photogr. J.*, **80**, 188, 1940). The Gurney-Mott theory is also able to account, in a general way, for the effect of temperature on reciprocity failure.

The original theory has been modified and extended in detail (*see* Berg, *loc. cit.*, N. F. Mott, *Photogr. J.*, **88B**, 119, Nov.–Dec. 1948), but no major change has been suggested until the March 1949 paper by Dr. J. W. Mitchell.

5.—Mitchell considers first experimental results obtained by Stasiw and Teltow published in Germany 1941–48 (*see* Mitchell's paper for a list of references), concerned with the properties of mixed silver halide crystals containing a few parts in 10,000 of silver sulphide. Detailed measurements were reported of the absorption spectra of mixed crystals subjected to different treatments. Stasiw and Teltow suggest explanations for the effects observed, but Dr. Mitchell is not in agreement with them and puts forward alternatives which lead to a revised view of the mechanism of the photographic process.

He first assumes that the ionic conductivity of silver halide crystals is due mainly to Schottky defects (of both kinds) rather than to Frenkel defects. As we have seen, Schottky defects involve vacant lattice sites. Mitchell suggests that when silver sulphide is dissolved in a halide crystal, the sulphur goes into a vacant halide site as a singly charged sulphur ion, the other electron being trapped in another vacant negative ion site to form what is called an "F-centre" (*see* Mott and Gurney, *loc. cit.*; Schneider, *loc. cit.*). The addition of silver sulphide would, in this case, make little difference to the ionic conductivity of the crystal, and this is confirmed experimentally. F-centres are, of course, neutral. An F-centre may migrate by the movement of the associated electron to an adjacent vacant negative ion site by tunnel effect. If two F-centres become close to each other, they interact such that the electrons move in lower energy states. These states become lower still if additional F-centres are added to form a linear, two- or three-dimensional aggregate. Aggregates of F-centres therefore form deep traps which may capture electrons present in the conduction band and become negatively charged. The negative charge of an F-centre aggregate with trapped electron may be neutralised by a vacant negative ion site approaching by diffusion (really diffusion of electrons the other way, as indicated above) thus increasing the proportion of silver. The deeper the trap, the less likely is the extra electron to enter the conduction band by acquiring energy thermally.

In Mitchell's view, the F-centre aggregates, formed during the "ripening process," are the sensitivity specks, and may be inside the grain as well as on the surface. When light quanta are absorbed electrons in the isolated F-centres are excited, may acquire extra energy thermally from the lattice and so enter the conduction band. The electrons may then be captured by the aggregates, which can increase in size by the process just described. Below a certain critical size they are assumed to be unstable (forming the unstable latent sub-image), but above that size are stable and normally positively charged (by association with vacant negative ion sites which have attached themselves by diffusion). The stable sub-image is then converted to a developable latent image by capturing electrons released by the absorption of quanta, increasing in size until, at a second critical size, it breaks away to form a colloidal silver speck. This, if near the surface, can grow in the developer in the same way as Gurney and Mott described.

The essential differences introduced by Mitchell are thus the replacement of Frenkel by Schottky defects, the rôle of the silver sulphide, the nature of the sensitivity specks and of the sub-image. His paper will undoubtedly stimulate further experimental and theoretical work.

METEOROLOGY. By P. A. SHEPPARD, B.Sc., F.Inst.P., Imperial College of Science and Technology, London.

THE EXPLORATION OF THE UPPER ATMOSPHERE.

1. SCOPE OF REVIEW

THE purpose of this review is to present information on the techniques which have been developed or extended for the exploration of the upper atmosphere since about 1939 and to give a descriptive account of some of the observations. It is doubly convenient to take 1939 as our starting-point; in the first place because the present series of "Recent Advances" in meteorology has aimed at reporting progress in the various departments of the subject since about that time, and, secondly, because a joint discussion on "The Upper Atmosphere" by the Chemical Society, the Physical Society and the Royal Meteorological Society was held and reported upon (Paneth *et al.*) in 1939. That report covered all current aspects of the subject except of temperature at high levels as inferred from spectroscopic data.

In the meantime a general work, *The Upper Atmosphere*, by S. K. Mitra (1947), has appeared, while *Reports of the Gassiot Committee*, by S. Chapman and others (1943), on the radiative

properties of the atmosphere, made a substantial contribution to that part of the subject. Some reference to the latter has already been made in the January 1947 article on Meteorology in SCIENCE PROGRESS, **35,** 87.

In the present article we shall treat the upper atmosphere as that part lying above the tropopause. This surface slopes upwards from about 8 km. over the polar regions, except as noted in § 12.1 below, to about 18 km. over equatorial regions, and marks off the troposphere of mainly well-stirred atmosphere beneath from the stratosphere, whose lower part is statically stable though probably not unstirred, above. It is common practice to designate the atmosphere above about 80 km. as the ionosphere.

2. TECHNIQUES OF EXPLORATION

It is convenient to begin with a list (Table I) of techniques which are being used for the exploration of the upper atmosphere. They are of two kinds, direct and indirect, and are classified accordingly in Table I, which also indicates the approximate range of height covered by each method and the nature of the data available.

TABLE I

TECHNIQUES OF EXPLORATION, APPLICABLE HEIGHTS, AND DATA OBTAINABLE

	Technique.	Height Range (km.).	Data Obtainable.
Direct.	Balloon	0–30	p, T, H,* wind, composition, solar spectra
	Aircraft	0–14	As balloon.
	Smoke shell	0–30	Wind.
	Rocket	0–160	p, T, composition, solar spectra.
	Mother-of-pearl cloud	21–29	H.
	Noctilucent cloud	78–84	Wind, thermal stratification.
Indirect.	Abnormal audibility	10–80	T, wind.
	Meteor trails	40–150	T, wind, thermal stratification.
	Atmospheric tides	0–300	T.
	Intensity of zenith twilight	20–60	Density (hence T).
	Spectroscopy—solar u-v (O_3)	0–50	T, composition.
	solar i-r.	?	Composition.
	night sky	60–1000	T, composition.
	aurora	80–1100	T, composition.
	Radio-wave reflection	80–400	p, T, wind.

* p = pressure, T = temperature, H = humidity.

The distinction between direct and indirect methods of exploration is to some extent arbitrary. Thus, for example, meteor trails provide direct evidence on wind but the temperature in the meteor layer is inferred; or again, a direct method of sounding may

provide information on solar spectra from which data on the atmosphere above the highest level reached by the sonde may be derived. The solar spectrum appears in the column for data as well as for technique, since in addition to providing evidence on the constitution of the atmosphere it is itself of prime meteorological importance in regard to the amount and distribution of energy passing through the atmosphere.

Of the techniques listed in Table I, two only are rather new, the high-altitude rocket and the determination of density from the intensity of zenith twilight. The latter was indeed investigated first by Hulbert in 1938, whereas the former represents a post-war use of the German V2 weapon, from which tremendous geophysical profit should accrue.

3. Balloon Sounding

Before the war the balloons used for atmospheric sounding usually burst below about 20 km. (50 mb.) but exceptionally reached 30 km. (10 mb.). New types of balloon are, however, now coming into use which may be relied upon to reach 30 km. with fair regularity, and the results of one year's soundings to this level over New Mexico have recently been reported upon by Gutenberg (1949). Such observations will be of great importance when made systematically over a range of latitude, for, though the general form (quasi-isothermal) of the temperature, height curve is already known to this level, the horizontal gradients of temperature are not known and these are directly related with the wind systems aloft. But soundings to such levels call for improved methods of pressure measurement, for the errors in aneroids at 10 mb. are comparable with the pressure to be measured. E. Regener (1941) has described an improved barometer, without hysteresis or temperature coefficient, consisting of a Bourdon tube of quartz glass. It is, however, rather expensive to manufacture and it is not known whether to date it has been at all widely used. Efforts have also been made in Germany towards the development of a hypsometer for high-level pressure measurement—at 50 mb. the change in boiling point of water is 1° C. per 2·9 mb. and at 10 mb. 1° C. per 0·7 mb., so that adequate sensitivity, roughly constant on a linear height scale, is available.

Hardly less important for the extension of balloon sounding to great heights is the elimination of radiation error from the thermometers employed. All presently used radio sonde thermometers probably over-indicate the air temperature at high levels in sunshine by several degrees, thus making comparisons in space

and time at these levels rather uncertain. Brasefield (1948) has shown that a small thermistor bead covered with a highly reflecting (90 per cent.) white paint, exposed directly to the air and sun, gives a much truer value of air temperature aloft in sunshine than the more conventional screened thermometer—an error as small as 0·5° C. at 20 mb. is claimed.

4. Aircraft Sounding

Although the ceiling of modern aircraft is much below that of the best balloons the advantage of space, weight-carrying capacity and a human observer gives the aircraft a continuing high importance in the exploration of the lower stratosphere. During the period under review it has been used by Dobson *et al.* (1946, 1948) to provide the first reliable measurements of the humidity of the lower stratosphere up to 12 km. Measurements of the carbon dioxide content were also made on some flights. A frost-point hygrometer operating down to about 190° K. was specially developed for the work, the error in the measurement being about $\pm$ 0·5° C. in the laboratory and somewhat greater in flight, where spatial variation in the humidity makes a lower accuracy acceptable.

It seems likely that valuable spectroscopic data may be obtained on high-altitude aircraft flights, but no results appear to have been published so far.

5. Rocket Sounding

A number of accounts of the way in which German V2 rockets have been adapted for atmospheric exploration in U.S.A. have been published, of which Krause (1947) and Newell (1947) may be mentioned. The rocket is tracked by radar so as to obtain its height and velocity throughout flight, and a graph of these quantities for one flight is shown in Fig. 1. The velocity is important as the air temperature is evaluated from it and other measured quantities. Much of the information is telemetered to the ground during flight while the successful recovery of recording instruments is ensured by bursting the warhead towards the end of the flight and allowing the instruments to land by parachutes.

Atmospheric pressure down to 10^{-3} mm. Hg is measured at an orifice on the side of the rocket where the ambient pressure prevails. An aneroid is used for pressures down to 1 cm. Hg (13 mb.) and a Pirani gauge for pressures between 2 cm. and 10^{-3} mm. Philips and ionisation gauges are used in the nose for pressures between 10^{-3} and 10^{-5} mm., the reduction to static pressure being

made through Taylor and Maccoll's theory of compressible flow at the nose of a projectile.

The temperature of the air is obtained by one of two methods, either (*a*) from the slope of the pressure–height curve using the equation of hydrostatic equilibrium, or (*b*) from the ratio of the pressure at the stagnation-point on the nose of the rocket (so-called ram pressure) to the static pressure. The former calls for consider-

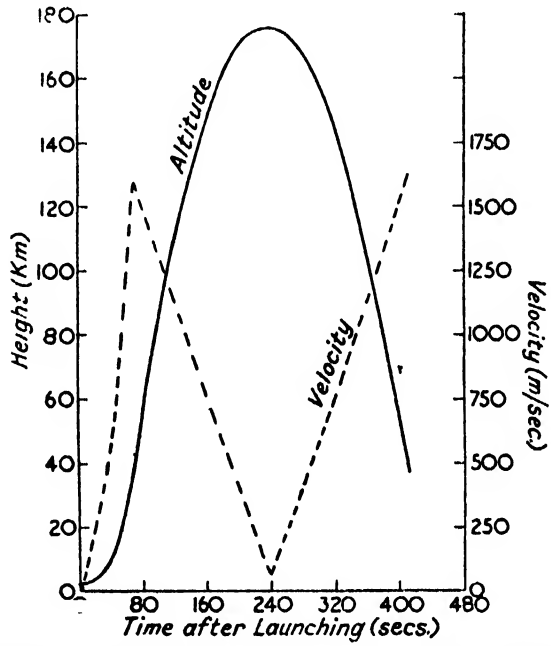


FIG. 1.—Velocity and altitude curves for rocket flight on March 7, 1947. (*Redrawn from* KRAUSE.)

able accuracy in pressure measurement, and the second is therefore favoured. The ram pressure is a function of Mach number (ratio of projectile velocity to velocity of sound) so that, knowing the velocity of the projectile, the velocity of sound and hence the air temperature is inferred. Probable errors of measurement are assessed at $\pm$ 25° C. from 50 to 60 km., $\pm$ 15° C. from 65 to 70 km. and $\pm$ 20° C. at 72 km.

The composition of the air may be determined by chemical sampling (no results have yet been reported) or spectroscopically, solar spectre having been obtained up to 88 km. altitude.

6. Meteor Trails

Lindemann and Dobson first used meteor trails in 1923 to infer the rather high temperature at the levels of their appearance and

disappearance. More recently F. L. Whipple (1943) has extended the method to determine the temperature from visual and photographic observations at four points along a trail, namely at the start, at the end, at the point of maximum brilliance and at any point of measurable deceleration. Whipple's analysis is somewhat tentative and involves several rather uncertain assumptions, but his results, for levels between about 60 and 110 km., are certainly to be considered with the other indirectly determined values in this region, direct determinations being still very few.

The drift and distortion of long-lived meteor trails provide direct evidence on the winds and turbulence at those levels, an optical technique having been developed by Störmer (1939) in Norway, Olivier (1942) in U.S.A., and Hoffmeister (1946) in Germany and South-West Africa, for a level of about 120 km. Measurements can now also be made on meteor trails by radar methods (Hey and Stewart, 1947), so that observations may become possible by day as well as at night.

7. Intensity of Zenith Twilight

The brightness of the zenith sky after sunset at the ground is mainly due to sunlight scattered vertically downward by the illuminated atmosphere above the observer, the layer of scattering atmosphere receding upwards as the sun sinks lower. The intensity of the light is proportional to the density of the air and the temperature may therefore be deduced. Multiple scattering introduces errors when the highest layers only are illuminated so that the method is limited to levels up to about 60 km. Hulbert (1938, 1947) has developed the method and reported on observations at 39° N. and 7° S. in the western hemisphere.

8. Spectroscopy

Recent spectroscopic data come mainly from well-established though often improved methods of observation. Of special interest is the extension of the solar spectrum into the far infra-red (to about 24 μ) by Adel and Lampland (*see* Sutherland and Callendar, 1943, for full references), from which evidence is accruing for the existence, possibly in significant amount, of polyatomic gases other than H_2O and CO_2 in the atmosphere. The method is not yet capable of locating the levels at which such gases occur.

The relatively recent availability of photoelectric multiplier tubes gives an exciting prospect of much wider knowledge on the temporal and spatial variations of atmospheric ozone as inferred from ultra-

violet spectra. It was reported at the 1948 Oslo meeting of the International Union of Geodesy and Geophysics that observations may be possible using the moon or stars as source, to provide diurnal variations and, let us hope, the variation during the polar winter.

9. Radio Wave Reflection

The inference of temperature at ionospheric levels from radio wave reflection dates from the early 1930's. This line of investigation continues and to it is now added the measurement of winds in the same levels (if the motion of such tenuous air may be appropriately designated wind). Fennell (1944) has reported on the motion of radio reflecting " clouds " of high ionic density in the F region and Munro (1948) and Beynon (1948) have recently added to such information. There is at present, however, some doubt as to the true nature of the apparent motions; they may be horizontal translations of ion clouds or perhaps wave motions in which changes in ion concentration are propagated by means of a pressure wave.

Other techniques listed in Table I call for no special comment and we shall now proceed to an examination of the results.

10. Results. Composition of the Atmosphere

Dealing first with the so-called permanent gases of the atmosphere, evidence is claimed for a very slight diffusive separation of helium between 20 and 25 km. (Paneth, 1939) and of oxygen between 20 and 29 km. (Regener, 1940), the upper level in each case being the limit of sounding. But this stratum is by no means free from wind (see § 13) and though the atmosphere is there statically stable some turbulent mixing may be expected, at least occasionally. Further, many months are required to establish diffusive equilibrium following mixing so that stronger evidence is required for its existence. Should it be real then it may be expected to occur up to the limit, about 50 km., of the first stable layer (see § 11) but hardly in the layer of lapse above. Moreover, the suggested separation is too small to have any significant effect on the apparent molecular weight which is involved in the pressure-height relation.

The spectra of the night sky and the aurora (*see* Chapman *et al.*, 1948, for a very complete symposium on these spectra) suggest that a change in composition first occurs at about 80 km. where molecular oxygen begins to give place to atomic oxygen, the transformation being complete according to Penndorf (1942) at about 120 km. The atomic oxygen is due to dissociation of O_2 by solar ultra-violet and when the radiation is absent, at night, recombination might

be expected to become effective. The rate is however so slow as to produce no appreciable diurnal change above about 130 km. Some workers have claimed to identify the lines of atomic nitrogen in the night sky and auroral spectra, but Mitra (1947) and Déjardin (1948) regard such identification as false. Sodium (between about 80 and 100 km.), calcium, and aluminium are identifiable in the night sky spectrum, but in quantities presumably much too small to be of direct importance in atmospheric structure. Some permanent gases, *e.g.* He, may perhaps be present aloft in significant amount but fail to show themselves for lack of suitable exciting radiations. It seems more probable, however, that the composition remains effectively constant throughout the atmosphere apart from the substitution of atomic for molecular oxygen in the ionosphere.

Turning now to the non-permanent gases, water vapour and ozone, Dobson *et al.* (1946) have shown that the concentration of the former falls off very rapidly to a few per cent. of saturation in the first kilometre or so above the tropopause over England (at any rate when the tropopause is not high) and the relative dryness appears to be maintained up to levels of about 20 km. from a few, probably less-accurate, balloon samplings by E. Regener (1940). This important result is already affecting our ideas on radiative conditions within the stratosphere, as shown by Dobson. The occasional appearance of mother-of-pearl clouds around 27 km. indicates that the relative humidity of the stratosphere is not always low, but temperatures at the same levels and times have not been available to determine the vapour content on these occasions. Dobson (1946) also presented more detailed information than has been available hitherto on the association of ozone variations with weather systems in the region of the British Isles. These variations, up to about 30 per cent. of the seasonal mean, almost certainly arise in the lower stratosphere, well below the normal level, about 23 km., of maximum ozone concentration, and they are accompanied by changes of air mass of the lower stratosphere in association with tropospheric fronts. Air of equatorial origin provides negative anomalies and of polar origin, positive anomalies. It seems likely that the occasionally observed double maximum in ozone distribution, with the lower maximum at about 16 km.—the spectra from the V2 rocket flight of October 10, 1946, provided such a case—arises with the passage of a cold front. In any case the fluctuations of ozone should prove a valuable aid to the study of the dynamics of large extra-tropical depressions.

The spectra of the rocket flight just referred to showed all

ozone to have been below 55 km. but extending above 34 km. This is in agreement with earlier inference.

11. The Variation of Pressure with Height

Over the range of height covered by sounding balloons quite the most accurate method of determining the pressure–height relation is to use the pressure–temperature data in the equation of hydrostatic equilibrium. Errors in pressure measurement are then of little consequence (they are of no consequence at all where the atmosphere is isothermal). If the height of a pressure sonde be

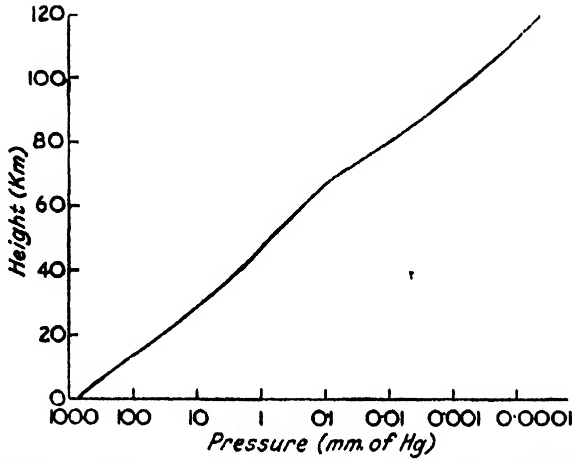


Fig. 2.—Variation of pressure with height on rocket flight of March 7, 1947. (*Redrawn from* Krause.)

measured directly, as by modern radar methods, the required relation is available directly but pressure errors are then, of course, of full consequence. Two pressure–height curves have been published from V2 rocket firings of which the second and more reliable is shown in Fig. 2, the curve to 15 km. being from simultaneous radio sonde data and above that level from direct observations. There are likely to be appreciable seasonal and geographical variations in this relation whose magnitudes are uncertain.

12. The Temperature of the Upper Atmosphere

12.1. *The Tropopause and Lower Stratosphere—Heights up to about 30 km.*—This is the region for which our knowledge derives from direct soundings, is fairly reliable and relatively complete, though there are important gaps. It has long been known that the lower stratosphere of equatorial and tropical regions is, in the mean, a layer with a moderate inversion of temperature whose magnitude

diminishes with increase of latitude. In middle latitudes, and to rather high latitudes in summer, the temperature increases slightly with height or is isothermal, but there are appreciable day-to-day variations with incursions of tropical or polar air aloft. In polar latitudes, particularly in winter, there is a lapse of temperature, though for Arctic stations, to which stratosphere soundings had until lately been confined, the tropopause remains fairly distinct

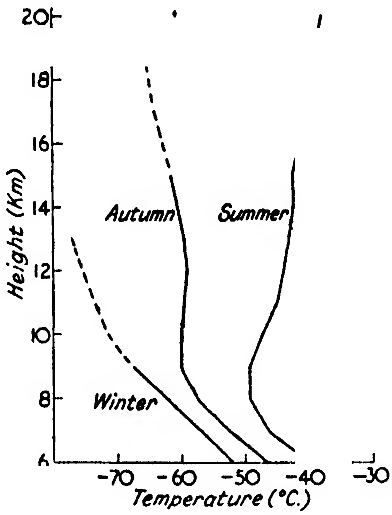


FIG. 3.—Mean temperatures for three 14-day periods at 78° S., 165° W (--- indicate mean of less than five observations)
Note the disappearance of the tropopause in the winter soundings.
(*Redrawn from* COURT.)

as a boundary between air masses of different stability. Important additions to this picture can now be made (Court, 1942) as a result of 190 soundings during most of a year, 1940–41, at Little America III (78° S., 165° W.) in Antarctica. There is a well-defined tropopause in summer (with some inversion above) which weakens with the advance of winter, as in the northern hemisphere, *but completely disappears at the end of the winter* (Fig. 3). This may be taken to imply a less-vigorous interchange of air in moderate and high southerly latitudes than in corresponding northerly latitudes, and Court quotes as additional evidence a greater seasonal variation of temperature in the Antarctic stratosphere, hardly proven as yet, a lower oxygen content of Antarctic air and asymmetry in the world ozone distribution. It also suggests that the effects of atmospheric radiation may be most in evidence in the Antarctic stratosphere.

At levels not too near the tropopause, which has maximum

height and minimum temperature in summer, the annual temperature curve of middle latitudes is symmetrical with a maximum in mid-summer, the phase being a month or more in advance of the upper troposphere wave. Dobson *et al.* (1946) attribute this to the annual variation of ozone, a maximum in spring and a minimum in autumn, and a consequent radiative control. The midsummer maximum remains in evidence up to at least 30 km. (Gutenberg, 1949) though the minimum is there delayed until about March.

The temperature gradient in level surfaces determines the

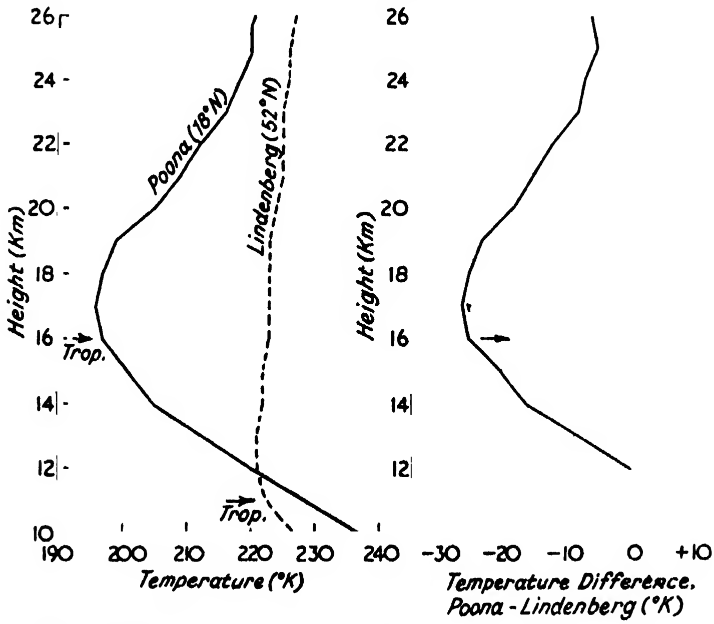


FIG. 4.—Mean temperatures over Poona and Lindenberg, and temperature difference indicative of the meridional increase of temperature towards higher latitude in the lower stratosphere.

variation of wind with height in extra-tropical regions (see § 13) and is therefore of great significance in dynamical meteorology, particularly in the lower stratosphere where the gradient may be considerable. In general the temperature *increases* polewards in this layer (see Fig. 4), though there may be a reversal of gradient in high latitudes in winter. There is a considerable day-to-day variation of gradient in a particular region. Further analysis of existing data and more data for the higher levels are required before a reasonably complete picture will be available.

Claims have been made, *e.g.* Ballard (1941), for an appreciable diurnal variation, increasing with height, in the temperature of the lower stratosphere but in view of uncertain radiation errors

on the thermometers such variation can hardly be regarded as well proven. It is certainly difficult to account for such changes by radiation and a dynamical cause is not apparent.

12.2. *The Upper Stratosphere—30 to 80 km.*—Knowledge of the temperature of the upper stratosphere derives directly from rocket sounding and otherwise indirectly from the abnormal propagation of sound (which is rather sensitive to wind), atmospheric tides, the form of ozone absorption bands, the intensity of twilight and, in the upper levels, from meteor trails and noctilucent clouds. Rocket information is necessarily limited and local at present, and no one of the other methods gives, on its own, values in which great confidence can be placed, though there is a measure of general agreement,

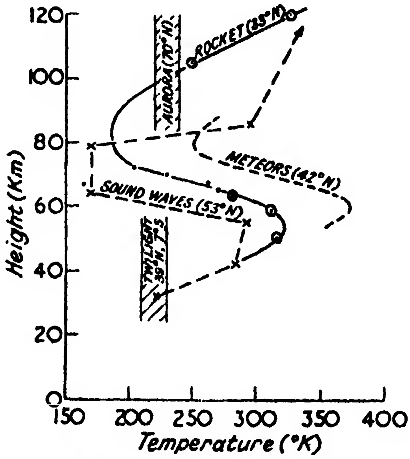


FIG. 5.—Recent determinations of the temperature of the air above about 25 km.

On the "rocket" curve, points marked ⊙ are derived from the pressure-height curve, and those marked • are from the ram pressure. (Sounding of March 7, 1947.) Temperatures inferred from sound waves above about 60 km. are rather tentative.

and some apparent disagreement. The indirect methods mainly involve an assumed distribution of temperature with which the data are found to be consistent.

The older methods were all consistent with a marked rise in temperature to higher than surface values at about 50 or 60 km., and there is good reason for such a rise in the strong absorption of solar ultra-violet radiation by ozone which extends up to about that level (*see* Penndorf, 1941, Gowan, 1947 (*a*)). The absence of any strongly absorbing gas between 60 and 80 km. (or of radiation of wavelength to be absorbed by the gases present) led to the surmise that the temperature again fell sharply over this layer. These conclusions are now supported by the rocket sounding by Best *et al.* (1947) and by the preliminary analysis of the reception of

waves from the carefully organised Heligoland explosion of April 18, 1947 (Cox *et al.*, 1948), the results being shown with others in Fig. 5. There is also indirect confirmation of the general trend from the most recent work on atmospheric tides by Weekes and Wilkes (1948). Hulbert's analysis of twilight intensity leads to a mainly isothermal layer of 220° K. ± 15° K. from 20 to 55 km. and so is apparently inconsistent with the above picture. But there may be a strong diurnal variation of temperature in the levels in which sunlight is, as here, absorbed, so that the time of day may be very significant. In this connection Gowan (1947 (*b*)) has computed a nocturnal cooling, under the assumption of radiative equilibrium, of from 0° C. at 11 km. to 30° C. at 55 km. Experiments on the propagation of sound at night would be of great interest.

F. L. Whipple's values of temperature from meteor trails show a progression similar to that from the rocket and the Heligoland explosion, but at an enhanced level (*see* Fig. 5). Their reliability is, however, difficult to assess.

12.3 *The Ionosphere—Heights greater than 75 km.*—Up to 1939 the temperature of the ionosphere had been inferred from the variation of ion density with height as found from radio wave reflections, from auroral band spectra, and, in the region of 75 to 100 km., from meteor trails and noctilucent (dust) clouds. The radio evidence indicated a temperature of about 200° K. at 75 km., consistent with the assumed drop in temperature towards the top of the upper stratosphere, followed by a sustained rise to the F_2 layer. The amount of this rise (the maximum value suggested for the level of 300 km. was about 1000° K.) was, however, in question. A secondary maximum in the ends of meteor trails at about 80 km. suggested also a minimum of temperature at that level, while well-defined dust clouds gave evidence of the onset of a stable temperature gradient at about the same level. Auroral spectra, however, led Vegard to much lower temperatures (*c.* 240° K.) at 100 to 150 km. than were derived from radio data, but that could be interpreted as due to a sustained lack of insolation in the auroral regions.

Again the rocket sounding and the Heligoland explosion (*see* Fig. 5) provide support for these earlier ideas, save for the highest temperatures derived from radio. A. and E. Vassy (1942) consider the width of the night-sky green line to be too small for the larger temperatures from radio, but also too small for the low temperature given by auroral spectra, a temperature which has been re-estimated by Vegard and Tönsberg (1941) and found to agree with Vegard's earlier value.

S. L. Seaton (1948) has computed values of the recombination coefficient in the E, F_1, and F_2 layers as a function of latitude and time of day from the data of eighteen ionospheric stations between 71° N. and 43° S. for January 1947. From these he derives the corresponding temperatures and gives horizontal and vertical sections which show a marked cellular structure between about 80 and 400 km. He finds temperature to range spatially and temporally from about 100° K. to 1000° K. in the F_2 layer with smaller ranges at lower levels, while at noon meridian two main high-temperature centres appear at 200 to 300 km., one at about 35° S. and the other, less peaked, at 30° N. to 50° N. The computed values of the temperature and temperature gradient are possibly questionable but the broad thermal patterns are thought to be fairly sound. If so they imply the existence of quite strong wind systems which should provide interesting studies in dynamics.

13. The Winds of the Upper Atmosphere

Systematic observations of wind up to about 20 km., by radio or radar, are now provided by a number of stations in Western Europe, the North Atlantic, and North America and at scattered points elsewhere, while 30 km. is being reached at one or two stations (Gutenberg, 1949). There is in the mean a fairly well-defined maximum of the order of 30 m/s from a westerly point at the tropopause in middle latitudes, which is a consequence of the reversal of the meridional temperature gradient at that level (*see* § 12.1). Gutenberg reports, however, that the maximum occurs appreciably below the tropopause in approaching the northern tropic. The day-by-day variations of wind in the lower stratosphere are considerable, as in the troposphere beneath, and it is to be hoped that the large, though still inadequate number of soundings of temperature and (or) wind of recent years will soon enable maps of vector means and variations to be prepared for large parts of the earth.

Johnson (1946) has reported observations of wind measured by smoke shell on 35 occasions in 1944–45 at 30 km. over southern England. A mean flow of 37 m/s from a westerly quadrant was obtained for the winter and of 12 m/s from an easterly quadrant for the summer, thus confirming F. J. W. Whipple's inference, from the propagation of sound, of a monsoon at about this level. The months of change were April and October. Gutenberg also reports easterly winds at 30 km. in summer over New Mexico and he considers easterly winds to be world-wide at this season from 18 km.

Evidence on wind at higher levels comes from meteor trails, noctilucent clouds, and radio reflections. A 13-year series of observations of the first two phenomena at the Sonneberg Observatory (50° N.) has led Hoffmeister to conclude that east winds prevail up to about 80 km. Between 90 and 100 km. there was less regularity, with the westerly quadrant strongly predominant, while in the region of 120 km. all winds blew from the south-west quadrant in summer at speeds up to 100 m/s (mean 50 m/s), and in winter mainly from the same or the northerly quadrant, at speeds up to 270 m/s (mean South-westerly 65 m/s, mean Northerly 89 m/s). A few observations in South Africa (23° S.) gave winds from all directions. The radio observations of Munro and Beynon, if interpreted as evidence of wind rather than the movement of a pressure wave, imply easterly winds in the F region over New South Wales and north-west Europe respectively with mean speeds of order 100 m/s.

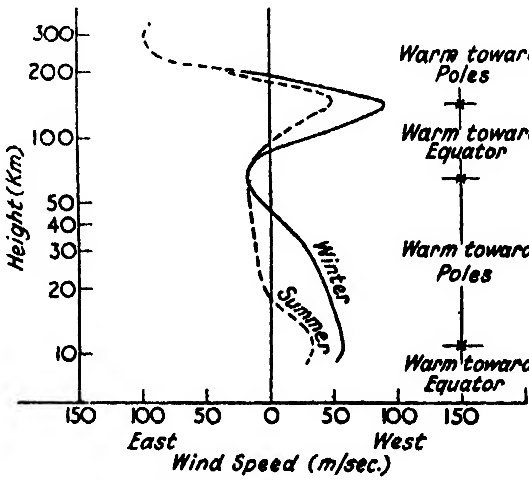


FIG. 6.—Tentative curves of variation of wind with height in middle latitudes for summer and winter East or west components are shown but these far outweigh in the main the north or south components. On right, the meridional temperature gradient for quasi-geostrophic wind.

The logarithmic scale of height enables approximate mass flow to be inferred directly.

Very tentative curves for the variation of wind with height in middle latitudes between the upper troposphere and the F region, as indicated by the above results, are shown in Fig. 6 together with the direction of the meridional temperature gradient on the basis of quasi-geostrophic mean flow. There is clearly much food for the thought of the dynamical meteorologist in these curves.

REFERENCES

BALLARD, J. C., 1941, *Mon. Weath. Rev., Washington*, **69**, 33.
BEST, N., HAVENS, R., and LA GOW, H., 1947, *Phys. Rev.*, **71**, 915.
BEYNON, W. J. G., 1948, *Nature*, **162**, 887.
BRASEFIELD, C. J., 1948, *J. Met.*, **5**, 147.
CHAPMAN, S., *et al.*, 1943, *Reports of the Gassiot Committee of the Royal Society. Reports on Prog. in Phys.*, **9**, 1-100.
—— 1948, *The Emission Spectra of the Night Sky and Auroræ.* Royal Society Conference; published by the Physical Society.
COURT, A., 1942, *Bull. Amer. Met. Soc.*, **23**, 220.
COX, E. F., ATANOSOFF, J. V., and SNAVELY, B. L., 1948, *ibid.*, **29**, 78.
DÉJARDIN, G., 1948, *The Emission Spectra of the Night Sky and Auroræ.* Royal Society Conference; published by the Physical Society, 3.
DOBSON, G. M. B., BREWER, A. W., and CWILONG, B., 1946, *Proc. Roy. Soc.*, **A185**, 144.
—— 1948, *Proc. Phys. Soc.*, **60**, 52.
FENNELL, P., 1944, *Bull. Amer. Met. Soc.*, **25**, 371.
GOWAN, E. H., 1947 (*a*), *Proc. Roy. Soc.*, **A190**, 219.
—— 1947 (*b*), *ibid.*, **A190**, 227.
GUTENBERG, B., 1949, *Bull. Amer. Met. Soc.*, **30**, 62.
HEY, J. S., and STEWART, G. S., 1947, *Proc. Phys. Soc.*, **59**, 858.
HOFFMEISTER, C., 1946, *Zeit. f. Met.*, **1**, 33.
HULBERT, E. O., 1938, *J. Opt. Soc. Amer.*, **28**, 227.
—— 1947, *ibid.*, **37**, 405.
JOHNSON, SIR N. K., 1946, *Nature*, **157**, 24.
KRAUSE, E. H., 1947, *Proc. Amer. Phil. Soc.*, **91**, 430.
MITRA, S. K., 1947, *The Upper Atmosphere.* Royal Asiatic Society of Bengal, Calcutta.
MUNRO, G. H., 1948, *Nature*, **162**, 886.
NEWELL, H. E., 1947, *Sc. Monthly*, **64**, 453.
OLIVIER, C. P., 1942, *Proc. Amer. Phil. Soc.*, **85**, 93.
PANETH, F. A., *et al.*, 1939, *Quart. J. Roy. Met. Soc.*, **65**, 303.
PENNDORF, R., 1941, *Met. Zeit.*, **58**, 1 (translated by C. CHAPMAN in 1946, *Bull. Amer. Met. Soc.*, **27**, 331).
—— 1942, *Gerlands Beit. z. Geophys.*, **59**, 175.
REGENER E., 1940, *Schrift d. Deutsch. Akad. d. Luftfahrtforschung.*
—— 1941, *ibid.*, **37**, 5.
SEATON, S. L., 1948, *J. Met.* **5**, 204.
STÖRMER, C., 1939, *Astrophys. Nor., Oslo*, **3**, No. 5.
SUTHERLAND, G. B. B. M., and CALLENDAR, G. S., 1943, *Rep. Prog. Phys.*, **9**, 18.
VASSY, A. and E., 1942, *J. Phys. et Rad.*
VEGARD, L., and TÖNSBERG, E., 1941, *Geof. Publ.*, **13**, Nos. 1 and 5.
WEEKES, K., and WILKES, M. V., 1948, *Proc. Roy. Soc.*, **A192**, 80.
WHIPPLE, F. L., 1943, *Rev. Mod. Phys.*, **15**, 246.

GENERAL AND PHYSICAL CHEMISTRY.

By J. W. SMITH, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic.

THE REACTION BETWEEN HYDROGEN AND OXYGEN.—Amongst the many chemical problems of which the solutions have been evolved

only slowly and as the result of numerous precise investigations spread over a considerable number of years, one of the most interesting is that of the union of hydrogen and oxygen. Not only is this reaction of great importance on its own account, but also through its bearing on other reactions. Thus the pronounced influence of a trace of moisture in facilitating the reaction between carbon monoxide and oxygen is due to the fact that, in its presence, this reaction can occur through the initial interaction of carbon monoxide with water, $CO + H_2O = CO_2 + H_2$, followed by the re-oxidation of the hydrogen. These reactions occur with a lower energy of activation, and hence more readily than, the direct oxidation process. The oxidation of carbon monoxide shows, therefore, many of the characteristics of the hydrogen—oxygen reaction, including a marked susceptibility to inhibition by halogens.

From the superficial form of the equation $2H_2 + O_2 = 2H_2O$, it would be anticipated, by application of the Law of Mass Action, that the rate of this reaction would increase proportionally to the pressure of oxygen and the square of the pressure of hydrogen. This simple relationship is found to hold, however, only under special circumstances, as, for instance, in the *heterogeneous* reaction when the gases combine slowly on the surface of certain catalysts, such as porcelain. For the *homogeneous* gas reaction, however, no such simple relationship can be traced. During the past few years, however, the picture of this reaction has been clarified very considerably, principally through the work of Sir Cyril Hinshelwood and his co-workers, the results of which were summarised in Hinshelwood's Bakerian Lecture to the Royal Society (*Proc. Roy. Soc.*, 1946, **A188**, 1).

When studied in glass or silica vessels at low temperature, the reaction has been proved to be principally a heterogeneous process on the walls of the vessel. It has a low-temperature coefficient and a rate not greatly influenced by pressure. At temperatures above 540° C., however, a reaction in the homogeneous gas phase comes into prominence. For this reaction no simple relationships between the velocity and the partial pressures of the reacting gases can be traced, and the rate is markedly influenced by the presence of inert gases including steam. Also, under certain well-defined conditions, explosion occurs.

Viewing the matter in the light of modern knowledge, the reaction appears to be difficult to initiate, but once this is effected it is propagated very rapidly, circumstances which suggest immediately that a chain process is involved. Such a mechanism would be expected to include an initiation reaction associated with a high

energy of activation, which would render the process difficult to start, followed by chain-propagating steps with low energy of activation. For some time this maintenance of the chain reaction was attributed to "energy chains," that is chains which are maintained through the product molecules passing on their excess energy to reactant molecules which are thereby activated. On the other hand, it has more recently been found that the facts of this and of other reactions for which such chains had been postulated are more in keeping with chain processes in which the intermediate steps are associated with free atoms or free radicals, the reactions of which have very low activation energies. This would lead to the circumstance that the chain once initiated has a good chance of being propagated to a great length. Further, the fact that the reaction, under favourable circumstances, can readily attain explosive violence suggests that chain-branching occurs, that is that the interaction of one active species with a molecule produces two reactive product particles.

The next problems to decide are the selection of the most probable chain-initiating and chain-propagating processes. A knowledge of the bond energies permits discrimination between the various possible reactions. Thus for the chain-initiating process it seems most likely that a bimolecular collision is involved. In the reaction mixture concerned such collisions will be of three types, each of which can lead directly to the formation of atoms or active radicals

$$H_2 + M \longrightarrow 2H + M\,;\quad O_2 + M \longrightarrow 2O + M\,;\quad H_2 + O_2 \longrightarrow 2OH$$

where M represents any molecule present in the mixture, whether hydrogen, oxygen, water, or inert diluent gas.

The dissociation of hydrogen into atoms requires much less energy than the dissociation of oxygen, and hence appears to be the most probable initial process. For reasons which will be discussed later this is now the most generally favoured chain-initiating step, although some still regard the formation of hydroxyl radicals as being the process involved. As, however, all three species participate in the chain-propagation mechanism this is not a point which is vital to the general argument, and one can profitably proceed to consider the possible fates of all three species of active product which may be produced by these reactions.

Bimolecular association reactions involving simple particles are very improbable, as they are highly exothermic. For instance, the heat of reaction of a hydrogen atom with an oxygen molecule to form a HO_2 radical is about 50,000 calories per g.-mol. Such reactions will occur, therefore, only if there is a third particle present

to accept the excess energy liberated in the reaction. Hence the most likely reactions for a hydrogen atom are

$$H + O_2 = OH + O \qquad (i)$$

and

$$H + O_2 + M = HO_2 + M \quad . \quad . \quad . \qquad (ii)$$

For an oxygen atom the most probable reactions are

$$O + H_2 = OH + H \quad . \quad . \quad . \quad . \qquad (iii)$$

and

$$O + H_2 + M = H_2O + M \quad . \quad . \quad . \qquad (iv)$$

whereas for the hydroxyl radicals the only reactions likely to occur seems to be

$$OH + H_2 = H_2O + H \quad . \quad . \quad . \quad . \qquad (v)$$

The only other change which appears to be probable, other than recombination reactions is

$$HO_2 + H_2 = H_2O + OH \quad . \quad . \quad . \quad . \qquad (vi)$$

The obvious interrelationship between hydrogen atoms, oxygen atoms, and hydroxyl radicals which emerges from these considerations renders the nature of the initial process as only of secondary importance.

Of these reactions which may participate in the chain mechanism, (i) and (iii) lead to chain branching, two active species being produced for each one entering the process. Unless, therefore, the branching of chains is held in check by the existence of chain-breaking processes the reaction will rapidly attain explosive violence.

Amongst the reactions which may terminate the reaction chains the most obvious are those in which two atoms or radicals might interact without the production of an active species which can propagate the chain further. These would include $H + H + M = H_2 + M$, $H + OH + M = H_2O + M$, and $OH + OH + M = H_2O_2 + M$. Such reactions will be very rare, however, as in each case they demand the collision of two species, each present only at very low concentration, in the presence of a third molecule which can act as an energy remover. Reaction (iv) involves the participation of one active species only in the ternary collision, but at low pressures much the most probable chain-breaking processes are those in which the active species reach the walls of the containing vessel, and there are adsorbed and recombined.

Since at constant temperature and pressure the rate of initiation of chains will depend upon the number of molecules present, and hence upon the volume, whereas the number of chains broken through the active species reaching the walls of the reaction vessel will depend on the surface area of the latter, there will be a considerable "scale effect." The volume of the vessel is proportional to the cube of the radius, whilst the surface area is proportional to

the square of the radius. Hence the ratio of the volume to the surface area increases with increasing dimensions of the vessel. In small vessels where the surface area is relatively large the chain-breaking effect is sufficient to neutralise the rate of initiation and branching of chains, and hence a steady reaction at moderate velocity is maintained. As the size of vessel is increased, however, the volume effect increases more rapidly than the surface effect, and hence the velocity of the steady reaction is increased. At a certain critical size, however, the surface chain-breaking effect will just fail to neutralise the branching of the chains, and an explosion results. The position is, in fact, precisely similar to that encountered in the disintegration of Uranium 235 or of Plutonium by neutrons, which is a chain process, each interaction between a nucleus and a neutron resulting in the production of several other neutrons. In this case the chain-breaking process involves the escape of neutrons from the mass, and consequently when the critical size is reached the rate of emission of neutrons becomes greater than the rate at which they can escape from the surface.

Similar effects are produced by changes of pressure, since the rate of diffusion of the active species to the walls decreases with increasing pressure. Therefore, at a constant temperature, it would be expected that a slow controlled reaction would occur at low pressures. The rate of this reaction should increase with pressure, but remain controllable, until a critical pressure is reached at which new reaction chains are formed more rapidly than they are terminated and explosion results. Hinshelwood and his co-workers have found that in a silica vessel of a few hundred cubic centimetres capacity at 550° C. this critical pressure is about 1 mm. for a $2H_2 + O_2$ mixture.

At higher pressures, however, the ternary processes represented by reactions (ii) and (iv) become of increasing importance. These interactions decrease the degree of branching of the chains, the one by resulting in the production of one active product particle only and the other by terminating the chain. Consequently at some higher pressure the position will be reached where, through the intervention of these reactions, the reaction chains can again be terminated at the same rate as that at which they are initiated. Hence a second explosion limit will occur, above which reaction takes place again at a controllable rate and without explosion. For a $2H_2 + O_2$ mixture at 550° this second critical pressure is about 100 mm.

The proof that the process controlling the reaction under these conditions occurs in the gas phase was developed by Hinshelwood

in the following way. The rate of branching of chains is proportional to a binary collision product [X][Y], where X represents H or O atoms and Y represents O_2 or H_2 molecules, whilst the rate of quenching of the chains is proportional to the ternary collision product of X and Y with any other molecule which may be present. At the second explosion limit these become equal, and therefore the condition for this limit is given by

$$K[X][Y] = K'[X][Y]\,(a[H_2] + b[O_2] + c[M])$$

or

$$H_2 + k_O[O_2] + k_M[M] = K^* \quad . \quad . \quad . \quad . \quad (1)$$

The relative values of the constants a, b, and c can be calculated approximately from the kinetic theory by assuming that for molecules of the same atomicity they correspond to the relative frequencies of ternary collisions.

From the temperature coefficient of the second explosion limit the energy of activation of the chain branching process was deduced as 26,000 calories. At 550° C. this will lead to one collision in about 10^7 leading to branching. In a vessel of 5 cm. radius about $1{\cdot}5 \times 10^{10}$ collisions would be made by particles diffusing at 1 atmosphere pressure from the centre of the vessel to the wall. Branching would occur many times, and hence explosion would result, unless ternary collision quenching controlled it. On the other hand it is to be expected that control by diffusion to the wall will become predominant when the number of collisions made by a particle on its way there is about 10^7. This will occur at a pressure of $10^7/1{\cdot}5 \times 10^{10}$ atm., or about 0·5 mm., which is of the order of magnitude observed for the pressure at the first explosion limit.

It has been pointed out that the two association reactions which may control the chain branching differ in that reaction (iv) gives the inert water molecule, and therefore ends any possible radical chain, whereas reaction (ii) gives the HO_2 radical, itself a reactive particle but one which can probably react only with hydrogen according to process (vi). In this case, therefore, the two steps result simply in the replacement of hydrogen atoms by OH radicals. The HO_2 radicals can and will diffuse to the wall, where they are destroyed, and in so far as this occurs the chain is broken, but the higher the total gas pressure the less important will this diffusion become.

At high pressures, therefore, the formation of HO_2 serves simply to delay, without inhibiting, the branching of the chains. Hence at high enough pressures a third explosion limit is to be expected, above which immediate explosion occurs. This has actually been observed during the past few years, although at pressures below

this critical limit the reaction velocity is very high and therefore there could be under these conditions departures from thermal equilibrium which would lead to explosion through mere rise of temperature. The studies made, however, upon the variation of this limit with changes in the hydrogen–oxygen ratio, and with the addition of "inert" gases (nitrogen, carbon dioxide, and steam) were consistent with the view that an essentially isothermal branching of chains occurs.

Between the two explosion limits the processes occurring are therefore regarded by Hinshelwood as being

A chain initiating reaction giving H or OH (rate $= f_1$).
$OH + H_2 = H_2O + H$ (velocity constant k_1)
$H + O_2 = OH + O$ (velocity constant k_2)
$O + H_2 = OH + H$ (velocity constant k_3)
$H + O_2 + M = HO_2 + M$ (velocity constant k_4)
$HO_2 \longrightarrow \frac{1}{2}H_2O$ at the walls (velocity constant k_5)
$HO_2 + H_2 = H_2O + OH$ (velocity constant k_6)

From these equations the following expression was derived for the rate of formation of water in the steady state:

$$\frac{d[H_2O]}{dt} = \frac{f_1 \cdot \dfrac{1.5k_5 + 2k_6[H_2]}{k_5 + k_6H_2}}{1 - 2k_2/\Sigma k_4[M] - k_6[H_2]/(k_5 + k_6[H_2])} \quad . \quad (2)$$

In this treatment certain simplifying assumptions have been made. The diffusion to the wall of all chain carriers other than HO_2 has been neglected. This was justified by the fact that the first limit occurs at very low pressures compared with the second and third limits. Equation (2) therefore describes the second and third limits but not the first. Also, of the two possible ternary quenching processes the one which results in the formation of the HO_2 radical is taken as representing both. This is reasonable, since the other process has no aftermath such as is represented by the decomposition of HO_2 on the walls or its interaction with hydrogen.

Evidently the rate of formation of water becomes infinite and hence explosion results if the denominator of equation (2) becomes zero. For fixed proportions of hydrogen, oxygen, and an inert gas (if any) this condition is satisfied by two values of the pressure, corresponding to the second and third explosion limits, respectively. Deductions from this equation are simplified through the circumstances that k_4 and k_5 can be derived from the values of K_O and k_M of equation (1), and the diffusion coefficient of the HO_2 radical.

Equation (2) then permits the calculation of the relative diffusion constants of various molecules present, which can be expressed in an inverse form as $D'_M = D_{H_2}/D_M$, and also of the relative collision

numbers k_M. Each of these figures can be compared with those deduced directly from the kinetic theory. The following comparative table was thence derived:

M	D'_M		k_M		
	Exptl. from Eq. (2).	From Kinetic Theory.	Exptl. from Eq. (2).	Exptl. from Eq. (1).	From Kinetic Theory.
H_2 . .	1·0	1·0	1·0	1·0	1·0
O_2 . .	3·0	3·8	0·38	0·36	0·40
N_2 . .	2·2	3·9	0·30	0·37	0·45
CO_2 . .	2·5	5·2	0·90	0·90	0·51
H_2O . .	4·0	4·1	8·3	9·1	0·62

The values of D'_M obtained confirm the theory, although the importance of the deviations observed is difficult to assess. The accord between the values of k_M as derived from the two equations is striking, and is important in showing that the second and third limits are governed by the same mechanism. The fact that these values are also in accord with those deduced from the kinetic theory in the cases of oxygen and nitrogen, but not for carbon dioxide and water vapour, is explained by Hinshelwood on the grounds that for simple gases of the same atomicity the relative quenching is proportional to the relative numbers of ternary collisions made, whilst for gases of differing atomicity or for more complex molecules the quenching power depends on specific interactions.

Another curious feature of the reaction between hydrogen and oxygen is that its rate can be reduced a hundred times or more by coating the reaction vessel with potassium chloride or other salt. The first and third limits, which are dependent on the surface, are affected by the salts, whereas the second limit, controlled by the gas-phase process, is not. The first explosion limit, for instance, can be increased to 2·4–18 times its value in uncoated silica vessels. The effect, therefore, seems to be due to the efficient removal of radicals at the surface, and as most, if not all, of the metals whose salts are active in this respect form hydrides it seems likely that definite cycles of reaction occur, for instance

$$KCl + H = KH + Cl;\quad KCl + H = K + HCl;$$
$$KH + HCl = KCl + H_2$$

This can therefore lead to a catalytic recombination of hydrogen atoms without permanent change of the salt.

When the walls are coated with potassium iodide, on the other hand, the mechanism of the reaction undergoes a complete transformation. The chain reaction in the gas phase is replaced by a

surface reaction, the rate of which is independent of the pressure of hydrogen or of added nitrogen, but shows a variation with the oxygen pressure of the form $v = a + b[O_2]$. It appears that in a cycle of the type which was suggested for potassium chloride-coated walls free iodine escapes from the surface in minute quantity. It is well known that even at pressures of a fraction of a millimetre iodine causes complete suppression of the chains in the gas phase owing to the removal of hydrogen atoms. This provides indirect confirmation that halide salts enter into direct cycles of chemical change.

As the conditions for explosion derived from Equation (2) are independent of f_1, the function describing the rate of initiation of the chains, the latter has been very difficult to determine. The study of the third limit permitted the determination of all the terms in Equation (2) except f_1, and hence a combination of measurements on the third limit with those of the reaction velocity at pressures between the second and third limits should permit the variation of f_1 with various factors to be tested.

The circumstance that the chain-breaking efficiency of potassium chloride is almost unity has facilitated the study of the region between these two explosion limits. When silica, pyrex, and other hard glass vessels were used the reaction was markedly autocatalytic owing to the water adsorbed on the walls lowering the chain-breaking efficiency.

At 570°–600° C. and in a vessel the walls of which were coated with potassium chloride it was found that f_1 fitted best a relation of the form

$$f_1 = k[H_2].(Z_{H_2}[H_2] + Z_{O_2}[O_2] + Z_M[M])$$

where Z_{H_2}, Z_{O_2}, and Z_M are the relative collision numbers for hydrogen molecules with hydrogen, oxygen, and any inert gas M, respectively. This relationship implies that the primary process involves the dissociation of hydrogen into atoms by collision with any other molecule present.

Under these same conditions the study of temperature effects indicates that the energy of activation of the chain-initiating process is 90,000–110,000 calories per g.-mol., consistent with the value of 101,000 calories for the energy of dissociation of hydrogen into atoms.

It cannot be claimed that all the problems in connection with this reaction have been solved. For instance at lower temperatures than those discussed above the reaction chains doubtless originate mainly on the wall of the vessel, thereby leading to a more complex

variation of the reaction velocity with conditions, and there are still some complexities regarding the influence of steam on the reaction and regarding the development with time of the reaction in its initial stages. Nevertheless, these seem to be relative trivialities now that the broad picture of the mechanism of the homogeneous gas reaction has been obtained, and the way has been opened up for the reinvestigation of the reaction under various specialised conditions.

ORGANIC CHEMISTRY.

By A. W. Johnson, Ph.D., A.R.C.S., The University, Cambridge.

Methods of Reduction and Hydrogenation

Catalysts and Reagents.—The usual method of preparation of Raney nickel of high and consistent activity is that of Mozingo (*Org. Syntheses*, 1941, **21**, 15) and is based on the earlier work of Adkins. The latter worker has now produced a number of more active nickel catalysts of the Raney type, by lowering the temperature of reaction of the alkali and Raney alloy, by shortening the reaction time and by incorporating an efficient washing process with the careful exclusion of air (Adkins *et al.*, *J. Amer. Chem. Soc.*, 1946, **68**, 1471 ; 1948, **70**, 695). Using these very active catalysts it is possible to hydrogenate esters to primary alcohols at temperatures $< 50°$.

Of the new chemical reducing agents, lithium aluminium hydride, $LiAlH_4$, is outstanding. It was introduced by Finbolt, Bond and Schesinger (*J. Amer. Chem. Soc.*, 1947, **69**, 1199) and is prepared by the action of lithium hydride on an ethereal solution of aluminium chloride :

$$4LiH + AlCl_3 \rightarrow LiAlH_4 + 3LiCl.$$

Lithium aluminium hydride is now available commercially, and as the reagent is soluble in ether its use is similar to the Grignard technique. Its main application is the reduction of acids, esters, acid halides and anhydrides as well as carbonyl compounds to carbinols, and amides and nitriles to amines, and already there have been many reports of its use. In addition organic and inorganic halides undergo hydrogenolysis and there is no doubt that the reagent will find wide applications in the future. It is indefinitely stable at room temperature, there is a favourable ratio of reducing capacity to mass and most reductions occur at room temperature. In general, double bonds are not attacked (some exceptions), so that it is valuable for selective reductions. On the other hand,

lithium aluminium hydride is rather expensive, it is not very satisfactory for ether-insoluble compounds, and those compounds containing active hydrogen atoms may decompose the reagent with evolution of hydrogen. On a quantitative basis, this latter observation has been developed into a method for the estimation of active hydrogens, as an alternative to the well-known Zerewitinoff method (Krynitsky, Johnson and Carhart, *J. Amer. Chem. Soc.*, 1948, **70**, 486; Hochstein, *ibid.*, 1949, **71**, 305). The scope of the reagent has been studied by Nystrom and Brown (*ibid.*, 1947, **69**, 1197, 2548; 1948, **70**, 3738):

Carbonyl compounds are smoothly converted to the corresponding carbinols, *e.g.* crotonaldehyde $\longrightarrow$ crotyl alcohol. Steroid ketones have been likewise reduced (Levin *et al.*, *J. Amer. Chem. Soc.*, 1948, **70**, 2958).

Carboxylic acids are similarly readily reduced to primary alcohols although allowance must be made for the decomposition of one mol. of the reagent by the free active hydrogen atom. Only triphenylacetic acid resisted reduction but this was achieved by use of the corresponding acid chloride. Free hydroxyl and amino groups did not interfere except to decompose the reagent to the extent of the active hydrogen atoms, so that hydroxy- and aminobenzyl alcohols were prepared by this method. Dicarboxylic acids were reduced to diprimary alcohols (*cf.* also Boekelheide and Rothchild, *J. Amer. Chem. Soc.*, 1949, **71**, 879), and only unsaturated acids of the cinnamic type suffered reduction of the double bonds, although even in these cases it was possible to isolate the first-formed cinnamyl alcohols (Hochstein and Brown, *J. Amer. Chem. Soc.*, 1948, **70**, 3484).

Esters have been the most widely studied group of compounds and are smoothly converted into the corresponding primary alcohols. Double bonds are unaffected in most cases. Examples of applications of the method include syntheses of vitamin A (Schwarzkopf *et al.*, *Helv. chim. Acta*, 1949, **32**, 443) and of polyenes related to vitamin A (Karrer *et al.*, *ibid.*, 1949, **32**, 436; Milas and Harrington, *J. Amer. Chem. Soc.*, 1947, **69**, 2247); $\alpha\beta$-unsaturated alcohols (Martin *et al.*, *ibid.*, 1948, **70**, 2601) and α-amino alcohols (Karrer, Portmann *et al.*, *Helv. chim. Acta*, 1948, **31**, 1617, 2088). Boekelheide and Rothchild (*J. Amer. Chem. Soc.*, 1949, **71**, 879) have reported a lithium aluminium hydride reduction of a quinolizidine ester where the carbothoxy group was replaced by hydrogen.

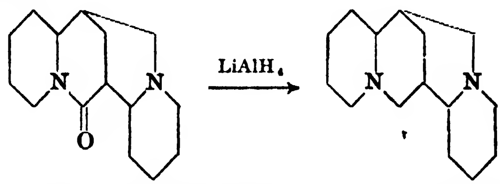
Acid chlorides and Anhydrides were smoothly reduced to the corresponding primary alcohols, *e.g.* sorboyl chloride $\longrightarrow$ sorbyl alcohol; benzoic anhydride $\longrightarrow$ benzyl alcohol.

Lactones were converted to diols, *e.g.* γ-valerolactone $\rightarrow$ 1:5-pentanediol.

Amides were reduced to primary amines and numerous examples of the reaction have been provided by Nystrom and Brown (*J. Amer. Chem. Soc.*, 1948, **70**, 3738) and by Uffer and Schlittler (*Helv. chim. Acta*, 1948, **31**, 1397), *e.g.*

$$CH_3.CH:CEt.CONH_2 \longrightarrow CH_3.CH:CEt.CH_2NH_2\ ;$$

An interesting application of the method was the synthesis of the alkaloid, *l*-sparteine from *l*-oxysparteine by Clemo *et al.* (*Nature*, 1948, **162**, 296):

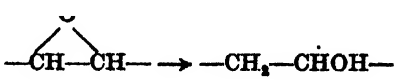


The reduction of NN-diethylbenzamide did not proceed as expected, and yielded benzyl alcohol.

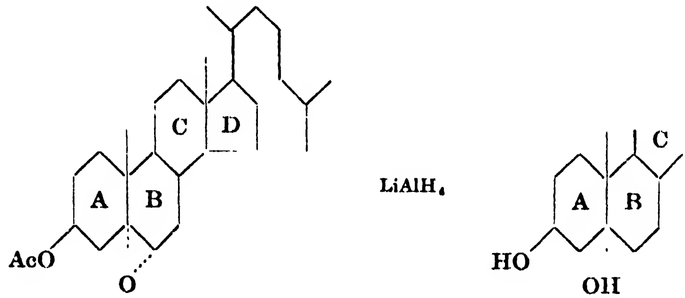
Nitriles were smoothly converted to primary amines and dinitriles to diamines, *e.g.* sebaconitrile $\rightarrow$ 1:10-diaminodecane. Similarly cyanhydrins formed β-hydroxyamines, *e.g.*, mandelonitrile $\rightarrow$ β-hydroxy-β-phenylethylamine.

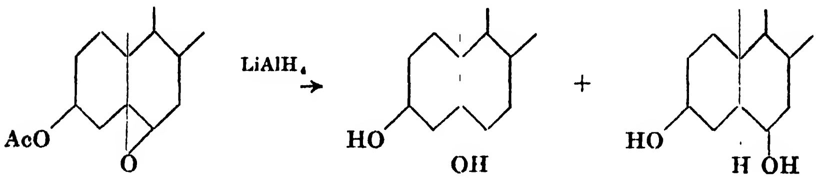
Organic halides were hydrogenolysed to the corresponding hydrocarbons, but in general this required more stringent conditions than are normally employed with this reagent, *e.g.* by the use of higher boiling ethers as solvents. As expected the hydrogenolysis depends on the activity of the halogen atom; thus bromides reacted more readily than chlorides and primary halides were more reactive than secondary which in turn were more reactive than tertiary. Alicyclic and aromatic halides were very unreactive (Johnson *et al.*, *J. Amer. Chem. Soc.*, 1948, **70**, 3664; Nystrom and Brown, *ibid.*, 3738). It was also found that the hydrogenolysis could be conveniently carried out by lithium hydride with a small amount of lithium aluminium hydride as hydrogen carrier.

Epoxides were converted to alcohols, *e.g.* styrene oxide $\rightarrow$ α-phenylethyl alcohol. The reaction has also been used by Prins (*ibid.*, 1948, **70**, 3955) for the preparation of desoxy sugars, *i.e.*



and by Plattner *et al.* (*Helv. chim. Acta*, 1948, **31**, 1885; 1949, **32**, 265, 587) for the preparation of 5- and 6-hydroxycoprostane and cholestane derivatives, *e.g.*

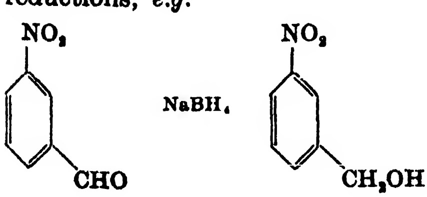




Plattner has also used this reaction in order to introduce hydroxyl groups into the 17-position of the steroid nucleus, *e.g.* in the partial syntheses of the naturally occurring 17-α-hydroxy cortical steroids J and O (*Helv. chim. Acta*, 1948, **31**, 2210).

In addition to all of the above types of compounds reduced by lithium aluminium hydride, Nystrom and Brown showed that *quinones* were converted to hydroquinones; *aromatic nitro compounds* to azo derivatives; *aliphatic nitro compounds* to amines; *azoxy compounds* to azo derivatives and *aldimines* to amines.

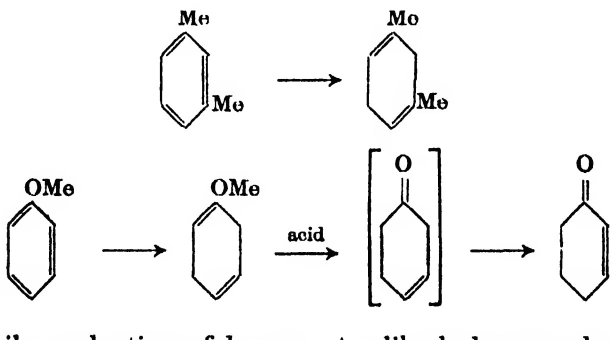
A more recent reducing agent of the same type as lithium aluminium hydride is sodium borohydride, $NaBH_4$, which is used in aqueous or methanol solution and is effective for the reduction of carbonyl compounds to carbinols (Chaikin and Brown, *J. Amer. Chem. Soc.*, 1949, **71**, 122). Carboxylic acids, esters, anhydrides and nitriles are unaffected but acid chlorides may be reduced to primary alcohols in non-aqueous media. In some respects the reagent is superior to lithium aluminium hydride for selective reductions, *e.g.*



; $PhCO.CH_2Br \rightarrow PhCHOH.CH_2Br$.

It has also been used for the reduction of cozymase to the dihydro compound (Mathews, *J. Biol. Chem.*, 1948, **176**, 229).

The reduction of organic compounds by dissolving metals has been known for many years, but one of the most interesting applications has been the partial reduction of benzenoid systems. Polynuclear hydrocarbons are reduced more easily than the monocyclic aromatic systems, although Willstätter's classical experiments on the reduction of terephthalic acid to the di- and tetrahydroterephthalic acids by sodium amalgam indicate that the presence of carboxy (also hydroxy) groups in the benzenoid nucleus facilitates the reduction. Although the partial reduction of benzenoid hydrocarbons had been mentioned in the earlier literature (*e.g.* Dumanski and Sverev, *J. Russ. P.C. Soc.*, 1916, **48**, 994; *Chem. Zentr.*, 1923, iii, 746), it was not until 1938 that Kazanski (*Chem. Abs.*, **32**, 2090; 1939, **33**, 787, 1287) studied the reaction in detail and showed that tetrahydro derivatives of these hydrocarbons could be obtained by the action of calcium ammonia ($Ca(NH_3)_6$), *e.g.* benzene was converted to *cyclo*hexene. This type of reduction has been examined further by Birch (*J. Chem. Soc.*, 1944, 430; 1945, 809; 1946, 593; 1947, 102, 1642) who used sodium in liquid ammonia as the reducing agent with the addition of an alcohol as a proton source, when the dihydrobenzenoid compound could often be obtained, *e.g.*:



The similar reduction of benzene to dihydrobenzene has been reported by Wibaut *et al.* (*Rec. trav. chim.*, 1948, **67**, 85) (*cf.* also Wooster, U.S.P. 2,182,242; *Chem. Abs.*, 1940, **34**, 1993).

Reduction of Typical Groups

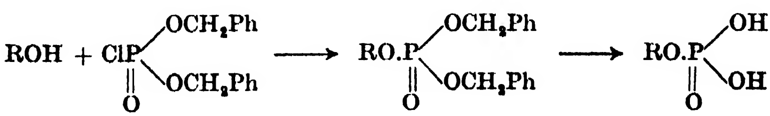
Acetylenic Linkages.—The increasing use of acetylenic intermediates in organic synthesis has led to the development and improvements in methods for the hydrogenation and reduction of acetylenic to ethylenic compounds. It is fairly general that

catalytic hydrogenation of acetylenic compounds yields the *cis* ethylenic derivatives and chemical reduction yields the *trans*. The acetylenic linkage can be reduced preferentially even when it is conjugated with other unsaturated bonds or when other double bonds are present in the molecule, and this has been extensively applied in certain polyene syntheses, particularly of vitamin A and its derivatives (Heilbron, *J. Chem. Soc.*, 1948, 386; Johnson, SCIENCE PROGRESS, 1948, **36**, 496; Isler *et al.*, *Helv. chim. Acta*, 1949, **32**, 489).

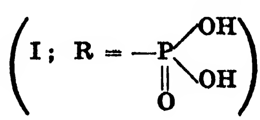
Hydrogenolysis of Ethers.—For preparative purposes, the most important example of this reaction is the ease with which the benzyl ethers undergo cleavage during hydrogenation, *e.g.* at 100–150° in the presence of Raney nickel, although milder conditions are often operative. The products are toluene and an alcohol, and the ease of removing benzyl groups in this manner has led to their use as protective groups for hydroxyls (and also amines):

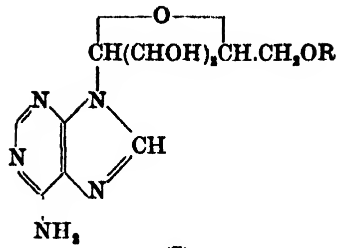
$$ROCH_2Ph \xrightarrow{H_2} ROH + PhCH_3.$$

There are many examples of the use of protective benzyl groups, but an outstanding example has been the methods developed at Cambridge by A. R. Todd and his colleagues for phosphorylations, which have culminated in the syntheses of muscle adenylic acid, adenosinediphosphate (ADP) and adenosinetriphosphate (ATP) (Baddiley, Todd *et al.*, *J. Chem. Soc.*, 1947, 674; 1949, 582; *Nature*, 1948, **161**, 761). Dibenzyl chlorophosphonate has been developed as the phosphorylating agent, and with alcohols in the presence of a tertiary base it yields dibenzylphosphoric esters from which the benzyl groups may be removed by hydrogenolysis. Similarly amines and phenols may be phosphorylated (Todd *et al.*, *J. Chem. Soc.*, 1945, 382, 660; 1946, 652; 1947, 674):



By this method, adenosine (I; R = H), after protection of the hydroxyls of the ribose side-chain as the *iso*propylidene compound,

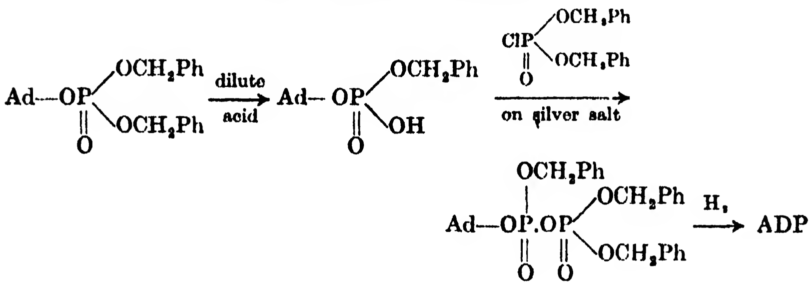
was converted into muscle adenylic acid :



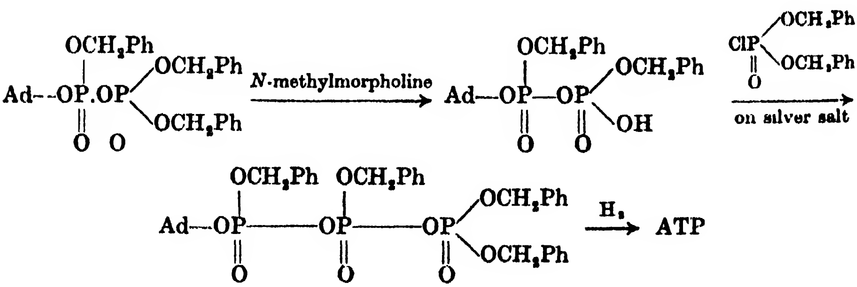
(I)

This method of phosphorylation has the great advantage that the protective benzyl groups may be removed stepwise by hydrogenolysis, hydrolysis or by treatment with a strong tertiary base and a partial hydrolysis was used in the conversion of the dibenzyl derivative of muscle adenylic acid to ADP:

(Ad = Adenosine residue.)



The synthesis of ATP depended on the observation that the neutral benzyl esters of phosphorous acid, phosphoric acid or pyrophosphoric acid reacted with strong tertiary bases, *e.g.* *N*-methylmorpholine, to yield quaternary salts containing an *N*-benzyl group in the cation, and the decomposition of these salts gave acid esters containing one less benzyl group. Thus the tribenzyl derivative of ADP was converted to ATP:



The product was isolated as the barium salt and purified as the acridine salt.

With regard to the *N*-benzyl compounds, these have also been used as protective groupings, *e.g.* for the preparation of secondary amines (Buck and Baltzly, *J. Amer. Chem. Soc.*, 1941, **63**, 1964):

$$PhCHO + RNH_2 \longrightarrow PhCH{:}NR \xrightarrow{H_2} PhCH_2NHR \xrightarrow{R'Br}$$
$$PhCH_2NHRR'\}Br \xrightarrow{\text{heat}} PhCH_2NRR' \xrightarrow{H_2} NHRR'$$

The same authors (*ibid.*, 1943, **65**, 1984) have investigated the use of alternative protecting groups for amines, but found that substitution in the benzyl group led to increased stability and the use of certain more labile groups was offset by increased difficulty of preparation, *e.g.* *p*-phenylbenzyl. The carbobenzoxy group, $—COO.CH_2Ph$, is fairly well recognised as a protecting group for amines, especially for peptide syntheses. The reagent, carbobenzoxy chloride, is prepared by the action of phosgene on benzyl alcohol, and the method was introduced by Bergmann and Zervas (*Ber.*, 1932, **65**, 1192). In the case of sulphur-containing peptides, *e.g.* glutathione, the reductive fission was effected with phosphonium iodide or with sodium in liquid ammonia.

Other aspects of the hydrogenation and reduction of specific groupings will be dealt with in a later report.

GEOLOGY.

By G. W. Tyrrell, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

Stratigraphical and Regional Geology.—J. G. C. Anderson has performed a useful task in systematising the involved and inconsistent "Stratigraphical Nomenclature of Scottish Metamorphic Rocks" (*Geol. Mag.*, LXXXV, 1948, 89–96). For the main divisions he adopts the non-committal terms Lewisian, Moinian, and Dalradian Metamorphic Assemblage respectively, thus discarding the nouns such as "system," "series," "schists," "gneisses," etc., previously used. He then correlates in a tabular manner the subdivisions employed for different regions—the North-West Highlands, Morar, Ross-shire, and Grampian Highlands; but for the Dalradian, with its wealth of subdivisional names, he utilises a separate table. This work should put Highland stratigraphical geology on a new and better basis.

The second of Dr. A. E. Trueman's Anniversary Addresses as President of the Geological Society of London dealt with "Stratigraphical Problems in the Coalfields of Great Britain" (*Quart. Journ. Geol. Soc.*, CIII, 1947, lxv–civ). His first address summarised the main features of the history and palæogeography of

Coal Measures times in Northern Europe and North America. In the light of principles and facts established in that address Dr. Trueman has now attempted to elucidate the relations of the Coal Measures, including the Millstone Grit, in Great Britain. While it is impossible to summarise this vast mass of arranged and digested information here, we may reinforce Dr. Trueman's plea for exploration by deep borings of the distribution of possible concealed coalfields in the "unknown triangle," the corners of which lie at Bristol, London, and South Staffordshire. Such a programme, in Dr. Trueman's words, "would not necessarily be unrelated to present necessities," and its plan of work, when formulated, would have to be guided by Dr. Trueman's address.

Two valuable symposia on the problems of the American Carboniferous were presented in 1946 and 1947 to the American Association for the Advancement of Science at St. Louis and Chicago respectively. These papers have been published as follows: H. R. Wanless *et al.*, "Symposium on Pennsylvanian Problems" (*Journ. Geol.*, LV, 1947, 183–308); J. M. Weller *et al.*, "Symposium on Problems of Mississippian Stratigraphy and Correlation" (*ibid.*, LVI, 1948, 253–402). The papers contributed by the numerous authors were solicited and chosen by the editors to include subjects of a wide geological and geographical range, especially surface stratigraphy, subsurface correlation, palæogeography, classification, and palæontology.

O. T. Jones's paper "On the so-called Metamorphism of the Trias in the Alps" (*Geol. Mag.*, LXXXV, 1948, 333–7) elucidates a paper by M. Lugeon with a similar title ("A propos du prétendu metamorphisme du Trias autochthone alpin," *Bull. Géol. Soc. France*, (5), XVI, 1946, 609), and refers to similar phenomena in this country. The basal Trias rests with great unconformity on ancient schists, gneisses and granites around the Aar Massif and the Aiguilles Rouges. A reddening effect is seen in the rocks underlying this and similar unconformities, whether metamorphic or not, and is attributable to contemporaneous weathering. In the Scheidnössli section the unconformity is marked by a peculiar arkose about 12 ft. thick, which grades downwards into a gneiss, but is sharply truncated above by the base of the Trias. This is interpreted as due to "disintegration *in situ*." A new interpretation by Perrin and Roubault claims that this arkose is a product of post-Triassic metamorphism, which is also presumably responsible for the production of the gneiss below; and that it is due to granitisation working up from below. The solutions, however, were blanketed off by the Trias which is assumed to have acted as

an impermeable barrier. The universal opinion of stratigraphers is that this view, to say the least, is highly improbable. Several of them use much stronger terms.

In their paper entitled "Flysch and Molasse," A. J. Eardley and M. G. White (*Bull. Geol. Soc. Amer.*, **58**, 1947, 979–90) have discussed the use of these terms in relation to North American stratigraphy. European geologists apply the term *flysch* to sequences of limestones, sandstones, and shales in thin, regular, alternating beds, which are deposited in a geosyncline or foredeep shortly before a major orogeny. The term *molasse* applies to the arkosic sandstones with lenses of conglomerate deposited on the flanks of rising mountains after rapid erosion. These terms are therefore used to designate sedimentary assemblages of certain lithologies and related to orogeny; but in practice they are mostly used as formational names. The authors are of opinion that the terms are of no value in American stratigraphy.

J. G. C. Anderson's paper on "The Occurrence of Moinian Rocks in Ireland" (*Quart. Journ. Geol. Soc.*, CIII, 1948, 171–90) deals with a large area of metamorphic rocks north of L. Erne in North-West Ireland. The succession of metamorphic sedimentary rocks begins with a psammitic group (L. Derg), followed by a pelitic group (Oughtadreen), and a Calcareous group, with altered igneous rocks represented by epidiorite and amphibolite. The evidence supports the view that the L. Derg group, mainly granulites, is of Moinian age, the other two being Dalradian. An injection complex similar to those of the Scottish Highlands forms kyanite, garnet, and biotite zones related to a belt of intense pegmatitisation. Structurally the metamorphic rocks form the L. Derg Anticline, a Caledonian fold with a north-easterly pitch.

In 1939 Dr. J. E. Richey and Professor W. Q. Kennedy found what they called a Sub-Moine Series lying below, and separated by a discordance from, the ordinary Moine metamorphic sediments. The Sub-Moine consisted of psammitic and pelitic rocks differing somewhat from the normal Moines, and were associated with hornblendic orthogneisses. As a result of further work Dr. A. G. MacGregor points to "Resemblances between Moine and 'Sub-Moine' Metamorphic Sediments in the Western Highlands of Scotland" (*Geol. Mag.*, LXXXV, 1948, 265–75), and claims that certain criteria which Richey and Kennedy had used to distinguish the Sub-Moines from the Moines, can all be found locally in the adjacent Moines, and are not infrequently absent from the Sub-Moines. Hence, for diagnostic purposes, these criteria are valueless, and MacGregor suggests that the Sub-Moine sediments of Morar con-

stitute a lower part of the Moine Series which is associated with hornblendic orthogneisses.

O. T. Jones describes "The Geology of the Silurian Rocks West and South of the Carneddau Range, Radnorshire" (*Quart. Journ. Geol. Soc.*, CIII, 1947, 1–36). The Carneddau Range is composed of Ordovician igneous and sedimentary rocks. In this paper the geological succession from Upper Llandovery to Lower Ludlow in the region to west and south of the range is dealt with. The Llandovery is developed as an eastern facies of calcareous sandstones (*Pentamerus oblongus*) and a western facies of green mudstones and dark shales (*Monograptus crenulatus*, etc.). In the Upper Wenlock and Lower Ludlow there are important developments of slumped mudstones. Structurally the area is a shallow syncline traversed by a series of powerful faults trending mainly in the strike direction.

From a critical examination of the literature and of the fossil faunas of the Silurian inliers of the Pentland Hills, A. Lamont (*Geol. Mag.*, LXXXIV, 1947, 193–208; 289–304) concludes that the horizons represented there are of earlier age than has been realised. The new synchrony proposed by Lamont is mainly with the Gala-Tarannon. The fossils represent neritic and freshwater faunas in formations which are thick in the north, but which thin out southward. There is evidence of the emergence of Gala-Tarannon islands in the Pentland and Melrose areas.

The earth movements responsible for "The Structure of the Coal Measures in the Pontardawe-Ammanford Area," described by F. M. Trotter (*Quart. Journ. Geol. Soc.*, CIII, 1947, 89–134), are referable to three tectonic episodes. The first is the Armorican orogeny which gave rise to differential northward movements of the strata, observable along north-north-west-trending tear-faults. The strata were forced against the pre-existing Towy Anticline, and the compression belts thus produced consist of folds, thrusts, and lag-faults. The second episode was a period of tension during which a second system of north-north-west faults was developed. In the final tectonic episode the structures of the two preceding episodes were moved laterally by the north-east-trending Tawe Valley tear-fault.

The discovery of a shallow oilfield in the Trias west of Formby led to an attempt to determine by boring the nature of the local Carboniferous rocks which were regarded as the most probable source of the oil. The deepest boring (7680 ft.) in Britain up to the present was the result. It is described by P. E. Kent ("A Deep Borehole at Formby, Lancashire," *Geol. Mag.*, LXXXV, 1948, 253–64). The boring started in the Keuper and ended in Lower

Carboniferous shales. The Permo-Triassic rocks proved to be much thicker than was forecast. They rested on early Millstone Grit strata notable for their induration and their unusual facies. A remarkable feature of the Formby succession is the extent of the Pre-Permian uplift and erosion. The facts indicate that the area had been raised 8000 ft. vertically with respect to the Manchester area and about 7000 ft. of sediments removed before subsidence again began.

As a result of stratigraphical studies it was concluded in 1940 that there was a considerable development of Mesozoic rocks in North Norfolk and a probability of the presence of Carboniferous rocks (1946). The D'Arcy Exploration Co. thereupon put down a boring to test the Pre-Mesozoic rocks, and the paper " A Deep Boring at North Creake, Norfolk," by P. E. Kent (*Geol. Mag.*, LXXXIV, 1947, 2–18) is a description and discussion of the results of this boring. The forecast for the Mesozoic rocks was accurate in considerable detail, but Palæozoic rocks were absent, the Trias resting directly on a Pre-Cambrian basement comparable with the tuffs and agglomerates of the Charnwood area. This indicates that North Norfolk overlies a broad Pre-Mesozoic " Schwelle " which extends to the northward.

D. F. W. Baden-Powell's examination of " The Chalky Boulder Clays of Norfolk and Suffolk " (*Geol. Mag.*, LXXXV, 1948, 279–96) has shown that Harmer's theory of a single ice-sheet is unsatisfactory. There is clear evidence of two separate " chalky boulder clay " ice-sheets which differed somewhat in their directions of movement. In the Lowestoft or " Lower Chalky " glaciation there were apparently two main ice-sheets which converged in Norfolk, a fact which may help to explain the famous " contortions " in the Cromer boulder clay.

" Ancient Arctica," a term applied to the lands and seas of the past in the Arctic region, is the subject of an illuminating paper by A. J. Eardley (*Journ. Geol.*, **56**, 1948, 409–36). A survey was made of the shields that are circumferential to the Arctic Ocean, the intra-shield orogenic belts, the troughs of deposition and orogenic belts of Alaska, and of the topography of the floor of the Arctic Ocean. It is concluded that the region is underlain by continental crustal material of shield character, and that the present deep basin began to sink in Carboniferous times. The region Alaska to North-Eastern Siberia was one of nearly continuous land connection during Mesozoic and Cainozoic time, and probably during the Palæozoic. The crust underlying the North Atlantic and Greenland Sea underwent Tertiary movements of sufficient magnitude to

provide land bridges, possibly on several occasions, between Europe and North America.

The work of H. C. Cooke, "Problems of Sudbury Geology" (*Canada: Geol. Surv. Bull.* No. 3, 1946, 77 pp.), has cleared up many of the discrepancies that exist between earlier accounts of the region. Most of the formations are now relegated to the Lower Huronian. Above this, and still Pre-Cambrian, is the Whitewater Series of Keweenawan age. New formations below the Huronian have been discovered, but their position in the stratigraphical column is otherwise quite unknown. A comparative stratigraphical table (omitting igneous rocks) elucidates the views of various writers on the region.

H. N. Fisk has published a large memoir on the "Geological Investigation of the Alluvial Valley of the Lower Mississippi River" (conducted for the Mississippi River Commission, Vicksburg, Miss., 1944. *War Dept. Corps of Engineers, U.S. Army*, 4to, 78 pp. Atlas, 33 plates, 11 tables, 80 figs.). The writer has not seen this work, but in a lengthy review W. H. Hobbs (*Journ. Geol.*, LV, 1947, 378–80) refers to: "This superb work, of which . . . only 100 copies were printed, is monumental in its importance in the field of geology. Many of the plates are coloured maps a square yard or more in area, and the data recorded on them are not alone from surface observations, but include those from many thousands of deep-well borings." A long and complicated geological history has been revealed. The solid rock basement is intricately fractured giving a rectangular pattern, and this pattern is reflected by the orientation of streams, both tributary and distributary.

The "Geology of Tobago, British West Indies" is comprehensively surveyed by J. C. Maxwell (*Bull. Geol. Soc. Amer.*, **59**, 1948, 801–54). The oldest rocks exposed are strongly folded schists and phyllites consisting mainly of metavolcanic material which are believed to be of Cretaceous age. This schist belt occupies the northern part of the island. South of it is a sheared but relatively unaltered igneous complex consisting of andesitic and basaltic lavas with intrusions of diorite and ultrabasic rocks. This activity belongs to the early Tertiary. Miocene–Pliocene sands and clays rest unconformably on the older series and coral limestones overlap all other rocks. Tobago has probably been a land area since the late Cretaceous.

M. I. Attia has published a memoir on the "Geology of the Barramiya Mining District" (*Geol. Surv. Egypt*, Cairo, 1948, 76 pp.). The area is in the eastern desert of Egypt between the Nile and the Red Sea. The rocks are mainly Pre-Cambrian schists, marbles,

and amphibolites intruded by huge masses of dunite and peridotite, largely transformed into serpentine, which form mountains. Granitic and doleritic dikes cut the older rocks. The Nubian Sandstone of Cretaceous age rests on the ancient basement. Chromite occurs as lenses, veins, and rounded masses in the serpentine and is of commercial importance.

The memoir by H. Bütler, modestly entitled "Notes on the Geological Map of Canning Land (East Greenland)" (*Medd. om Grönland*, 133, No. 2, 1948, 97 pp.), is really a detailed work on the general geology of Canning Land. The rock succession begins with a Pre-Cambrian series (Eleonore Bay) which is intruded by a late-Caledonian granite. The greatly eroded Caledonian folds are unconformably overlain by a volcanic series (C. Fletcher) consisting of porphyry lavas and tuffs, which is of early Devonian age. It is in turn overlain by sandstones of the Middle Old Red Sandstone, Upper Devonian, a continental Carboniferous series, and Middle Permian, all of which are separated by small unconformities. In Triassic times sandstones and arkoses of New Red Sandstone type were deposited in a broad depression. The memoir is built round an excellent coloured map and an illuminating stereographic diagram of the structures.

The work by H. Bütler entitled "Die Westgrenze des Devons am Kejser Franz Joseph Fjord in Ostgrönland" (*Mitt. Naturf. Ges. Schaffhausen*, XXII, 1948, 73–152) is a study of geological profiles on the western margin of the Old Red Sandstone area in Ymer Island and Strindberg Land. The region is situated in the northwestern part of the "geological depression" of Royal Bay, in which a coastal strip, 80–100 km. wide, has subsided with reference to the interior of the continent. It is bounded on the west by a great fault ("Highland Boundary Fault") the escarpment of which can be followed for 350 km. Sandstones and conglomerates of the Old Red Sandstone have been deposited in the trough which is situated in the interior of a Caledonian folding zone. This deposition is regarded as a consequence of strong dislocations in the Caledonian mountain ranges.

L. R. Wager continues his series of "Geological Investigations in East Greenland" with a Part IV dealing with "The Stratigraphy and Tectonics of Knud Rasmussens Land and the Kangerdlugssuaq Region" (*Medd. om Grönland*, **134**, No. 5, 1947, 64 pp.). The main rock groups in this region of 30,000 sq. km. are: (1) The basement complex of metamorphic rocks, almost certainly Pre-Cambrian; (2) the Kangerdlugssuaq Sedimentary Series of conglomerates and sandstones about 600 ft. thick and of lowest

Tertiary age, which rest unconformably on the basement complex; (3) a great series of Tertiary "plateau basalts" more than 6 km. thick; and, finally, a series of large and small Tertiary intrusions which cut the lavas. The main tectonic feature is a great monoclinal flexure which follows the coast for 350 km. in a general north-east to south-west direction, and is associated with a large dike-swarm. Professor Wager also contributes valuable chapters on hypotheses in explanation of the Thulean tectonics of East Greenland, and on Thulean tectonics in relation to the formation of the North Atlantic.

As in previous "Advances" (*e.g.* SCIENCE PROGRESS, Oct. 1947, 703) we have to chronicle several of the useful "outline" compilations of the geology of more or less restricted regions, in which so often the latest work on the subject is presented to the geological public. Such are:

"Geology of Florida," by C. W. Cooke (*Florida Geol. Surv. Bull.*, **29**, 1945, 339 pp.), prepared in co-operation with the U.S. Geol. Survey. Review by G. G. Parker (*Journ. Geol.*, LV, 1947, p. 120).

"The Geology and Mineral Resources of British Guiana," by S. Bracewell (*Handbook of Natural Resources of British Guiana*, (Chap. IV, 1946, 20–38).

"Outline of the Geological History of Spitsbergen," by A. K. Orvin (*Norges Svalbard og Ishavs-Undersök. Skr. om Svalbard og Ishavet*, Oslo, No. 78, 1946, 57 pp., 4 plates and map).

"The Geology and Mineral Resources of Kenya," by W. Pulfrey (*Bull. Imper. Inst.*, XLV, 1948, 277–99).

"An Outline of the Geological History of Southern Rhodesia," by A. M. Macgregor (*Geol. Surv. Southern Rhodesia, Bull. 38*, 1948, 73 pp.).

"Report on the Geology of Basutoland," by G. M. Stockley (*Basutoland Govt., Maseru*, 1947, 114 pp.).

BOTANY. By PROFESSOR W. H. PEARSALL, D.Sc., F.R.S., University College, London.

MINERAL NUTRITION.—The Masters Memorial Lecture of the Royal Horticultural Society in 1948 was given by Dr. T. Wallace on the nutritional problems of horticultural plants with special reference to trace elements (*J. Roy. Hort. Soc.*, **73**, 366 and 423, 1948). In this published account, Dr. Wallace points out that, in addition to the six major elements required by plants, six other "trace elements" have been proved to be required in small amounts,

iron, manganese, copper, zinc, boron and molybdenum, while others like sodium, chlorine, silicon, aluminium and nickel are often beneficial, as for example with sugar beet, which gives better growth if sodium is supplied. The effects of deficiencies and excesses of the different necessary elements are described in detail and a good deal of attention is given to the conditions under which these effects are found in nature. Thus manganese deficiency is commonly found in alkaline soils with impeded drainage—especially if they are peaty—as well as on over-limed and dunged garden soils. Copper deficiency is common on heather-covered moorland soils, while acid soils in general may show a variety of deficiency and excess effects of various sorts.

An important part of the problem is that different crop plants have different requirements. Thus black currants have a high nitrogen requirement while pears are particularly liable to iron deficiency. Carrots and parsnips are very sensitive to any scarcity of potassium. Calcifuges and calcicoles are generally complex in their soil demands. Calcifuges, for example, while flourishing at low concentration levels for soil cations like calcium, and generally at high iron concentrations, must tolerate or even require high concentrations of hydrogen, manganese and aluminium ions, the latter of which accumulate in a remarkable way in certain calcifuge species. In general, calcicole species have requirements of the opposite type and frequently also high magnesium and potassium demands.

It is obvious that knowledge of the special requirements of different species is of great value in attempting to assess the nutrient status of a particular soil and a useful summary in tabular form is given of crop plants which have indicator value in this respect.

The lecture concludes with an account of the methods useful in combating the effects of unfavourable mineral status. The application of manures is not always effective and for certain of the trace elements, particularly iron and manganese, treatment by other means is necessary, such as by foliage sprays or direct injection.

Practical problems of mineral nutrition are further discussed by D. S. Riceman (*Council Sci. Ind. Res.* (*Australia*), *Bull.* **234**, 1948) in dealing with the manurial treatments necessary on two sandy soils on the Ninety-Mile Plain, South Australia. One of these is siliceous and the other calcareous. On the former, phosphate manuring is essential, and then both zinc and sometimes copper give additional beneficial effects. On the calcareous soil, addition of copper is essential before almost any other treatment, though for cereals nitrogen may be almost equally necessary.

Leguminous plants require copper and then also zinc. Both soils become so productive after treatment as to justify the expense of the heavy manurial dressings required.

The theoretical aspects of mineral salt absorption have recently received further investigation by R. N. Robertson, J. S. Turner and M. J. Wilkins (*Austral. J. Exp. Biol. Med. Sci.*, **25**, 1, 1947) who have examined the effects on respiration of salt accumulation from potassium chloride solutions by sliced beet tissue. They find that respiration increases when well washed slices are put into salt solution (0·02*M*). Both this " salt respiration " and salt accumulation are inhibited by the addition of potassium cyanide (0·01*M* and 0·001*M*). Cyanide also inhibits the wound respiration which is a marked feature of the first 100 hours after cutting. The results thus bear a general resemblance to earlier results with sliced carrots. Very similar results are recorded by J. Milthorpe and R. N. Robertson (*ibid.* **26**, 189, 1948) for barley roots cut into 1 cm. fragments for estimations with Warburg manometers. The results as a whole thus agree with those of Lundegardh (*Ann. Rev. Biochem.*, **16**, 503, 1947), although different methods and materials have been employed.

The comparison with Lundegardh's hypotheses is further elaborated by R. N. Robertson and M. J. Wilkins (*Austral. J. Sci. Res.*, *B*, **1**, 17, 1948), who have studied the effect of salt concentration on salt accumulation and salt respiration. They used chlorides, mainly potassium chloride, and sliced carrots and found that as the salt concentration rises the ratio between salt accumulation (g. mol. monovalent salt) and salt respiration (g. mol. oxygen utilised) approaches a limiting value of 4. Since each molecule of oxygen requires 4 hydrogen ions and 4 electrons to convert it to water, assuming a cytochrome type of respiratory system, it is argued that each electron leaving this system could thus exchange for or balance an anion from the external solution, if the Lundegardh theory is correct. Similarly, the cations would be assumed to exchange with hydrogen ions. A full discussion of the difficulties involved in subjecting the hypothesis to quantitative test is given.

Yeast Cytogenetics.—Interest in the cytological and genetical characters of yeasts continues to develop, although progress is slow on account of the difficulty of interpreting the structural peculiarities of the yeast nucleus and also because of its small size. M. K. Subramaniam has tackled respectively the cytology of a distillery yeast and its genetical interpretation in two papers (*Proc. Nat. Inst. Sci. India*, **12**, 143, 1946, and **13**, 129, 1947). The yeast investigated had initially two chromosomes, but poly-

ploid forms were found, some of which have been isolated and maintained in culture. Most of the forms with varying chromosome number which have been recorded disintegrate after a few divisions and are not perpetuated. The cytological effects of treating the two-chromosome yeast with acenapthene are recorded in the second paper mentioned above. Haidenhain's hæmatoxylon was used as a nuclear stain, although other staining techniques are said to give similar results. A weakly-staining or chromophobic region occupies the centre of each mass of nuclear material or "chromosome" after treatment and this is considered to correspond to heterochromatin. The other effect of acenapthene is to produce polyploid forms, a tetraploid strain with some degree of permanence, resulting from prolonged treatment. It is considered that the facts would agree with the appearance of heterochromatin along with the production of polyploids, most of which are non-viable. In a further note (*Current Sci.*, **16**, 157, 1947) attention is drawn to the cytological differences between cells of brewer's yeast in the growing and in the fermenting stages of development. In the strain with two chromosomes (mentioned above) aerobic and growing cells show normal mitosis and cell-division. Fermenting cultures on the other hand, taken and re-inoculated into fresh medium, show many dying cells as well as those capable of division. Among the latter, normal division stages are rare and the cells show a preponderance of varying degrees of polyploidy, along with budding, in which most of the cells are left with irregular numbers of the nuclear fragments which are regarded as chromosomes. Thus, though a small percentage of the cells retain some power of normal vegetative growth, the usual products of cell multiplication in fermenting cells apparently have no genetical future at all.

From among the brewer's yeasts strains observed in this work, M. P. Bai and M. K. Subramaniam (*Curr. Sci.*, **16**, 380, 1947) observed one in particular, a diploid, which later gave rise during culture to a tetraploid form. The giant colonies produced by these yeasts on plate cultures have normally a characteristic form, the diploid giving folded and striated colonies of distinctive appearance. The tetraploid appeared first as a smooth sector in one of these giant colonies, and it retained this feature in giant colonies formed after its isolation. An acenapthene-induced tetraploid had a similar colony form. On mixing approximately similar cultures of this latter strain with the original diploid and plating, smooth giant colonies were obtained, indicating that the tetraploid had a higher growth rate under these conditions. A subsequent investigation by K. K. Mitra (*Curr. Sci.*, **17**, 55, 1948) has shown

that in the acenapthene-induced tetraploid a higher chromosome number is also associated with a higher rate of fermentation as compared with the original diploid strain. The final concentration of alcohol produced was, however, similar in both.

The possibility of gene mutation in these brewer's yeasts is also considered to have been verified. M. K. Subramaniam and S. N. K. Murthy (*Curr. Sci.*, **17**, 92, 1948) believe that they have observed the reversal of an observed mutation. A diploid strain which produced giant colonies of a given type was observed to produce quite suddenly colonies of a different appearance, more striated and with a rougher texture, although no cytological differences could be observed in the cells. It was therefore thought that a gene mutation was involved. Actively growing cultures of this strain with new colony form were then exposed in clear silica tubes to ultra-violet irradiation (4 hours at 90 cm. from a mercury arc lamp). Twenty-four hours later the treated cultures were plated, when one colony in nine examined gave the original form. Later a spontaneous reversal of the new rough-colonied type to the original smooth strain was also observed. Reversal of the original mutation thus took place naturally as well as being induced by ultra-violet irradiation. The authors consider that this confirms the idea that the changes were due to alterations in the alleles at a particular locus.

INTERESTING PLANTS.—In the new journal, *Watsonia*, **1**, 24, 1948, W. A. Sledge describes the distribution and ecology of *Scheuchzeria palustris*. This rare species in Britain is apparently now confined to its Scottish stations in the vicinity of the Blackwater valley and Rannoch Moor, although within the last century it was present and recorded in at least four English vice-counties, and formerly it must have been abundant in mosslands like those of the Somerset levels—whence *Scheuchzeria* peat has been collected by A. R. Clapham and H. Godwin—as well as in some of the Shropshire peatmosses. The plant is characteristic of the wetter parts of base-poor peatmosses and its habitats on Rannoch Moor appear to be typical of those recorded elsewhere in Europe and North America, in spite of the altitude (1100 ft.). A striking feature of the Scottish localities is its almost constant association with *Carex limosa* and *Sphagnum inundatum*, while other species frequently found with it include *Rhynchospora alba*, *Carex lasiocarpa*, *Narthecium ossifragum* and *Drosera* spp.

Various North American and Asiatic fossils which were formerly ascribed to *Sequoia* were proved not to belong to that genus as originally described, and in 1941 the new genus *Metasequoia* was

formed to accommodate this group of fossil conifers. Only four years later a living conifer was found by T. Wang in China, hitherto undescribed, which must also be put into this genus. The living trees, comparatively few in number, have been found in an inaccessible area in north-eastern Szechuan, very close to the Hupeh border. A grant provided by the Arnold Arboretum enabled an expedition to be organised, which brought back seeds from which plants raised at Kew have already been distributed. The curious features of the distribution in time and place are paralleled, to some extent, by the Maidenhair Tree, *Gingko biloba*, among gymnosperms. There is also the monotypic genus *Platycarya* of the Walnut family (*Juglandaceæ*) in eastern Asia, which is also found as a fossil in the flora of the London Clay.

Details of the discovery of the new *Metasequoia*, which is to be described as *M. glyptostroboides*, were given by E. D. Merrill in *Arnoldia*, **8**, 1, 1948, and the paper has been reproduced with illustrative plates in *J. Roy. Hort. Soc.*, **73**, 211, 1948. When full grown the plant is over 100 ft. high, with a trunk diameter of 7 ft. or more. As in swamp cypress (*Taxodium*), the leaves are deciduous and the leafy branches are flattened, the leaves being arranged in two rows.

The Cinchona plant from which Peruvian Bark and the drug quinine is obtained must rank as one of the most notable of those which the New World has given to the Old, for, until recent years, it was the only effective remedy for malaria, which attacks some 800 million people each year. The plant owes its name to Linnæus, who, misinformed as to the correct spelling of a name, called it after a Countess of Chinchon whose legendary cure by the bark had attracted his attention. J. Jaramillo-Arango (*J. Linn. Soc. Lond.*, **53**, 272, 1949) has recently re-analysed at length the evidence about the early use and history of Cinchona, and has shown that the story about the cure of the Countess is entirely unsupported by evidence, and is not even credible, for her husband, who was Viceroy of Peru from 1629 to 1639, was also continually languishing from the disease and his diary though frequently referring to illness, contains no mention of any such method of cure. There does exist a record, however, that Father Calancha in 1633 was writing that the bark had " produced miraculous cures " in Peru and it seems curious that no attempt appears to have been made to administer it to the Viceroy.

Even the name quinine seems to owe its origin to a confusion. The early Spanish-American accounts describe two trees of medicinal value, the Peruvian Balsam (*Myroxylon peruiferum* of the

Linnæan classification), which was called *quina-quina* by the natives and which yielded a "marvellous oil" and *Cascarilla del Loja* which described the tree yielding the "fever-bark" from the Province of Loja or Loxa. It seems clear from the description that the latter was Cinchona as we now know it and that Europeans subsequently mixed up the names of the two trees and their barks, as they did their curative properties. Thus Cinchona fell into complete disrepute for a time in Europe, apparently owing to the use of the wrong bark.

Even the name "Jesuit's Bark" contains some elements of uncertainty, though it appears to be associated with the Jesuit Cardinal de Lugo, who apparently contributed greatly to the spread of knowledge of its properties about the time of the assembly of the General Council of the Order of Jesus in 1649. No doubt this accounts for the other name, "Cardinal's Bark," used in the sixteenth and seventeenth centuries.

Before the war, nearly 90 per cent. of the world's quinine came from Java and from the greatly improved *Cinchona ledgeriana*, called after Charles Ledger, who after years of search obtained the seeds (of *C. calisaya*) in 1864 from which the modern plants have been developed and the Java plantations stocked.

ZOOLOGY.

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Population Ecology

One of the most significant features of the history of biology in the past 100 years has been the emergence of a wholly new complex of investigations concerned with the properties of biological populations as such. The theories of Malthus and Darwin on the one hand and of Mendel and his successors on the other, with their union in the modern genetical theory of natural selection, resulted from the combination of statistical with biological concepts. As a result of the statistical outlook common to these theories it is the population and not the individual that is at the focus of attention.

This approach suggests to most biologists the concepts of "natural selection," "the gene complex" and "the new systematics," but it is the purpose of this short review to emphasise a further branch of population biology which owes its origins just as much to Darwinian theory but has developed in another direction, mainly concerned with the size and composition of populations of different species and their interrelations.

Within this field of "synecology," we can distinguish three different types of approach and, less clearly perhaps, at least four levels of investigation. Firstly, a number of investigators, frequently not biologists but mathematicians and demographers, have considered from a purely theoretical or mathematical point of view the effects of variation of simple conditions on different population systems. Secondly, various workers, either in order to investigate the applicability of the ideas of the theorists or in order to try to abstract and simplify from more complex natural conditions, have worked with experimental populations in the laboratory. And, thirdly, population research in the field has been undertaken by economic biologists interested in exploiting or controlling populations, by naturalists, and by "pure" scientists directly interested in the factors influencing the size of populations and its variations. At all these levels one can distinguish (*a*) *intra*specific studies—those confined to populations of a single species; (*b*) those *inter*specific studies which concern two or more species as potential competitors for a common habitat or at least belonging to a single "trophic level"; (*c*) *inter*specific studies on the relations between animals and plants belonging to different "trophic levels"; these are usually studies of the interactions between populations of predators and their prey, or parasites and their hosts; (*d*) studies which attempt to synthesise the analytical results of the foregoing or, by their own methods, to build up a quantitative picture of the living community as a whole.

None of these is exclusive of the others, but each has the virtue of concentrating attention on the sorts of "ties" which pervade the structure of an organic community and are responsible for its peculiar properties. The utility of the organic community concept is still disputed by a few biologists who would deny that it exhibits features which could not be predicted from the characteristics of the biology of its component individuals (*see*, for example, Bodenheimer, *Problems of Animal Ecology*, 1938). But the range of achievement of these community studies would seem to belie such an attitude on heuristic grounds, while few can be found to-day to defend it on grounds of logic. Indeed, it is only with the help of such community concepts that the ecologist is able to build up an unusually complex mass of data (complex even by biological standards) into an orderly picture of the community as a whole.

INTRASPECIFIC POPULATION STUDIES.—The mathematical work of Lotka in the 1920's was responsible for drawing biologists' attention to the applicability to intraspecific studies of the demographic methods used by actuaries and others interested in the

"life table" of man. Lotka has recently written on this subject (*Essays on Growth and Form*, Oxford, 1945) and has published a number of more mathematical papers, while Leslie has introduced new methods and useful adaptations to animal populations. The first population study of this sort is that of Leslie and Ranson (*J. Anim. Ecol.*, 1940, **9**, 27), and Leslie has since described a number of improvements in technique (*e.g. Biometrika*, 1945, **33**, 183; *ibid.*, 1948, **35**, 213; *J. Roy. Stat. Soc., A.*, 1948, **111**, 49). The general result of this demographic influence has been a considerable clarification of the meaning of the term "growth rate" of a population, the acceptance of the "intrinsic rate of natural increase" as the most useful index of growth rate, an appreciation of the importance of the life table in considering the age distribution of a population and the linking of the "intrinsic rate of natural increase" to the "stable age distribution." Such techniques would appear to have great relevance to the quantitative study of food consumption and "energetics," but there are no signs as yet that their significance is sufficiently appreciated.

A recent encouraging sign of a spread in the use of demographic techniques is the publication by L. C. Birch of a paper (*J. Anim. Ecol.*, 1948, **17**, 15) on the intrinsic rate of natural increase of an insect. This paper, incidentally, can be recommended for a very clear explanation of the use and calculation of this important parameter. The sort of result which Birch obtains and which would certainly not be obvious from a cursory examination of the figures of egg-laying rates is that, in the species considered, the eggs laid during the first two weeks of life are much more important than those laid during the remaining ten or so weeks in determining the intrinsic rate of natural increase; the economic implications of such conclusions are obvious.

A number of workers setting out to investigate experimentally the interactions of populations of two or more species have been forced back to *intra*specific studies by the peculiar behaviour of the single species. This has happened to Park in his work on *Tribolium* (flour beetle) populations and to Crombie working also on flour beetles. (T. Park, a series of papers in *J. exp. Zool.* and other American journals, 1932–41; *Ecological Monographs*, 1948, **18**, 265. A. C. Crombie, *J. exp. Biol.*, 1941–43; *Proc. Roy. Soc. B.*, 1945, **132**, 362; *J. Anim. Ecol.*, 1947, **16**, 44.) Early work was concerned with the influence of population density on fecundity, mortality and similar factors, and on the relation of food supply, cannibalism, chemical "conditioning," etc., to the size of population attained. The effects of eliminating the various limiting factors

are described. Park placed the whole problem on a new basis when, in his later experiments, he showed that all laboratory populations of *Tribolium* are infected with Protozoan parasite. By elaborate precautions this was eliminated, and the result was that the population size attained by *T. castaneum* increased markedly, but that of *T. confusum* remained at the same level, as if the parasite were still present; the effects of these peculiarities on the outcome of competition are discussed below. Various other biological characteristics are modified in the presence of the parasite and the general impression given from these studies is that a single species population is far from a simple system.

Field studies of single species populations, because of the impossibility of isolating such populations from those of their competitors, predators and prey, cannot usually be separated from studies of interactions between populations and will therefore be discussed below. The type of work which is due to Lack and his colleagues (*e.g. J. Anim. Ecol.*, 1948, **17**, 45) on the significance of clutch size in birds might logically be included here, but, although these studies are of the greatest significance for evolutionary theory they can only be mentioned in a brief review such as this; the same remarks apply to work on territory and other "population limiting" devices.

Interspecific Population Studies.—If the most significant *intra*specific theoretical advances have been with the help of more novel techniques, those of interspecific studies revert to ideas mainly connected with Darwinian theory and "natural selection." To Gause (*The Struggle for Existence*, Baltimore, 1934, or No. 277 in Hermann's *Actualités Sci. et Indust.* series, Paris, 1935) belongs the distinction of clearly formulating the corollary for population biology that, where two or more species have precisely the same ecological requirements, they cannot exist together in the same habitat. This consequence of the theory of natural selection is frequently referred to as "Gause's Theorem." A good deal of misunderstanding has arisen in this connection but, providing one inserts in the definition after "requirements" the words "in short supply"—which Gause evidently implied—most of the objections to the theory of "competition" disappear. While it is true that frequently in nature one finds several species consuming the same food living together and apparently not affecting the numbers of the rival populations, competition can never be assumed simply on account of spatial proximity; the size of one or both populations can be limited by other factors than food supply; the food may be exploited in different ways or at different times by the different

species, any of which may have alternative sources of food. The idea of "Gause's Theorem" has been behind much recent work, including that of Lack (*J. Anim. Ecol.*, 1946, **15**, 123) in relation to birds of prey, where little overlap of feeding was found, and Elton (*J. Anim. Ecol.*, 1946, **15**, 54). The latter paper, the findings of which have been contested by Williams (*J. Anim. Ecol.*, 1947, **16**, 11), is an analysis of data from a wide range of plant and animal communities, showing that a disproportionately low number of species of a common genus occur together in the same habitat, the inference being that their ecological requirements will be more similar than those of species of different genera and so they are unable to exist together under conditions where competition is likely.

No recent work has been done on experimental predator-prey systems, and Gause's experiments (*loc. cit.*) with two species of mites, protozoa and yeasts, and with two species of protozoa, remain to provide rather striking confirmation of the conclusions of the mathematical workers concerning oscillations in numbers resulting from such systems. These were' first predicted by Volterra and Lotka, and became widely known to biologists after Nicholson's (*J. Anim. Ecol.*, 1933, **2**, 132) exposition of the biological consequences, and his theory of "balance," referred to below. Also rather before this time W. R. Thompson (papers in *Bull. Ent. Res.*, 1929, 1930, *Parasitology*, 1927, 1928, 1929) worked out Parasite-Host relationships specifically from the point of view of the economic entomologist. In general there is a tendency for oscillations to occur in experimental populations and for the intensity of "damping factors" to determine whether the oscillations are maintained or whether one or other species is wiped out. Provision of adequate "cover" for the prey, a suitable relationship between the lengths of the life-cycles of the two species, the influence of periodical immigrations or emigrations, are all factors tending to perpetuate the oscillations, while lack of such factors leads to the extinction of one or both species—so called "relaxation oscillations."

When extended to field conditions the same types of modifying factors are present with increasing influence, and the patchiness of distribution of suitable habitats for both species permit local extinctions of one or both species to fit into a general pattern over a wide area of the survival of the species. The catholic habits of many predators frequently cause them to vary the amount of attention which they give to a particular food source in accordance with its abundance, thus providing an important "damping" effect. It is generally felt, therefore, that a very wide gap must

be bridged between the simple conditions of the theoretical treatments and conditions in nature, where a multiplicity of factors of ever-changing intensity will have results which can only be predicted by rather elaborate methods ; a step in this direction, using methods of partial regression, has been taken by G. A. Riley (*J. Mar. Res.*, 1941, **4**, 162, and 1946, **6**, 104, 114) in an analysis of Zooplankton Populations in relation to food supply and many other factors.

The theoretical discussion of simple population systems can be expanded to cover all the factors controlling the size of a population, whether of the nature of interspecific competition (for mutually helpful " coactions " see W. C. Allee, *Animal Aggregations*, Chicago, 1931 ; G. E. Hutchinson, *Ecology*, 1947, **28**, 319), predator prey-relationships or inanimate environmental influences, and in this field lie some of the most influential ideas of present-day population ecology. Nicholson (*J. Anim. Ecol.*, 1933, **2**, 132) and Nicholson and Bailey (*Proc. Zool. Soc. Lond.*, 1935, p. 551) were responsible for a general theory of population balance and, perhaps, the most important idea stressed (although not originated) by them was that of the distinction between density-dependent and density-independent factors. The size of a population can only ultimately be limited by factors whose effect on mortality is related to the density of the population. Factors whose intensity is independent of population size, although they can, and do, of course, influence it, cannot result in the maintenance of a population level between fixed limits. Thus, while catastrophic climatic effects, for instance, may wipe out a population or allow it to expand, the ultimate size of the population will be governed by factors which are more important in a large population, such as disease, increase in predators and parasites or, ultimately, the amount of food available per head. Such factors, owing to their lesser intensity in sparse populations, will tend to keep the population within certain limits of size, which are regarded as including a very approximate " balance " level. This concept, although greatly criticised by Bodenheimer (*loc. cit.*), E. J. Clark, 1947, and Uvarov (*Trans. Ent. Soc.*, 1931, **79**, 1), is now rather generally accepted in principle—although still ignored by many economic entomologists. H. S. Smith, in a particularly clear exposition of its implications for economic work (*J. Econ. Ent.*, 1935, **28** (6), 873) and in his successful applications of biological control methods, has done much to further the doctrine in America. A. D. Voûte (*Arch. Néerland. Zool.*, 1946, **7**, 435) has considered this problem in the light of outbreaks of insect pests especially ; his rather unusual nomenclature is clearly explained by Varley (*J. Anim. Ecol.*, 1948, **17**, 82) and the same thesis is argued by

Crombie (*J. Anim. Ecol.*, 1947, **16**, 44). Two papers by J. Davidson and H. G. Andrewartha (*J. Anim. Ecol.*, 1948, pp. 193 and 200) describe circumstances in which the authors claim that climate is acting with a peculiar system of patchy distribution of refuges as a density dependent factor ; this shows that " density dependent " factors may not necessarily be synonymous with " biotic " factors, thus underlining H. S. Smith's (*loc. cit.*) insistence on the desirability of the former term.

Much recent practical evidence on questions of interspecific relationships has been produced. Firstly, there are the laboratory studies of Park and Crombie, already mentioned, which in general confirm the teaching of " Gause's Theorem " in showing that two species under simple conditions cannot live together when competition for a single food supply is assured. Crombie showed that the results of such competition are frequently predictable under fixed environmental conditions and can easily be reversed by making alterations in the conditions, such as the provision of glass tube refuges for the larvæ in an *Oryzæphilus-Tribolium* beetle system where the larvæ of the former are preyed upon by the latter. Park showed that the results of competition between two *Tribolium* species can be almost completely determined by whether or not the protozoon parasite *Adelina* is present in the cultures, and his studies are outstanding for the large number of replications and the long time period (four years) of the experiments which permitted the expression of his results in the form of statistical probabilities of the outcome of any particular competition set-up. As a result of this study a number of interesting complications have been demonstrated showing that such systems can be very far from simple.

An outstanding field study of the factors controlling population size has recently been made by Varley (*J. Anim. Ecol.*, 1947, **16**, 139) on the Knapweed gall-fly. This is the first complete study of this nature involving the determination and measurement of the causes of mortality in a field population and providing results of the greatest interest and significance. The study covers the gall-fly itself, its density, fecundity and mortality, the effects of climate, parasites and predators and their relative importance. Three density-dependent factors are demonstrated : larval mortality and the fecundity of two Chalcid parasites which was shown to depend on the gall-fly population. To the non-specific mortality, feeding by field-mice made an important contribution. On the basis of the known mortality factors and estimates (after Nicholson) of the " area of discovery " of the parasites, the steady densities of host and parasite were estimated and shown to conform in a

most remarkable way with the conditions found in nature. Thus for the first time an approximate quantitative picture of a population balance has been obtained; the implications of the technique for biological control studies are evident.

From factors determining the size of populations at least two types of study can be traced; those concerning the evolutionary and general biological significance of intraspecific population size and those concerning the existence and causes of fluctuations in numbers. The first link-up with modern genetical problems, the detection and significance of the "Sewall Wright" effect, the significance of population limiting mechanisms and similar studies, cannot be treated here. The most spectacular and perhaps the most difficult field of population investigation so far, however, is that which is concerned with fluctuations in numbers. The best-known examples are to be found among the mammals, and this field has been recently summarised by Elton (*Voles, Mice and Lemmings*, Oxford, 1942), who was the original worker on these problems. The economic implications of these effects, which occur on a vast scale, concern us both from the point of view of pest species and also that of fur-bearing forms such as the arctic fox and the snowshoe rabbit. Elton's book and a series of articles in the *Journal of Animal Ecology* (*e.g.* 1948, **17**, 30) by Elton, Chitty and Chitty cover this field of enquiry. Some ideas on the causes of fluctuations are presented by these authors and by J. R. Dymond (*Trans. Roy. Soc. Canad.*, 1947, **41**, 1), but the dominant note to-day is one of caution following the demonstration by statistical workers that coincidence between two statistics showing cyclical fluctuations cannot usually be established by methods of correlation calculus. A rather different phenomenon of fluctuations, due mainly to migration, is reported by F. Braestrup (*Medd. om Grønland*, 1941, **131**, 1) based on his study of Greenland arctic foxes.

A number of continental workers have described fluctuations in the numbers of insects, the paper of Schwertfeger (*Z. Angew. Ent.*, 1941, **28**, 254) being one of the most important, especially as a source of literature and of statistics. His main theoretical conclusions confirm those of Nicholson and his followers, but he points out that a sufficiently disturbed population system may revert to a new balanced condition unrelated to the earlier one.

Studies of Communities and Ecosystems as a Whole.—As distinct from an analysis of the factors controlling the component populations of a community and their mutual interactions, a considerable part of synecological work is devoted to the study

of such communities as a whole and comparisons between different communities; such studies are of three main types:

(*a*) Studies based on the methods of the plant sociologists, which attempt an analysis in terms of the abundance, dominance and ecological specialisation of the component species.

(*b*) Studies of the qualitative compositions of communities in terms of the available niches and the "food chains" involved.

(*c*) Studies which extend the qualitative approach to a more quantitative treatment by the introduction of biomass and energy relationships.

The "sociological" type of method, although well known to botanists, seems to have had little appeal to zoologists in this country; on the Continent, and especially in Scandinavia, it has become quite popular, however. By an analysis of the species composition of a number of samples taken over an apparently uniform area and by comparisons with other areas, figures of estimated abundance, percentage dominance, and frequency of occurrence (*i.e.* the proportion of samples in which an animal is found) can be obtained; thus an idea is formed of the relative tolerance of the different animals, of which animals tend to be associated together and also of the ecological stringency of the different types of habitat. Such a study was made by Krogerus (*Acta Zool. Fenn.*, 1932, **12**,) on the sand-dune fauna of the Finnish coast and this remains the most complete of its kind. Using rather more refined techniques adapted from botanical practice, I. Agrell made a more restricted study of Lapland Collembola (*Opuscula Entomologica*, Lund, Suppl. 3, 1941) and produced a convincing description of the fauna and its living conditions. However, despite the claims that such methods are "objective" no one has yet combined the courage and the energy needed to use truly random sampling methods, the selection of sampling points being made on the basis of macroscopic vegetation and similar considerations, so that one of the essential conditions of the method is usually ignored. Some outspoken criticism is presented by Tuxen (*Zoology of Iceland*, 1944, **1** (2), 169). The methods used by Agrell developed from Kulczynski (*Bull. Int. Acad. Cracovie*, Suppl. 2, 1927, 57) have, however, undoubted applications in ecology, and Gisin's use of the techniques (*Arch. Sci. Phys. Nat.*, 1947, **29**, 42) would appear to be more justifiable.

The qualitative study of communities, which in the botanical field lead to the concepts of succession and climax, among animal

ecologists resulted in the concepts of the "food chain" and the "niche" (Elton, *Animal Ecology*, 1926) and the "trophic level" of Lindemann (*Ecology*, 1942, **23**, 399) and other American authors. These remain the dominating theoretical concepts in their respective fields and, although much attention is now devoted to more quantitative studies, the great complexity and challenge for work in the qualitative field have recently been stressed by many workers. A. S. Watt (*J. Ecol.*, 1947, **35**, 1) has shown that apparently stable plant communities frequently show phenomena of active cyclical replacement between the constituent species—with important implications for the animal ecologist—while Elton (*J. Ecol.*, 1949, in the press) has stressed the complexity and variety of habitat "interspersion" in animals and the need for more adequate techniques for describing different types of dispersion of habitats both in space and in time.

The use of the total mass of organisms instead of numbers of individuals, when comparing species which make up a community, has long been advocated and is obviously desirable. The method has, however, until recently, been almost entirely confined to limnological and marine fisheries work. G. J. Petersen (*Rep. Dan. Biol. Sta. to Board of Agric.*, 1918-24, **25–30**) first used biomass figures in his study of the quantities of living organisms belonging to the plant, herbivore and successive carnivore "levels" in the marine communities on which local Danish fisheries are based; he showed how great is the "inefficiency" in the conversion of biomass at each successive level of the food chain. In America this line of thought influenced limnologists through Birge and Juday (*e.g.*, *Trans. Wisc. Acad. Sci. Art Let.*, 1929–31, **24–27**), Lindemann (*Ecology*, 1942, **23**, 399), G. L. Clarke and W. E. Ricker (Symposium in *Ecol. Monogr.*, 1946, **16**). In Denmark its most important manifestations have been in the works of soil biologists C. H. Bornebusch, ("The Fauna of Forest Soil," *Forstl. Forsøksv. Dann.*, 1930, **11**, 1) and C. Overgaard (Copenhagen, 1949, in the press) and in a study of shore forms by H. M. Thamdrup (*Medd. Komm. Dansk Fisk. og Havunderseg. Fisk.*, 1935, **10**, (2)). In Germany the same tradition has influenced Thienemann (*e.g. Arch. Hydrobiol.*, 1931, **22**, 616 and 1947, **41**, 626) and many other limnological and fishery workers, including Elster (*Z. Fisch. u. deren Hilfswiss.*, 1944, **42** (2/3)) and K. Kalle (*Deutsche Hydrogr. Z.*, 1948, **1**, 1–17) and also lead to at least one soil community work (A. T. Ulrich, *Mitt. Forstwirts. u. Forstwiss.*, 1933, **4** (2), 283). In England it appears to have borne few fruits as yet, although the present author has contributed a short theoretical discussion (*J.*

Anim. Ecol., 1948, **17**, 75). In Russia the attention of limnologists has been directed to this type of method and valuable papers have been contributed by V. S. Ivlev (*Uspekhi Sovremennoi Biologii*, 1945, **19**, 98) and E. V. Borutzky (*Proc. Kossino. Limnol. Sta.*, 1939, **22**, 156). As a result of this and similar work it has been confirmed that normal abundance figures can be extremely misleading and that biomass is a very much more reliable index of ecological significance of a species. At the same time it has emerged that a more useful index still is the integrated metabolic activity of a species and this for two main reasons: firstly, biomass is, at best, only a crude measure of protoplasmic weight, because animals contain varying amounts of inorganic substances, water, silica, calcium carbonate etc., and, secondly, because the rates at which food is consumed and also the length of a generation vary very greatly in different species. Thus, according to Kalle (*loc. cit.*), ten times as great a mass of living organisms is needed under marine arctic conditions to bring about the same quantity of chemical change as in tropical conditions. Furthermore, when comparing two communities containing organisms with different life histories, the more rapid circulation of minerals in one is by no means an indication of greater activity. For these reasons a number of workers have been forced to adopt an "energetics" approach to problems of "productivity" and "yield" of habitats. This was first done, perhaps, by Bornebusch (*loc. cit.*) when he measured respiration rates of soil animals, in order to assess their relative importance in forest soils, and found that the smaller Arthropods, although of much lower total biomass than the earthworms, were more important as agents of chemical change. The same concept is explicitly summarised in Kalle's definition of production which is: "that part . . . of the sun's energy which is dissipated as heat or energy of movement through the process of reduction of carbon dioxide to organic substances." This concept of prime importance for quantitative community studies has already borne fruit in a most useful quantitative evaluation of the rôle of the free-living Nematoda of the soil (Overgaard, *loc. cit.*) and is likely to provide a key not only to theoretical problems of production and the relative importance of different species in natural systems, but also to the practical comparison and evaluation of different exploitation techniques—a matter not without significance for man, a species which, having overcome most of the density-dependent population checks, is now faced with the ultimate check—limited food resources.

In a short review of a widely ramifying subject much valuable

work has inevitably been omitted, but it should at least be evident that population ecology is a vigorous and growing science, in which quantitative techniques, revealing the amazing complexity of this field of study, offer new methods for gaining an understanding of our environment which will ultimately lead to its more profitable exploitation. All the experience of ecology points to simple "single stand" systems as being the least efficient and the most liable to catastrophic extinction by disease or predation; "practical" and æsthetic considerations therefore unite in demanding a reversal of current trends toward the extinction of all organisms which do not at first sight appear to be of economic utility.

PHYSICAL ANTHROPOLOGY.

By N. A. Barnicot, B.Sc., University College, London.

The two previous articles dealt with the comparatively new serological methods and genetical concepts which are being used in unravelling the more recent phases of human prehistory. For the elucidation of more remote stages one must turn to fossil evidence and the methods of comparative anatomy, although, even in this field, the application of genetical principles can still, as we shall see, prove fruitful. The last ten years have been so rich in discoveries and publications in human and primate palæontology that this short review must be far from comprehensive.

The discovery of new specimens of *Pithecanthropus* in Java in the years just before the war, and the publication of Weidenreich's monumental study on the skull of *Sinanthropus* (*Palæo. Sinica.*, N.S.D, No. 10, 1943), in which he not only describes the rich material from Peking, but discusses its significance for human evolution in general, have greatly extended our knowledge of early Pleistocene man. Le Gros Clark (*Biol. Rev.*, **15**, 202, 1940), in a judicious review of this and other evidence, draws attention to the fact that the lower limbs of these early hominids were substantially modern in form, although the skulls show many primitive features, such as the small brain size, retreating forehead, massive supraorbital ridges, protrusive jaws and large teeth. He infers that the divergence of man from the evolutionary line leading to the great apes must have occurred at a considerably earlier period. In the commemorative volume to Dr. Robert Broom (*Publ. of the Roy. Soc. S. Africa*, Cape Town, 1948) he turns again to the theme of the very different rate of evolution displayed by different regions of the body, which is evident from many palæontological studies. It is

possible that some workers have paid too little attention to this asynchronism and have relied too much on a belief in a " harmonious " change affecting all regions equally when reconstructing and interpreting fragmentary remains. It is also worth noting the range of variation which more abundant fossil material often discloses; the five more or less complete brain cases described by Weidenreich (*loc. cit.*) range in capacity from 850 to 1300 c.c. Some of this variation may reasonably be attributed to sexual differences, but, even so, it is clear how much caution must be observed in drawing far-reaching conclusions from single specimens.

Some workers consider that the stage represented by *Pithecanthropus* gave rise to Neanderthal man, but it has long been disputed whether *Homo sapiens* could have evolved from the latter. Weidenreich (*Am. J. Phys. Anthr.*, N.S. **3**, 21, 1945), commenting on Mahony and Wunderly's description of the anatomy and geology of the Keilor skull which was excavated near Melbourne, Australia, in 1940 (*Mem. Nat. Mus. Melbourne*, No. 13, 1943) states that it is closely similar to the Wadjak skulls discovered by Dubois in Southern Java, which are tentatively dated as last glacial or post-glacial. He considers that Wadjak man, which is morphologically a primitive *sapiens* form, evolved from *Pithecanthropus* with Solo man as an intermediate, so that the Keilor skull provides a transition to the Australian aboriginal. He doubts whether the Keilor skull is indigenous to the deposits of supposed last interglacial age in which it was found, and suggests that it is more recent: Zeuner (Nature, **153**, 622, 1944) adopts a cautious attitude to the dating of this skull, which, if its discoverers are correct, would be a *Homo sapiens* contemporary with the European Neanderthals. Whether or no this direct evolutionary sequence from *Pithecanthropus* to *Homo sapiens* in Indonesia proves to be well-founded, it is less easy to fit the European data into such a simple scheme. The remains from Piltdown (*Eoanthropus*) and from Swanscombe provide cogent evidence for the existence of a type of man with *sapiens* features prior to the appearance of the Neanderthals. A brief preliminary report by Mlle. Henri-Martin (*C. r. Acad. Sci.*, **225**, 766, 1947) of the fragmentary remains of two skulls recovered from the Fontéchevade cave, Charente, suggests that this evidence may soon be strengthened. The bones were found below a stalagmite floor, which underlay a Mousterian level, so that they can scarcely have been intrusive and must considerably antedate the Neanderthal period. They were associated with bones of a warm temperate fauna and crude implements of Tayacian type. According to the report, the skulls show no evidence of a heavy supraorbital torus

and may prove to approach *Homo sapiens* in type.* The Galley Hill skeleton, which again is substantially modern in form is also relevant to this problem. Most workers, however, have been disinclined to accept it as belonging to the Middle Pleistocene deposits from which it was excavated. Oakley (*Q.J. Brit. Ass.*, **4**, No. 16, 1948), in describing the fluorine-analysis method for the relative dating of fossil bones, suggests that it might help to settle this question. The method depends on the fact that fluorine gradually accumulates in bone exposed to fluorine-containing ground-waters, as a result of ionic exchange with the bone minerals. On average the fluorine content of bone may rise from 0·058 per cent. to 0·360 per cent. during the course of the Pleistocene (½–1 million years), but an absolute chronology cannot be achieved by this method, because the rate of fluorine accumulation depends on the local fluorine content of the soil. A comparison can, however, be made between bones from the same site and horizon which are presumed to be contemporaneous, and if the fluorine content is found to be significantly different this would afford evidence that they are not in fact the same age.

The recent Javanese and Chinese material has extended our knowledge of a form which, although primitive, was still definitely man. Hitherto there has been a serious gap in our knowledge of the still earlier, Tertiary phases of man's evolution, and indeed of the early evolution of the whole group of Anthropoidea in which he is included along with the monkeys and apes. Some of the spectacular finds in the Transvaal of ape-like species, which incorporate many distinctively human features, have been the subject of a monograph by Broom, their discoverer, in collaboration with Schepers ("The South African Fossil Ape-Men; The Australopithecinæ," *Publ. of the Transvaal Mus.*, No. 2, 1946), and Le Gros Clark has already discussed the wider implications of this material in this journal (SCIENCE PROGRESS, **35**, 377, 1947) and has presented his own observations on their anatomy (*J. Anat.*, **81**, 300, 1947). Since

* More recently still Vallois (*C. r. Acad. Sci.*, **228**, 598, 1949) has given a short description and figure of the more complete specimen. By comparing the frontal curvature and position of the frontal sinus with that in Neanderthal, Cromagnon and Piltdown man, he concludes that the Fontéchevade form probably had a vertical forehead without heavy supraorbital ridges, and this is supported by the frontal fragment of the other specimen. According to Vallois the vault height is low and close to the estimates for the Piltdown and Swanscombe skulls. He regards the Fontéchevade man as a representative in the last Interglacial of a separate line of human evolution of which Swanscombe and Piltdown man are geologically earlier examples (*C. r. Acad. Sci.*, **228**, 949, 1949).

that time more material has come to light, but still awaits full description. Broom and Robinson (*Nature*, **160**, 430, 1947) reported the discovery of an almost perfect adult female skull of *Pleisianthropus* and further remains of the post-cranial skeleton, including a fragmentary scapula, which they regard as somewhat divergent from either man or anthropoid ape. They also present a drawing of a complete pelvic bone, which in the low, broad ilium is remarkably human and very different from that of any known ape. The ischial tuberosity, they say, is not typically human, but this specimen clearly affords very substantial support for the previous evidence of the limb bones that these Australopithecinæ were upright-walking animals. A critical note is provided, however, by Straus (*Am. J. Phys. Anthrop.*, N.S., **6**, 285, 1948) in his paper on the humerus of *Paranthropus*; on the basis of comparative measurements of a caste of the fragment of this bone and those of living anthropoids and catarrhine monkeys, he is unable to find sufficient evidence to support Le Gros Clark's view (*loc. cit.*) that the upper limb of the Australopithecinæ was probably slender and unspecialised for arboreal swinging. Broom and Robinson (*Nature*, **160**, 153, 1947) also figure an adult mandible of *Pleisianthropus*, which lacks only the condyle. They draw attention to certain hominid features, such as the relatively small canine tooth, which is worn to the level of the adjacent teeth and cannot have been a large ape-like fang, to the absence of a simian shelf on the posterior aspect of the symphysis, and to the flat, backward slope of the anterior face of the symphysis, which contrasts with the rounded contour in living apes. They suggest that the animal from which this jaw came may have had a brain volume of 600–700 c.c., which would be large in relation to the probable body-size of this species. In a further note (*Nature*, **161**, 438, 1948) they report a new brain case from Sterkfontein, which they believe permits a more accurate estimate of cranial capacity. After restoration the capacity is 530 c.c., which is considerably larger than that of the type specimen, which is 450 c.c. If they are correct in their belief that the range for the male sex extended to 750 c.c. this would almost overlap with the lowest value reported for *Pithecanthropus*. Nevertheless, it seems that the small brain size of the Australopithecinæ is a relatively primitive feature as compared with the structure of the teeth and limbs, and it is interesting, as pointed out already, to find a similar situation in early Pleistocene hominids. The remains of caves containing bone breccia are very numerous in the Transvaal, and Broom, in reporting yet another find at a different site in the Sterkfontein region (*Nature*, **163**, 57, 1949)

writes that there are about 100 of them within an area of 10 square miles; this new specimen consists of a lower jaw and some teeth, which he assigns to a new species *Paranthropus crassidens.* The variety and dispersal of the Australopithecinæ is still further extended by Dart and his co-workers' discovery of an occiput in limestone dumps at Makapansgat, some 200 miles to the north-east. Dart assigns this specimen to a new species of the genus *Australopithecus*, the name which he gave to his initial find at Taungs in 1924; he calls it *A. prometheus*, because he believes that traces of carbon in the breccia show that this creature could make fire. The occiput resembles that of *Paranthropus robustus*, the species which Broom found at Kromdraai. The surface for the attachment of the neck muscles is more horizontal than it is in apes, and the inion is situated relatively low down. The deposits contain the same baboon species as occurs at Sterkfontein, and they are regarded as Lower Pleistocene in age. Dart again finds evidence that the baboon skulls and other animal bones at the site have been fractured in a way that suggests to him that *Australopithecus* could use weapons. Continued work at the same site has now yielded an adolescent lower jaw, which Dart describes (*Am. J. Phys. Anthrop.* N.S., **6**, 391, 1949) and refers to the same species as the skull fragment. This massive mandible has two completely erupted permanent molars, but the premolars are not fully erupted, a sequence which is characteristic of apes rather than man, but on one side a milk molar is retained and shows great wear. The degree of wear of the milk molars in other Australopithecines has been pointed out by other workers also as an indication of a prolonged adolescence in these creatures and hence an approach to the human condition. The symphysis shows no simian shelf or genial pit. The cheek teeth are somewhat larger than those of *Paranthropus*, and there are also sufficient differences in the detail of the crown pattern to lead Dart to regard it as belonging to a different genus. While many authorities regard the Australopithecinæ as structurally close to the human ancestral form, their remains come from Pleistocene or at earliest Pliocene deposits, and they must therefore be regarded as survivors of the actual ancestral forms which presumably lived at a still earlier period of the Tertiary. Apes from the Upper Miocene (Dryopithecinæ) have been found in Europe and in Northern India, and the teeth of the latter sometimes approach the human pattern. In recent years Africa has provided new and more abundant evidence of the complex evolutionary radiation of anthropoid ape forms which was apparently occurring during the Lower Miocene. The work of Leakey and his collaborators in the Kavirondo Gulf region

of Lake Victoria yielded teeth of Lower Miocene anthropoids as early as 1931. On the basis of this material Hopwood founded several genera, including *Proconsul*, which he regarded as an ancestral chimpanzee. Later expeditions resulted in more finds at the rich site on Rusinga Island, and in 1943 Leakey (*Nature*, **152**, 319, 1943) reported a substantially complete mandible of *Proconsul*. He stressed certain hominid features, such as the absence of the simian shelf and genial pit, the convergence of the rows of cheek teeth and the form of the condyle ; the large canine tooth, the form of the first premolar and the crown pattern of the third molars, however, betrayed anthropoid affinities. The British-Kenya Miocene Expedition to Rusinga in 1947 collected some 50 specimens, most of them jaw fragments, but including a few limb bones. McInnes has described and figured some of this material (*J.E. Africa Nat. Hist. Soc.*, **17**, 1943), and Le Gros Clark (*Nature*, **161**, 667, 1948 and *Q.J. Brit. Ass.*, **5**, No. 20, 340, 1949) has given a preliminary interpretation of the results. The majority of the specimens belong to a species closely allied to, but rather larger than, the original *Proconsul*, and there are jaw fragments of a related form which may have been the size of a gorilla. The slender limb bones, says Clark, indicate a cursorial form more like the cercopithecoid monkeys, and lacking the specialisations for arboreal locomotion (brachiation) which characterise living apes. This is important, because it indicates that the anthropoid ape stock cannot be excluded from the human evolutionary line on this account. McInnes stresses what he considers to be human features in the specimens of foot bones (talus and calcaneum). Apart from *Proconsul*, there are jaws of a form whose teeth resemble those of *Sivapithecus* from the Lower Pliocene of India, and of *Limnopithecus*, which appears to be close to the Gibbon and to *Propliopithecus*, a putative Gibbon ancestor from the Egyptian Oligocene. The latest and most remarkable of the Kenya discoveries is an almost intact skull of *Proconsul* (*Nature*, **162**, 688, 1948). This skull shows that there were no heavy supraorbital ridges such as occur in living apes and indeed in early Pleistocene man. The arch of the jaws is narrow in front, and the nasal aperture is also narrow, in a manner reminiscent of some monkeys. It may well be that such features as the supraorbital ridges and the absence of a simian shelf were common to many early anthropoids, that the human line never possessed the latter and, in some cases at least, acquired the former independently of the later anthropoids. These important developments in Kenya will doubtless throw much light on the affinities of both the anthropoid apes and the Old World

monkeys and will provide a broader foundation for our understanding of human evolution.

Apart from these recent discoveries, which have made Africa the focal point of human and primate palæontology, this continent has yielded few remains of Pleistocene man that are not uncertain in their affinities or their geological dating. It is, therefore, interesting to read Vallois' short account of the human maxilla and mandible fragments excavated at Rabat, French Morocco, in 1933 (*C. r. Acad. Sci.*, **221**, 669, 1945). He considers that the bones represent a form of man resembling, but more primitive than, the Neanderthals. The massiveness of the lower jaw puts it in the *Sinanthropus* range, and the very large molars and premolars present primitive features which are found in *Sinanthropus*, but not in the Neanderthals. The age of the find is still uncertain, but Neuville and Ruhlman attribute it to the last Interpluvial, which would make it contemporary with some of the earliest Neanderthals.

In 1934–36 Kohl-Larsen found fragments of a braincase at the edge of Lake Eyasi, Tanganyika Territory. Weinert restored and described this skull, which appears to be much more primitive than *Homo sapiens*, and he named it *Africanthropus*. Leakey and Reeve (*J.E. Africa Nat. Hist. Soc.*, 1946) have now re-examined the site and agree with the discoverers that, in spite of the primitive form of the skull, it belongs to Upper Pleistocene times. It was associated with implements of the African Levalloisian type; in Europe the form of man found with the corresponding industry is invariably Neanderthal.

The taxonomy of fossil man has been the subject of some discussion in recent years. As Zuckerman pointed out (*Nature*, **145**, 510, 1940), there seems to be an almost unavoidable tendency for discoverers to over-emphasise the distinctiveness of their particular find, thus leading to a superfluity of genera. He, therefore, welcomed Koenigswald and Weidenreich's recognition that *Pithecanthropus* and *Sinanthropus* are sufficiently similar to be regarded as one genus or even a single species. Le Gros Clark (*Nature*, **145**, 70, 1940) considered that *P. erectus* and *P. pekinensis* would be suitable names. The opposite tendency is exemplified by Gates (*Am. J. Phys. Anthrop.*, N.S., **2**, 279, 1944), who states that if *Sinanthropus* or *Pithecanthropus* were any other mammals than man they would certainly be regarded as separate species, and, in his view, living man should be classified as five distinct species. Osman Hill (*Nature*, **145**, 260, 1940) takes a similar view, and indeed would even restrict the dignity of the name "sapiens" to the "white" race. The theoretical basis of taxonomy has been expounded by

Simpson (*Bull. Am. Mus. Nat. Hist.*, 85, 1945) in an admirably concise and authoritative work. In his new classification of Mammals, Simpson has established a super-family, Hominoidea, which is composed as follows:

Super-family Hominoidea:

1. Family—Parapithecidæ.
2. „ —Pongidæ.
   Sub-family (*a*) Hylobatiæ.
   (*b*) Dryopithecinæ.
   (*c*) Ponginæ.
   (*d*) Australopithecinæ.
3. „ —Hominidæ.

In the Hominidæ he recognises only two genera other than *Homo*, namely *Pithecanthropus* and *Eoanthropus* (Piltdown); Heidelberg, Neanderthal, Rhodesian man and other prehistoric forms he includes in the genus *Homo*. He recognises that the complex radiation of the Pongidæ which is now being revealed will probably necessitate a revised classification as more fossil finds establish their phylogeny in increasing detail. Simpson's discussion of the relationship between classification and phylogeny is particularly subtle and valuable, and in his appreciation of genetic criteria his presentation accords substantially with that of Dobzhansky, who has contributed a valuable paper on human taxonomy from a genetical standpoint (*Am. J. Phys. Anthrop.*, N.S., **2**, 251, 1944). Dobzhansky takes the view that species are essentially groups in which reproductive isolation has proceeded to a point where exchange of genes with other populations no longer occurs to a significant extent, whereas in the case of sub-species or races gene exchange is still possible. An acceptance of this viewpoint eliminates Osman Hill's and Gates' contention that any living population qualifies as a separate species, since clearly intergradations exist and reproductive isolation has not been achieved. This dynamic view of species must, however, be judiciously applied in cases where the possibility of gene exchange cannot be assessed, because the groups in question inhabit different territory (allopatric species). In such cases one must rely on a comparison with the degree of differentiation which is manifest in allied true species. Dobzhansky points out that man was evidently differentiated into local variants at all known stages of his evolutionary history; in many instances it is impossible to decide whether these variants were reproductively isolated, but in the case of the Mount Carmel (Skhūl) skeletons, where the bones display a mosaic of *sapiens* and Neanderthal features, the only likely explanation, consistent with genetical knowledge, is hybridisation. It is not

unlikely, therefore, that other morphologically distinct types of early man were capable of interbreeding, and the pattern of human evolution may, perhaps, be better represented by a network of interconnected lines than by a series of divergent or parallel branches. In this way, such forms as Neanderthal man may have contributed genes to later types, and the search for a unique, consecutive evolutionary line leading to *Homo sapiens* may be misdirected. This view may also be contrasted with Weidenreich's, already alluded to, which conceives a parallel evolution of several early Pleistocene forms to the *sapiens* level in various parts of the world. If this is the case, the end-products have evidently not achieved specific distinction in Dobzhansky's sense. Weidenreich's view also involves a belief in an "orthogenetic trend" in human evolution, the cardinal feature of which was an increase in brain size, which carried in its train various changes in the rest of the skull. Weidenreich was struck by the occurrence of certain features both in *Pithecanthropus pekinensis* and in the modern Mongoloids, but Dobzhansky considers that this does not necessarily imply an isolated local evolution, but might be due to the retention of these features, due to natural selection, in a stock which had in fact hybridised extensively during the course of its evolutionary history.

NOTES

Report of the Water Pollution Research Board for 1947 (E. L. S.)

The work during 1947 of the Water Pollution Research Laboratory is described in *Water Pollution Research 1947** published recently. The Report of the Board deals briefly with the results obtained in some of the investigations, and the Report of the Director of Research gives a more complete survey of the work on the treatment of water, sewage and industrial effluents, and on the effects of pollution of natural waters. An appendix gives references to papers published during the year in scientific journals describing some of the investigations in detail.

The effect of chlorinated water on corrosion of domestic fittings such as brass ball valves was studied as a result of enquiries received towards the end of the war from many water undertakings. It was found that chlorine, in the concentrations used in waterworks practice, had no decisive effect on fittings made of cast brass, but did increase corrosion in valves made of hot pressed brass.

The report gives a summary of further results obtained in large-scale experiments in the treatment of sewage by alternating double filtration and by filtration with recirculation of effluent.

When sewage has been treated by sedimentation and a biological process, a second period of sedimentation generally removes enough of the sludge formed during biological treatment to render the final effluent suitable for discharge into a river. At some sewage-disposal works, however, the effluent has to be discharged into a stream affording very little dilution and a more complete removal of humus is desirable. Experiments have begun on the filtration of settled effluent through sand and anthracite in rapid filters, to compare the efficiency and cost of various methods of removing humus and to ascertain the effect of the removal on other properties of the effluent.

Experiments carried out on the control of flies in percolating filters treating sewage have shown that the insecticides DDT and Gammexane are effective, but must be used in carefully measured

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amounts to avoid discharging effluents toxic to fish and other water life.

Much of the work of the Laboratory during the year has been concerned with the treatment of industrial waste waters. These include waste waters from the processing of coffee and sisal, manufacture of cyder, slaughter houses, manufacture of dyestuffs, electroplating, and the dyeing and finishing of textiles.

Miscellanea

The following were elected to be Fellows of the Royal Society at the meeting held on March 17: Prof. J. F. Allen, professor of natural philosophy, University of St. Andrews; R. W. Bailey, head of the Mechanical and Metallurgical Research Department at Metropolitan-Vickers, Manchester; Dr. F. C. Bawden, head of the Plant Pathology Department at Rothamsted Experimental Station, Harpenden; Prof. F. W. Rogers Brambell, Lloyd Roberts professor of zoology, University College of North Wales, Bangor; Prof. K. E. Bullen, professor of applied mathematics, University of Sydney; Dr. E. B. Chain, university demonstrator in chemical pathology, University of Oxford; Dr. U. R. Evans, reader in metallic corrosion, University of Cambridge; Prof. E. D. Hughes, professor of chemistry, University College, London; Prof. W. Q. Kennedy, professor of geology, University of Leeds; Prof. W. B. R. King, Woodwardian professor of geology, University of Cambridge; Sir Ben Lockspeiser, chief scientist, Ministry of Supply; Dr. J. M. McNeill, naval architect, John Brown & Co., Ltd., Clydebank; Dr. H. R. Marston, chief of the Division of Biochemistry and General Nutrition, Commonwealth Council for Scientific and Industrial Research (University of Adelaide); Prof. K. Mather, professor of genetics, University of Birmingham; Prof. P. B. Medawar, Mason professor of zoology and comparative anatomy, University of Birmingham; Dr. W. T. J. Morgan, research worker, Lister Institute of Preventive Medicine and reader in biochemistry in the University of London; N. W. Pirie, head of the Biochemical Department, Rothamsted Experimental Station, Harpenden; Prof. C. F. Powell, Melville Wills professor of physics, University of Bristol; Dr. D. A. Scott, research member, Connaught Laboratories, University of Toronto; Prof. Wilson Smith, professor of bacteriology, University College Hospital Medical School, London; Dr. G. B. B. M. Sutherland, reader in spectroscopy, Department of Colloid Science, University of Cambridge; Prof. O. G. Sutton, professor of mathematics and physics, Military College of Science, Shrivenham; Prof. Meirion Thomas, professor of botany at King's College, Newcastle-

upon-Tyne; Prof. J. M. Whittaker, professor of mathematics, University of Liverpool; Prof. F. G. Young, professor of biochemistry, University College, London.

H.M. the King has approved the award of the Royal Medals of the Royal Geographical Society for 1949 as follows: Founder's Medal to Prof. L. Dudley Stamp, professor of geography, London School of Economics, for his work in organising and directing the Land Utilisation Survey of Great Britain and his application of geography to national planning; Patron's Medal to Prof. Hans Pettersson, professor of oceanography at Göteborg Högskola, Sweden, for his leadership of the recent oceanographical cruise in the *Albatross*, in the course of which much new evidence has been obtained as to the geology of the ocean floor.

The Royal Astronomical Society have awarded their Gold Medal to Prof. Sydney Chapman, for his contributions to geophysics and solar physics, and the Jackson-Gwilt Medal and Gift to Mr. A. M. Newbegin, for his observations of solar prominences and sunspots during the past forty years.

Other medals awarded during the quarter include the Gold Medal of the Zoological Society of London to Mr. H. G. Maurice, C.B.; Linnean Medal of the Linnean Society of London to Prof. D. M. S. Watson; the medals of the Geological Society: Wollaston Medal to Dr. Robert Broom, Murchison Medal to Dr. E. M. Anderson, Lyell Medal to Dr. W. J. Arkell, and Bigsby Medal to Prof. W. Q. Kennedy; Meldola Medal of the Society of Maccabæans for 1948, on the recommendation of the Council of the Royal Institute of Chemistry, to Dr. R. A. Raphael; Holweck Prize and Medal of the Physical Society and the Société Française de Physique to Prof. L. F. Bates; James Alfred Ewing Medal of the Institution of Civil Engineers for 1948 to Sir Edward Appleton; James Watt International Medal of the Institution of Mechanical Engineers to Dr. F. Ljungström of Sweden; Bessemer Medal of the Iron and Steel Institute to Prof. J. H. Andrew; Institute of Metals (platinum) Medal to Dr. W. Hume-Rothery.

The Brewster Gold Medal of the American Ornithologists Union has been awarded to David Lack, Director of the Edward Grey Institute of Field Ornithology, Oxford, for his book *Darwin's Finches*, published in 1947 by the Cambridge University Press.

Sir Robert Robinson, president of the Royal Society, has been elected an honorary member of the Parliamentary and Scientific Committee.

Sir Ben Lockspeiser has been appointed Secretary to the Committee of the Privy Council for Scientific and Industrial Research in succession to Sir Edward Appleton.

Prof. S. Zuckerman, professor of anatomy in the University of Birmingham, has been appointed deputy chairman of the Advisory Council on Scientific Policy and of the Committee on Industrial Productivity.

Dr. W. K. Slater has been appointed secretary to the Agricultural Research Council, in succession to the late Sir John Fryer.

Prof. J. W. McBain, F.R.S., has been appointed the first director of the National Chemical Laboratory of India.

Sir Wallace Akers, a director of Imperial Chemical Industries, Ltd., has been appointed a trustee of the National Gallery.

We have noted with great regret the announcements of the death of the following scientific workers during the quarter: Dr. S. H. Ball, mining consultant to the U.S. War Production Board; Prof. C. O. Bergstrand, formerly professor of astronomy in the University of Uppsala; Prof. T. Carleman, professor of mathematics in the University of Stockholm; Mr. W. C. Chesterman, O.B.E., assistant secretary of the University Grants Committee; Dr. A. Connal, O.B.E., formerly director of the Medical Research Institute, Lagos; Dr. A. Cunningham, agricultural bacteriologist; Prof. T. L. Davis, emeritus professor of organic chemistry in the Massachusetts Institute of Technology; Prof. J. Drach, professor of analytical mechanics at the Sorbonne; Sir Thomas Hill Easterfield, K.B.E., formerly director of the Cawthron Institute, Nelson, N.Z.; Prof. W. Garstang, emeritus professor of zoology in the University of Leeds; Sir William Hale-White, consulting physician to Guy's Hospital; Prof. D. Hay, chief mining engineer (management) of the National Coal Board; Mr. Will Hay, actor and amateur astronomer; Dr. A. D. Imms, F.R.S., lately reader in entomology in the University of Cambridge; Prof. C. H. Lander, C.B.E., dean of the Military College of Science, Shrivenham; Dr. F. H. A. Marshall, C.B.E., F.R.S., formerly reader in agricultural physiology in the University of Cambridge; Sir Archibald Page, formerly chairman of the Central Electricity Board; Prof. T. S. Patterson, emeritus professor of organic chemistry in the University of Glasgow; Prof. John Percival, emeritus professor of agricultural botany in the University of Reading; Mr. R. A. Todd, a Ministry of Agriculture and Fisheries inspector; Prof. R. C. Tolman, of the California Institute of Technology, vice-chairman of the U.S. National Defense Committee; Prof. Bailey Willis, geologist.

In January of this year, two Thomas Gray Memorial Trust lectures were given at the Royal Society of Arts. The subject of the first lecture, by Lieutenant P. G. Satow, D.S.C., R.N., was "Radar and its Application to the Science and Art of Marine Navigation." The lecturer began by outlining briefly the development of radar during the war, including a description of the continuously rotating aerial and the Plan Position Indicator, which together provide an all-round radar cover and a constant "aerial view" of nearby objects. In 1945, when the accumulated store of knowledge could be used to more peaceful ends, the naval scientific staff undertook the production of a prototype radar equipment which, for the first time, was to be designed basically for marine navigation. Several new features were incorporated, notably the ability to detect, at close range, small objects such as buoys. Many demonstrations of this equipment took place, including trials in the Norwegian fjords. Another set of trials made with special equipment on the dockside at Liverpool paved the way for the establishment in 1948 of the magnificent Harbour Supervision Radar at Gladstone Dock, which represents a major contribution to marine navigation.

Nowadays well planned and efficient radar sets are being fitted throughout the Merchant Marine, but the problem does not end there. Without the efficient training of operators, radar can be more of a danger than an asset and here again the Admiralty has been able to help by sending a nucleus of instructors to the colleges and schools of navigation. More convenient methods of interpreting the radar picture as, for instance, by optically matching it to the appropriate chart, are being introduced. In the future it may be found desirable to modify the existing charts by marking more prominently those features which show to best advantage on the radar screen. Despite the complexity of modern radar, the extent to which master mariners, pilots and officers have acquainted themselves with radar techniques bears testimony to the fact that it has become one of the most prized of modern aids to navigation.

The second lecture was given by Captain J. W. Josselyn, D.S.C., R.N. (Retd.), on "Weather Forecasting at Sea." Captain Josselyn began by explaining that the main body of seamen are only concerned with those aspects of the weather which may endanger or impede their ships, in particular the state of the sea and the visibility. Compared with the meteorologist ashore, their forecasts are relatively simple. However, the information upon which they have to base them, obtained from radio messages and their own observations, is far more restricted and, except in the case of meteorologists on board aircraft carriers, they know little about the state of the

upper air in their vicinity. As a result, simpler techniques have to be adopted in place of the more precise methods now in general use.

The state of the sea is largely dependent on the wind. Normally it is the wind blowing near the ship which will raise the sea, but a wind blowing hundreds of miles away can cause a swell. The lecturer dealt at some length with the forecasting of wind, including tropical storms. A tropical storm is a true vortex motion, whose origin is not clearly understood. Early warning of such a storm is given by a small but definite drop in the daily mean pressure. Unfortunately the seaman forecaster is unlikely to be in one area long enough to observe such a change and, except in the South-West Pacific, tropical storms have a habit of starting in places where there are few weather-reporting stations. The seaman has therefore to rely on other indications, in particular the presence of a swell, which cannot be explained other than by assuming a tropical storm in the direction from which the swell is coming.

Finally, Captain Josselyn explained the forecasting of sea fog, mist, drizzle and other causes of bad visibility. The advent of radar has greatly helped to reduce the dangers from reduced visibility, but, in the lecturer's own words, " it is still unpleasant to find oneself steaming through a thick fog with a few dots and smudges on the radar screen as the sole indication of the other ships and land in the vicinity."

The issues of the *Bell Laboratories Record* for the months December 1948 to March 1949 contain much interesting information, but unfortunately it is only possible to mention here a few of the topics discussed. In the December issue a very neat solution is given to the problem of designing a standard parallel plate capacitor of one-tenth of a micro-micro-farad. This capacitance is extremely small, being approximately a tenth of that between the heads of two drawing-pins when separated by a thin sheet of paper. In this particular case, if plates of reasonable dimensions are used, they have to be so far apart that, even with a guard ring, the lines of force tend to " bulge out " at the sides and the simple formula is not applicable. This difficulty was overcome by placing a cylindrical single layer coil of fine resistance wire between the outer edges of the guard ring and the opposite plate, each end of the coil being connected to the appropriate plate. The lines of force at the edges are now surrounded by a surface whose potential falls linearly between the plates, there is no tendency for the lines to bulge, and the simple formula applies. The measured value of a

condenser constructed on this design was found to be in very good agreement with that calculated.

Considered to be one of the most significant advances in telephone engineering since the introduction of the dial system, the "automatic message accounting" system (A.M.A.) has recently been installed in Philadelphia. By automatically punching holes in a tape 3 inches wide a record can be kept of the calling and called numbers and of the month, day, and time of the beginning and end of the conversation. The tapes are cut daily and taken to the accounting office where they are fed into five successive machines. The first of these groups together all the information on any one call. This is an extremely ingenious operation, as the beginning and end of a call may be separated by several yards of tape, representing many hundreds of telephone calls. Other machines convert the duration of each call into chargeable units, sort out the calls according to the directory number of the subscriber and print details of the calls for making out the bills. The tape on one reel, about 2 feet in diameter, holds about 25,000 calls when filled to capacity, and passes through the first machine in the surprisingly short time of about 2 hours.

In the January, February and March issues the great advances which have been made in the design of repeaters for telephone circuits are described. In the early days of the telephone it was realised that long-distance communication would be impossible unless the attenuation in the line could be compensated by suitable amplification. In 1903 Shreeve introduced his repeater, relying on the amplification which can be obtained by placing a carbon transmitter directly facing a receiver, but this type of repeater was never completely satisfactory. In 1915 the first vacuum-tube repeater was developed, and since that date the size and power consumption of repeaters have been successfully reduced. In the V3 repeater introduced in 1948, by using miniature components developed during the war, the space occupied by the amplifier has been reduced to a few hundredths of a cubic foot. This is an important consideration when many hundreds have to be installed. The V3 repeater, which is connected to the line circuit by means of an eleven-pin plug to facilitate replacement, is capable of giving a maximum gain of 36 db. that is substantially flat over the audio frequency range.

Another type of repeater has recently been developed, in this case for the radio-relay system between New York and Boston. Although the distortion produced in the prototype model was found to be extremely small, it was still necessary to know its value with

accuracy before completing the design. This was necessary because many repeaters of this type were to be used in tandem, when the distortion might build up to objectionable values. To measure the distortion in the one prototype model in the usual way would have been difficult and to build a number of prototype models for this particular measurement would have been awkward as well as expensive. Fortunately this was made unnecessary by the suggestion of G. W. Gilman that the same signal could be passed through the repeater a large number of times and the distortion measured after any desired number of transits. The circuit which was set up to do this consisted essentially of a loop containing the repeater, a delay line, an attenuator and a special amplifier. The delay in the loop was made longer than the duration of the signal and the net gain was made unity. A test pulse was introduced into the circuit and observed on an oscilloscope after different numbers of transits. In the article, the appearance of a square-shaped pulse is shown, by means of a set of photographs, after it has made 1, 3, 10, 30 and 80 trips round the loop. It must be mentioned that great care was taken in these measurements to see that none of the auxiliary equipment introduced appreciable distortion into the system.

In the issue of the U.S. *Journal of Research of the National Bureau of Standards* for December 1948 a neat method for measuring the temperature of moving parts, such as turbine blades, is described. The method, originally suggested by Gnam, was tested, using an experimental set up.

Measurements of this type have always been difficult to make. In this system the use of slip rings is avoided by employing instead a special magnetic converter to couple together two circuits, one of which contains a thermocouple and rotates with the machinery, while the other remains stationary. The thermocouple is connected in series with the rotor coils of the converter, one junction being attached to the point whose temperature is to be measured, while the other is brought out along the axis of rotation and is surrounded by a small furnace. Measurements are made by adjusting the furnace temperature until there is no current flowing in the rotating circuit, an oscilloscope connected through an impedance matching transformer to the stator coils of the converter being used as the null point indicator. When the null point is reached, the temperature of the furnace, as determined by conventional means, is equal to the temperature of the measuring junction.

To make the system work efficiently, any e.m.f.s resulting from

stray magnetic fields, such as the earth's, were reduced to a minimum. This was done by screening the coils of the converter with a Mu metal shield and by mounting the thermoelements coaxially. The system appears to have distinct possibilities, the principal limitations being the difficulty of fitting it to existing machinery and the fact that the temperature can be measured at only one point at a time.

One of the relatively unknown causes of power cuts, the formation of deposits on the external surfaces of water tube boilers, in air heaters, etc., is described in a press release from the D.S.I.R. dated February 1949. Owing to these deposits, plants may have to shut down after running for only 3–4 weeks and, as the cleaning process may take anything from a few days to a few weeks, this means that the "boiler availability" is seriously reduced.

The constituents of coal which are mainly responsible for these undesirable deposits are sulphur, phosphorus and the alkali metals. The amounts of these substances in the coal burned have been correlated with the severity of the deposits in over 70 British power stations. Phosphate and alkali matrix deposits have not so far been found in plants using pulverised fuel, so that the reservation of coal with the highest content of these impurities for boilers fired in this way is one of the most direct methods of alleviating the trouble. Other methods have been tried with success, including the introduction of smokes into the flue gases, and power stations have been advised on suitable methods of cleaning, both on and off load. Research is still continuing, and it is expected that a considerable improvement in boiler availability will soon result.

Although the development of an electron microscope which is capable of resolving detail of the order of 10 Angstroms is in itself a great achievement, an attempt is now being made to improve upon this performance by means of a new technique. In this new technique a diffraction diagram is formed and recorded. This diagram is then made to "recombine" into a "likeness" of the original object by means of a second diffraction process in which ordinary visible light can be used. By this means electron lenses, with their imperfections, can be eliminated from the formation of the final image.

A purely optical experiment illustrating this new method is described in the January–February 1949 issue of *B.T.H. Activities*. By means of a reducing process some words were printed on to a tiny square of transparent material 0·5 mm. × 0·5 mm. This square was illuminated by monochromatic light from a very fine pinhole 0·0003 cm. in diameter, and its shadow was recorded on

a photographic plate. Due to diffraction effects the shadow is unlike the original, resembling in many ways the appearance of a badly focussed negative. This shadow has been referred to as a "hologram," as it contains complete information about the object in a code which is not easily deciphered. By removing the object, placing the hologram in the position in which it was photographed and illuminating it with a pinhole source, the original object appears in the light beam and can be observed through a lens placed behind the hologram. Comparison of the words before and after the process described shows that the reproduction is surprisingly good, particularly when the appearance of the hologram is remembered.

This method is to be applied to electron microscopy by taking the hologram with electrons, and then reconstructing it using visible light. It is hoped that with this new technique it will be possible to resolve detail of the order of 1 Angstrom and so be able to "see" for the first time the very atoms of which matter is composed.

It is important when cutting a diamond for use as a tool or die that the crystallographic axes should first be determined, so that cutting can be in optimum directions. Failure to orientate the crystal correctly can result, in an extreme case, in the tool becoming useless when it has only done about one-hundredth of the work of which it is capable. Such a failure would probably be due to the fact that the hardness of a diamond varies considerably with direction in the crystal, measured relative to the crystallographic axes. As these axes cannot always be determined by inspection, an X-ray method has been used for many years. Unfortunately this particular method requires comparatively complicated apparatus including a special type of goniometer. To overcome this difficulty H. Vardhan has recently developed a simpler X-ray method which he describes in the December 1948 issue of the Indian *Journal of Scientific and Industrial Research.*

In this new method a Laue pattern of the crystal is taken, using simple apparatus which is readily available. Instead of laboriously interpreting each pattern as it is obtained, Vardhan has prepared a chart showing 55 Laue patterns of the same crystal taken at different angles with the X-ray beam, each pattern being " plotted " against the angles the X-ray beam made with two given directions in the diamond crystal. Thus, as each new pattern is obtained, it can be quickly recognised, either directly or by interpolation between neighbouring patterns on the chart, and the crystal correctly orientated before the cutting process is commenced.

REVIEWS

MATHEMATICS

Theory of Equations. By J. V. Uspensky. [Pp. viii + 353.] (New York, Toronto, London : McGraw-Hill Book Co., Inc., 1948. 27*s*. net.)

A course in the elementary theory of equations still forms part of the mathematical curriculum in most American Universities, and the present book is designed for use in such a course. Its subject matter is therefore fairly well standardised, and the approach is necessarily analytical rather than algebraic. But Prof. Uspensky's treatment is fuller and more scholarly than that of most previous writers, and he has taken great care not to slur over any of the difficulties.

Much attention is given to the problem of separating and evaluating approximately the roots of a given equation; and in this connection the author stresses the value of a method of Vincent, which consists essentially of making transformations of the form $x = a + y^{-1}$, $y = b + z^{-1}$, etc. The account of determinants and linear equations seems to the reviewer to be excellent, but algebraists may perhaps consider it rather old-fashioned.

There are five interesting appendices. The first outlines Gauss's first proof of the fundamental theorem of algebra. The second is on Vincent's method. The third gives Hurwitz's conditions for all the roots to have a negative real part. The fourth shows how to approximate to the largest root of the frequency equation by an iterative process. The last gives an account of Graeffe's method for the computation of complex roots.

The presentation is clear and detailed throughout and the book is one which will be of value to all who have to deal with algebraic equations.

H. Davenport.

Number Theory and its History. By Oystein Ore. [Pp. x + 370, with frontispiece and 24 figures.] (New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. 27*s*. net.)

Most mathematical textbooks suffer from an inadequate treatment of the history of the subject. In this book the author consciously aims to treat the theory of numbers from an historical point of view. The result is a very attractive volume which will appeal strongly to the educated layman, the scientist in general and also to the specialist who is already well acquainted with the subject, but wants more detailed information about its history, in particular about the recent discoveries in the history of ancient mathematics.

The first chapter of the book appears to the reviewer as the most valuable one. It gives a clear and concise account of the methods used to record numbers in the most important civilisations, *i.e.* Mayan, Chinese, Japanese, Egyptian, Babylonian, Greek, Roman, Hindu, Arabic and modern European. In the following chapters the elementary parts of the theory are developed

more or less in the usual way. But complete, accurate and up-to-date references are given throughout. The following are the main subjects covered in the book: Euclid's algorithm, prime numbers, diophantine analysis, representation of a number as a sum of two squares, congruences, Wilson's and Fermat's theorems, primitive roots and regular polygons. The quadratic law of reciprocity is not included. Many problems of the "puzzle" type are quoted, which were so important to the survival of mathematics during the Middle Ages.

No knowledge of mathematics beyond the Higher School Certificate level is necessary for a study of the book.

Good bibliographies and indexes are provided.

H. Heilbronn.

Regular Polytopes. By H. S. M. Coxeter. [Pp. xx + 321, with frontispiece, 8 plates and 88 figures.] (London: Methuen & Co., Ltd., 1948. 50*s*. net.)

This is a fascinating and stimulating book. The convex regular polyhedra, the five Platonic solids, have been known from time immemorial, but further developments as described in this book did not occur until much later. Kepler discovered two of the four regular star polyhedra—the two which have pentagrams (the simplest star polygon) as faces; the two other star polyhedra, with triangles and pentagons as faces, but with star-shaped vertices, were only discovered about two centuries later, by Poinsot.

Extension of the ideas connected with polygons and polyhedra to polytopes in four and more dimensions seems first to have originated with Schläfli, before 1853, but many other workers have studied such polytopes since then—mainly in isolation until the researches of Coxeter, into polytopes, and into the literature of polytopes, brought some unification to all those efforts. He has been indefatigable in searching the literature, in tracking down theses and unpublished work, and in making personal contact with workers in this field—British, Continental or American.

The book now under review summarises the work of Coxeter and others to an extent not previously accomplished; he is meticulous in his tributes to earlier workers, although he rediscovered a very large part of the field himself before earlier work had been unearthed.

The field is vast, so that this work is concentrated on *regular* polytopes, though other kinds are dealt with freely where they throw light on the regulars, and in such a way that exciting side tracks for further exploration are glimpsed, and in some cases partially explored.

Chapters I to VI deal with polygons and polyhedra in not more than three dimensions. The connection with Group Theory is dealt with thoroughly in Chapter III, Rotation Groups, and in Chapter V, The Kaleidoscope, which concentrates on Reflections. Chapter IV, Tessellations and Honeycombs, considers how planes can be filled with polygons, and space with polyhedra. Then, in Chapter VI, come the Star-Polyhedra.

The rest of the book, Chapters VII to XIV, extends into four and more dimensions. The existence is established of three regular polytopes in n dimensions—the simplex, cross-polytope and measure polytope (analogous respectively to tetrahedron, octahedron and cube) and of the cubic honeycomb (*e.g.* a plane filled with squares, or space filled with cubes). In four dimensions three special regular polytopes exist, one without analogue in more or fewer

dimensions and based on the symmetry of the tesseract (or four-dimensional measure polytope) and two with pentagonal symmetry. It is also shown that 10 star-polytopes exist in four dimensions, all having pentagonal symmetry, and that polytopes with such pentagonal symmetry end abruptly in four dimensions. It is also shown that star-honeycombs do not exist. The regular compounds consisting of several regular polytopes arranged so that their combination is still regular (*e.g.* 5 cubes with the vertices of a dodecahedron) are enumerated in three or four dimensions, with a glimpse into still higher space.

This brief statement does not give much idea of the domains of mathematics visited on the way: algebra, synthetic and analytic geometry, plane and spherical trigonometry, kinematics of a rigid body, the theory of groups and topology. Nor does it indicate well the unexplored fields; the regular polytopes as such are exhaustively dealt with, but many other less regular kinds remain to be considered—for instance, Archimedean or uniform polyhedra, with all faces regular, though not all the same, and with superposable vertices; of these the thirteen convex cases are fairly well known, though the star-polyhedra (of which 66 have been found) are less well-known. These ideas may be extended to more dimensions and to honeycombs. Then there are zonohedra, and the solids associated with reflection groups that do not give rise to regular polytopes, Gosset's polytopes, which reach a climax in eight dimensions, and so on.

The plates and diagrams of all the regular polyhedra and compounds, and of two- and three-dimensional representations of higher polytopes, are a prominent feature of the book, which winds up with eight useful tables of collected results, and a good bibliography and index.

The price of the book is high, but this should not be allowed to deter anyone interested in the subject, whether from recreational, educational or other aspects, from obtaining the book.

J. C. P. Miller.

ASTRONOMY AND PHYSICS

Kinematic Relativity. By E. A. Milne, M.A., D.Sc., F.R.S. International Series of Monographs on Physics. [Pp. viii + 238.] (Oxford: at the Clarendon Press, 1948. 25*s*. net.)

In cosmology we may usefully adapt a term of Eddington's and distinguish between a "uranoid" and the "universe." The universe is defined as the system we observe using the kind of observing apparatus which is in fact employed by terrestrial observers. It is a function of this apparatus and contains a liberal measure of extrapolation. For example, the universe as an assembly of extra-galactic nebulæ is a product of the 100-in. telescope and differs greatly from the universe of nineteenth-century astronomy. A uranoid, on the other hand, is a product of the scientific imagination: neither its contents nor the observing apparatus available to its observers need be the same as those in the universe, any more than the world imagined by Lewis Carroll is necessarily identical with that of sense-experience.

The uranoid of kinematical relativity is characterised by a definition of distance based on the exchange of light-signals between observers and by invariance under Lorentz transformations. The distance-definition implies that only time-transformations ("scales of time") are fundamental and not the joint time-and-distance transformations of Newtonian and Einsteinian

mechanics. Thus the reader of this book must be on his guard to remember that symbols such as r, x, etc., which normally stand for distances measured, directly or indirectly, with material scales and are independent of a particular observer's way of measuring time, now stand for quantities that are alterable by simply changing the rate at which the observer's clock is working. Attention is concentrated on the dynamics of particles, gravitation, electromagnetic theory, atomic systems, etc., in the uranoid consonant with this distance-definition and with Lorentz invariance. The quantities which here play the parts of the universe's gravitational constant and of Planck's constant turn out to be time-variables, when an appropriate time-scale is chosen, results traceable to the altered meaning of distance. Unlike the universe, whose extra-galactic nebulæ are largely spheroidal or irregular in form, the uranoid apparently contains only spiral nebulæ and no solar systems. Thus the difficulty that the gravitation theory does not predict the rotation of the perihelion of Mercury is avoided and attention can be devoted to the shapes of the arms of the spiral nebulæ, regarded as the present positions of particles emitted at various epochs from one or more fixed points. In the universe, it is observationally very doubtful whether spiral nebulæ are in a state of internal motion at all; certainly no "orbit" of a portion of a nebula has been determined.

In the atomic and electromagnetic theories of this uranoid, electrons have no spin, photons conserve their energy in spite of the red-shift and various other ideas and formulæ applicable to the universe are modified. Whether the universe can be described in terms of such a uranoid is a matter for each reader to decide for himself, but all will agree that, as a mathematical construct, its ingenuity and beauty cannot be surpassed.

G. C. McVittie.

Introduction to Physics. By Harley Howe. Second edition. [Pp. xii + 599, with 505 figures.] (New York, Toronto, London: McGraw-Hill Publishing Co., Inc., 1948. 27*s*. net.)

This is a pleasant book to handle and to read. Published in U.S.A., it is well printed and excellently illustrated, and it is written in a lively style, likely to excite the interest of an elementary student of physics. Though the scope of the book approximates to that of H.S.C. in the subject, the treatment is much more descriptive and superficial, and it is best used as an introductory text before a more detailed and quantitative treatment. There is little mathematics beyond fourth-form standard.

The topics range from the simplest mechanics to the synchrotron, but many of them get only a brief mention. The whole of light, including luminescence, diffraction and polarisation, is covered, for example, in 100 pages—one sixth of the book. There are, however, scattered through the text useful hints and tips to the student, and many questions and examples. At the end of each chapter there is a brief summary. The whole outlook is up to date, and, though every teacher will think that some topics could be treated better his way, the text is generally sound and reliable.

In the second edition some chapters have been rearranged, there is more emphasis on alternating currents, and answers to the problems have been added. The book can be recommended to fifth and sixth forms of schools, to public libraries, and as an introductory text for first M.B. students.

F. A. V.

The Elements of Physics. By Alphеus W. Smith. Fifth edition. [Pp. xvi + 745, with 4 plates and 917 figures.] (New York, Toronto, London: McGraw-Hill Publishing Co., Inc., 1948. 30s. net.)

That this book has reached a fifth edition is some measure of its success. It was first published in 1923, and the fourth edition was reviewed in these pages in 1939 (Vol. 34, p. 158). Physics is, however, a rapidly growing subject, and the changes in outlook as well as in content over 25 years should be reflected even in a comparatively elementary text. It is a tribute to the care with which Prof. Smith has carried out the revision that there are very few places in which the book is out of date, the only one noticed of any consequence being the definitions of the photometric standards (p. 514). A number of the illustrations are new.

The intentions of the author are perhaps best expressed in his own words: "The central aim has been to introduce students to physics as a way of thinking that is largely characteristic of our daily lives and to develop in them a capacity for seeing relations between physical phenomena and for applying universal physical principles. Throughout the book, the attempt has been to lay a basis for correct scientific thinking rather than to stress the accumulation of facts or to trace the historical development of the subject." The book is thus not written to cover any examination syllabus, and there is a very wide range of topics, from the most elementary mechanics to Compton scattering, the relativity correction to electron mass, the Michelson interferometer and the interpretation of spectral lines. It is inevitable that the treatment is superficial, but the book, and especially some of the applications of physics mentioned as examples, is so interesting that it should stimulate the enquiry for more detailed and quantitative information. There are a few rather curious omissions; for example, there is no clear statement of the sign convention adopted for the mirror and lens formulæ, and some very good photoelastic patterns are reproduced (Figs. 185–188) without any explanation of how they are obtained or interpreted.

The book is excellently printed and bound, and copiously illustrated—even luxuriously so, since some of the illustrations are more decorative than informative (*e.g.* Figs. 85, 100, 203, 264, 625). There is a good index, and appendices containing additional mathematical work. Though the price is more than many students can afford, it is not high considering the size of the book and its quality. It would be read with interest by those in fifth and sixth forms, and should be a valued addition to public libraries.

F. A. V.

Physics Revision Course for Higher School and Intermediate Examinations. By V. H. L. Searle, M.Sc. [Pp. viii + 120, with 74 figures.] (London: Edward Arnold & Co., 1948. 6s. 6d. net.)

Most of the basic concepts, definitions and units of intermediate physics are assembled in this book, many of the relevant formulæ being derived. Quite appropriately the first chapter is on examinations in physics and contains some good advice to candidates. Very few numerical examples are worked out and none are left for the student. There is a comprehensive index.

A glance at the book produces a favourable impression, but a closer inspection brings to light such unsatisfactory features as the following: Unconventional symbols are quite frequently used, and, on p. 30, M appears both as thermal capacity and mass. k is used to a remarkable extent and it is

not surprising that $pv = k$ on p. 28, but $= kv_0^2$ on p. 37. Although the author advises students to "state the relevant units of the quantities described," he makes no attempt consistently to set a good example; sometimes he states units, sometimes he omits them and sometimes he uses the convenient "c.g.s. units"; in one case, namely specific heat, the units are twice wrongly recorded (p. 36). Fig. 14 does not refer to Young's modulus, and "work of free expansion" is hardly a fitting caption to an indicator diagram. The treatment of thermal conductivity is only valid, provided the steady state has been attained; yet this important point is omitted.

Such criticisms as these lead the reviewer to the unwelcome conclusion that the book itself needs a careful revision before it can honestly be recommended.

J. W. F.

Thermodynamics. By LESTER C. LICHTY. Second edition. [Pp. xiv + 341, with 152 figures and 12 charts.] 27*s*. net.

Thermodynamics. By EDWARD F. OBERT. [Pp. xiv + 571, with 207 figures and 9 charts.] 33*s*. net.

(New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948.)

THE last twenty years have seen the publication of a variety of texts upon thermodynamics. Prof. Lichty in the preface to the first edition of his work probably expresses the feelings of many authors of these when he says: "The subject of thermodynamics has so baffled the average student that it has acquired the undesirable reputation of being a most formidable subject. . . . This text is an attempt to simplify the treatment, to strip it of many of the mathematical manipulations for which there is no excuse except to obtain an equation into which data can be substituted without fundamental analysis. . . ."

This quotation provides a convenient means of assessment of the two works here reviewed, since each is intended as a text for undergraduate students of engineering.

Prof. Lichty commences with a very brief chapter upon defining co-ordinates, pressure, and temperature, in which absolute temperature is defined in terms of gas scale temperatures with no mention of a thermodynamic scale. It is not until p. 141 that the thermodynamic scale is encountered, when a brief discussion is given which would not impress its unique character upon a student meeting the concept for the first time. In the second chapter the author discusses energy and makes the ambiguous statement, p. 20, that the recommendation at the 1929 International Steam Table conference defining the calorie as $\frac{1}{860}$ watt hour eliminates the necessity of further experimental determination of the mechanical equivalent of heat. Entropy is soon introduced by writing a heat transfer as the product of an intensity factor, temperature, and a qualitative factor, entropy. It is the concept of entropy and its peculiar properties which often proves such a stumbling-block to students; this bald introduction is not likely to clarify their ideas.

These preliminaries occupy rather less than thirty pages, the author then passing on to the general energy equation and the analysis of processes, and thence to the properties of mediums and an analysis of various thermodynamic processes. The major part of the book is devoted to practical problems in real cyclic processes, flow of fluids, and combustion processes.

Temperature entropy diagrams are used extensively and the clarity with which processes having a practical application are described is much to be commended. The text is well illustrated and there are numerous worked examples as well as a profusion of exercises at the end of each chapter. Most of those demand the use of the tables and charts of thermodynamic functions provided at the end of the volume.

How far the aims of the author as expressed in the preface have been fulfilled is difficult to decide. The impression left upon the reviewer is that a student conscientiously reading the text and working the examples would have a good working knowledge of the application of thermodynamics to practical problems, but how far simplification of the subject has been achieved by limiting the discussion of fundamentals is another question.

Prof. Obert's book is intended to cover much the same ground as the above work, but the method of approach is more orthodox. After a survey of units and dimensions in the first chapter, the second starts with the sentence "It is necessary in any subject to define the fundamental concepts rigorously, for these definitions lay the foundation for a workable theory." This chapter deals with fundamental concepts and gives a survey of a field often inadequately dealt with. Chapter III introduces the first law of thermodynamics, the general energy equation being used as the starting-point for a discussion of compressors, pumps, turbines, and other devices, so that the importance of the first law in practical applications is clearly demonstrated. The next two chapters, dealing with the reversible process and the second law, occupy nearly sixty pages and comprise a very useful and clear account of this part of the subject.

Chapters follow on the properties of fluids, characteristics of gases, and approximate calculations for real gases. There are numerous worked examples, mostly of a kind where the application to engineering problems is immediately apparent. The discussion of the flow of fluids, thermochemical calculations, and power cycles in subsequent chapters has the same practical outlook. A short but adequate chapter on refrigeration concludes the book. Each chapter is well provided with problems, and most have a list of references for further reading. Appendices and charts at the end of the volume give thermodynamic properties of a number of fluids. The book could very usefully be read by physics students of a practical bent to whom thermodynamics too often appears an abstract mathematical exercise.

F. E. H.

Heat. By A. G. WORTHING and D. HALLIDAY. [Pp. xii + 522, with 245 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 36*s*. net.)

PROF. WORTHING is "one of the outstanding authorities on heat" in the U.S.A., and his investigations of phenomena at high temperatures are well known in this country. Accordingly, this book, of which he is the senior author, hardly needs a recommendation.

The subject is mainly "classical heat" and it is treated from the experimental point of view. The titles of the chapters in order are: some laboratory procedures; temperature and its measurement; the expansivities of solids and liquids; the dynamical theory of heat; calorimetry; specific heats of solids and liquids; thermal conduction of solids and liquids; thermal properties of gases; elementary thermodynamics; change of phase; heat

engines, refrigerators, and human power plants; convection; and radiant energy.

The book contains a lucid exposition of fundamental principles and is characterised by a wealth of experimental illustration. A reasonable proportion of the experimental work is of fairly recent origin and is not to be found in any other general textbook known to the reviewer. Many interesting applications of thermal phenomena both in nature and industry are discussed. At the end of each chapter there are problems—in general far too many for the average student. The tables contain much data likely to prove valuable to research workers.

There are omissions, *e.g.* the experimental realisation of the absolute scale of temperature and a modern determination of J. The authors admit that American work is "overemphasized." Slips occur, such as the omission of J from equation (7), p. 134, and the obvious errors in the bottom row of Table I, p. 210, and equation (25), p. 301. Sources of confusion to the English reader are the use of lb. for poundal; η for Reynolds' number and ν for ordinary as distinct from kinematic viscosity; and vapour tension for S.V.P. Such a reader must also excuse certain inelegant phrases.

In view of its high price, libraries should acquire the book, so that students will not be denied the opportunity of consulting what is on the whole a most satisfactory textbook. J. W. F.

The Interpretation of Spectra. By WILLIAM MAYO VENABLE. [Pp. vi + 200, with 12 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. 36*s*. net.)

THE interpretation of spectra here presented differs fundamentally from that currently accepted. It is based upon numerical relationships which the author has discovered between line frequencies in many spectra. If these were genuine his rejection of the quantum theory of spectra, his introduction of sub-atoms, and his other heresies, would have to be taken seriously; but a comprehensive examination of the validity of these relationships would be a long and arduous task. Most spectroscopists will no doubt decline to abandon the present theory, which, although it does not account completely and precisely for the detailed structure of spectra, does give a simple and satisfactory explanation of all their main features. It may be, of course, that familiarity with the orthodox viewpoint prejudices one against the radically different approach proposed by Mr. Venable, but an honest and moderately prolonged attempt to appreciate this on the part of the reviewer has induced a condition of bewilderment mingled with exasperation.

Two quotations may suffice to indicate the general tenor of the book. First (p. 54): "The atomic volume of helium is 2·36 times that of hydrogen, which suggests that the helium atom is a hydrogen molecule with something added, which makes it heavier, much more stable and consequently chemically inert. It also suggests that atomic volume is attributable to the structure of ions rather than to the positions of electrons with respect to them." The second (p. 51) proposes a new interpretation of some of the He_2 bands on the following basis: "There are, however, several methods of making the correction, the most direct being to identify the 1X level of Curtis with the hydrogen level *baa*, of which the wave number is 12345·10. This requires addition of 20·8 wave numbers to Curtis' 1P level and to all other levels with which it combines directly to give band heads. . . . The corrected

initial Z level is then found to be the hydrogen level *abb* less 106·71 ; this is probably due to *bbaaa–abbbb* which will be found . . . to be 106·09. The discrepancy is not beyond the range of probable error, though it is not sufficiently small to make it certain that some other combination . . . may be preferable when the problem is fully solved."

For the benefit of the older generation of spectroscopists it may be remarked that the present work is strongly reminiscent of that of W. M. Hicks. There is the same preoccupation with the wave numbers of spectral lines, to the exclusion of their other characteristics, the same obsession with numerical relationships of very questionable significance ; and in all probability the author will find the same difficulty in securing any general acceptance of his views. His immense industry and enthusiasm, and the high quality of the production, deserve a better reward.

W. E. C.

Physical Aspects of Colour. By Dr. P. J. Bouma. [Pp. 312, with 113 figures.] (Philips Technical Library, distributed by the Cleaver-Hume Press, Ltd., London. 30*s*. net.)

Dr. Bouma had occasion to study colorimetry in connection with his work at Eindhoven on vision and illuminating engineering, and we have an account here of the approach to the subject which in his experience was the best. The book was written during a long illness which eventually proved fatal, his colleagues being responsible for this English translation of the original Dutch text.

The book is very largely confined to a discussion of the principles and application of the system of colour measurement introduced in 1931 by the International Commission on Illumination, a system based on the experimental fact that colours can be expressed in terms of the mixture of three stimuli. The controversial field of colour vision theory is deliberately avoided, although certain visual phenomena outside the immediate confines of colour matching data, such as colour adaptation, colour discrimination and defective colour vision, are considered, while there is also an interesting chapter on the historical development of colour science. In general, however, this is a book of special value to those who have a technical interest in colour.

Colorimetrists will be intrigued by the author's method of writing colour equations, since the use of the ordinary type of algebraic equation to express the fact that two lights of different spectral composition have the same brightness and colour, has been the subject of a good deal of criticism, especially from the United States. The objection has been made that, in such an equation, there is in fact no equality between the two halves of the equation in the usual algebraic sense, and that it is misleading and confusing to use a well-established symbolism in a new context and with a different meaning. Dr. Bouma seems to have met this criticism effectively.

His distinction between the objective qualities of the light stimulus and the subjective attributes of the sensation is perhaps not quite as clear as it could be, and there is some confusion in the earlier part of the book as to whether the colour triangle is a stimulus diagram or a chart of colour sensations. Subsequently, however, the legitimate uses of the colour triangle are brought out quite clearly.

This is a valuable book and we can only regret the death of the author at such a fruitful stage of his career. W. D. Wright.

Photography in Crime Detection. By J. A. Radley, M.Sc., F.R.I.C. [Pp. 186, with 104 figures, including 72 plates.] (London: Chapman & Hall, Ltd., 1948. 21*s*. net.)

This is not so much a textbook of photographic technique as a book to stir the interest of any who may have the opportunity to apply photography in crime detection. The subject is treated by first describing the aims of photographic methods, supported by numerous examples, and then by giving just sufficient indication of technique to enable the photographer to experiment intelligently.

It appears that by far the most important application of photography to crime detection is the rapid and accurate recording of enormous quantities of detail for subsequent study by—if necessary—several investigators. An important additional use for this mass of detail is the simplification in presenting evidence in a convincing way to the accused and to judge and jury. Photography has, however, a further use in this work since it can make visible some things which could not be recorded in any other way. Enhancement of contrast, ultra-violet and infra-red photography appear to be the principal methods of recording the invisible, and sufficient technical information about all these is given.

The book is well printed and very well illustrated. A good index and bibliography with references to specialist sources of information are provided.

The reviewer noticed few typographical errors. There is, however, a reversal of descriptions of the photographs in Fig. 95.

S. O. Rawling.

Optical Design and Lens Computation. By B. K. Johnson. [Pp. viii + 175, with 80 figures, including 5 plates.] (London: The Hatton Press, Ltd., 1948. 36*s*. net.)

Some years ago, when logarithmic computation was generally employed in the optical industry, the late Prof. A. E. Conrady wrote a book entitled *Applied Optics and Optical Design* which has enjoyed great popularity as a textbook and guide among lens designers. The present volume consists substantially of numerical examples illustrating Conrady's procedure. Frequent references are made to Conrady's book, which must be consulted for the theory. The examples relate solely to rays lying in an axial plane of a rotationally symmetrical instrument, and to refracting surfaces which are either plane or spherical. The work is wholly logarithmic and trigonometric.

In recent years lens designers have tended to employ calculating machines instead of logarithms. To obtain full advantage of these mechanical aids the routine adopted in logarithmic work needs modification, and the changes may go so far that little resemblance remains. The alterations may bring important advantages; for instance, some forms enable skew rays to be calculated as readily as axial rays, and refracting or reflecting surfaces formed by rotating any plane curve about a normal can be treated. Recently much attention has been given to the design of optical systems embodying such non-spherical surfaces, to methods of manufacturing them with the requisite accuracy, and to means of testing their perfection of form. With these present-day problems the book now considered does not deal. The method adopted here in fact is inappropriate, and for this reason will probably become less valuable to the professional designer than it was in the past.

In the opinion of the reviewer this volume should be regarded as a supple-

ment to Conrady's book. Beginners interested in the design of the traditional types of instrument, who are obliged to work without the mechanical aids now regarded as desirable, if not essential, may well find it helpful. The author lays stress on the care taken to ensure freedom from numerical errors. There are five chapters; the first is of a general character, and the rest deal with telescope objectives, eyepieces, photographic lenses, and microscope objectives respectively. T. S.

The Optical Principles of the Diffraction of X-rays. By R. W. James, M.A., B.Sc., F.Inst.P. [Pp. xvi + 623, with 224 figures, including 16 plates.] (London: G. Bell & Sons, Ltd., 1948. 80*s*. net.)

This long-expected book forms Vol. II of the series *The Crystalline State*, edited by Sir Lawrence Bragg, of which the first volume, bearing the same title as the series, was written by Prof. Bragg as an introductory survey of the whole field and appeared in 1933. The present volume deals with X-ray diffraction as a branch of optics. Only to a lesser degree does it propose to lead up to the practical methods of structure analysis of crystals to which Vol. III of the series will be mainly devoted. In fact, this is the first English book treating the matter from this angle, and it is all the more welcome as its account is nearly complete, detailed but not overloaded, and altogether very readable.

Many of the problems tackled nowadays by X-ray methods require for their answer a deep penetration of the optical principles. For work not aiming at disclosing new crystal structures it is generally not the diffraction pattern of the normal kind which is of interest, but rather the faint or rare effect which might often be passed by in structure analysis. Problems of this kind are the imperfections of crystals due to growth or to subsequent deformation, the study of the forces between the atoms as revealed by the effects of temperature on the diffraction, the external shape of submicroscopic crystals, and the intimate study of the crystal as a refractive and absorptive medium for X-rays.

These and other topics, classified as problems of optics, are given a thorough discussion, which is well up to date in spite of the long period (1933–47) over which the preparation of the publication extended. During this period much the same subjects were treated on the basis of the optical principles by M. v. Laue in his book *Röntgeninterferenzen* (Leipzig, Akadem. Verlagsgesellschaft, 1940); both books draw largely on v. Laue's previously published papers, v. Laue adding further original discussion and James giving a more restricted but thereby more easily readable account.

The scope of the book will best be seen from the headings of the ten chapters:

I. The Geometrical Theory of Diffraction by Space-Lattices; II. The Intensity of Reflection of X-rays by Crystals; III. The Atomic Scattering Factor; IV. The Anomalous Scattering and Dispersion of X-rays; V. The Influence of Temperature on the Diffraction of X-rays by Crystals; VI. Experimental Tests of the Intensity Formulæ; VII. The Use of Fourier Series in Crystal Analysis; VIII. Laue's Development of the Dynamical Theory—Kossel Lines; IX. The Scattering of X-rays by Gases, Liquids and Amorphous Solids; X. Diffraction by Small Crystals and its Relationship to Diffraction by Amorphous Material.

A particularly difficult chapter to write in this book must have been

Chapter VII, since the application of Fourier methods in crystal analysis will be fully dealt with in Vol. III of the series. The chapter, as it stands, presents a clear discussion of the theory of the Fourier method without reference to technical detail. It seems a pity that Bunn's fly's eye method could not have been discussed as a further application of optical principles.

The only other omission noted by the reviewer is that of multiple, or simultaneous, reflexion and its influence on the intensity.

There are welcome historical introductions and well-balanced references in the various chapters, which bear testimony, like the entire presentation, to the fact that the author belongs to that circle of intrepid and indefatigable pioneers who were united in Manchester under W. L. Bragg's leadership. This Vol. II forms a worthy sequence to Vol. I and will help maintain the safe foundation of all X-ray diffraction work on the principles of optics.

P. P. EWALD.

The World and the Atom. By C. MØLLER and EBBE RASMUSSEN. Translated from the second Danish edition by G. C. WHEELER and B. MIALL. [Pp. 193, with 41 figures, including 8 plates.] (London: George Allen & Unwin, Ltd., 1948. 12*s.* 6*d.* net.)

THIS is a well-written book, the outcome of a happy partnership between a theoretical and an experimental physicist. In it is traced the progress of atomic and nuclear physics from the last decade of the nineteenth century to the present day. The treatment is thoroughly sound and very well balanced, and the book could be read with pleasure and profit by all those with an interest in the subject and a knowledge of physics up to about matriculation standard. The historical approach is used to advantage, and the authors, by drawing attention to the nationalities of the various scientists who have contributed so much to the progress during the last fifty years, have emphasised the importance and reality of international collaboration. A more suitable title of the book might perhaps be " The World *of* the Atom."

The translation reads smoothly, except for occasional minor infelicities (*e.g.* " metre measure " for " metre rule," " a blend of powder and " instead of " a mixture of . . ."). For this edition, the book has been revised and brought up to date (to 1947), though even now the meson is mentioned only in a footnote. (The discoveries of different forms of meson are, naturally, too recent to be included.) There is practically no mathematics.

The book is specially recommended for school and public libraries.

F. A. V.

A Textbook of Radar. By the Staff of the Radiophysics Laboratory, Council for Scientific and Industrial Research, Australia. [Pp. viii + 579, with 347 figures, including 31 plates.] (London: Chapman & Hall, Ltd., 1948. 50*s.* net.)

EACH chapter of this book is by a different author, but the whole fits together to form a very readable account of radar principles and practice. That the book, written and printed in Australia, should also be published in this country is an indication of the great increase in research and engineering development in that Dominion during recent years. It also demonstrates how little mere distance matters in these days, for most of the work described was carried out in Britain or the U.S.A., but was clearly followed very closely in Australia.

There does not appear to be any book written in this country which covers the same field. The reader is assumed to have a knowledge of ordinary radio circuits and from this basis the special problems of radar are discussed. There are chapters on magnetrons, modulators, waveguides, transmission lines, aerials, receivers, display circuits, etc., and these are followed by some descriptions of actual equipments which were in use during the war. A chapter on radar navigation concludes the book.

The story of radar development and its effect on various phases of the war is dealt with in an interesting manner, though this sometimes entails discussing obsolescent methods of working.

Most of the references are to war-time confidential reports. This is very misleading, because in nearly all cases the information has now been available to the general reader for some time, for example, in the *Journal of the Institution of Electrical Engineers* or in the American *Radiation Laboratory Series*. This may be due to the time taken nowadays to get a book published, but, if a second edition is called for, the authors would be well-advised to give references which are generally available.

The book is well-produced—print, line diagrams and photographs are all good—and it can be recommended as a thorough, general guide to the subject.

C. R. S.

Radar Scanners and Radomes. Edited by W. M. CADY, M. B. KARELITZ and L. A. TURNER. Massachusetts Institute of Technology, Radiation Laboratory Series, Vol. 26. [Pp. xvi + 491, with 232 figures.] (New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. 42*s*. net.)

THE twenty-sixth volume of the Radiation Laboratory Series is concerned mainly with the engineering problems arising from the design of antenna mountings and scanning mechanisms. The authors do not purport to deal with equipment operating at wavelengths longer than 10 cm. and confine themselves to designs which were developed at the Radiation Laboratory.

Many of the terms used in the text have meanings only recently acquired and the authors are to be complimented on having included a glossary of terms which, to the uninitiated in this new form of technical language, will serve as a useful guide.

The book is divided into two parts; Part I is written largely for the benefit of the mechanical engineer, while Part II is devoted to the associated electrical problems. There is an introduction to Part I dealing with the functions of radar scanners, and this is followed by chapters giving detailed consideration of these devices for ground, sea-borne and air-borne use. In the two latter cases the important question of mechanical stability of the antenna is fully discussed. The concluding chapter of Part I contains a general review of Scanner Control Mechanisms.

The aspects of electrical design of the part of the antenna housing which is transparent to R-F radiation, *i.e.* the "radome," are treated in Part II. Following a survey of radome problems, the factors in design of normal-incidence and streamlined radomes are outlined and the theoretical considerations involved in the transmission of electromagnetic waves through dielectric materials are enumerated. The remainder of the book is given to a discussion of radome materials, methods of fabrication and the problems relating to radome installation and testing.

It is fitting that the associated mechanical and electrical problems connected with the design of antennæ should be linked together in a book of this kind, which emphasises the need for closer co-operation between the mechanical and electrical engineers involved in such work. Much of the material, particularly that in Part I, is given here for the first time, and the book is very well illustrated. It is, however, written for the specialist in this field and to him it will serve as a valuable reference work.

JOHN LAMB.

Radio's Conquest of Space. By DONALD MCNICOL. [Pp. x + 374, with 53 figures.] (New York: Murray Hill Books, Inc.; London: Chapman & Hall, Ltd., 1946. 18*s*. net.)

PRESIDENT of the Institute of Radio Engineers in 1926, Donald McNicol relates a fascinating story of the growth of experimental radio from the nineteenth century. He begins with a brief and interesting account of electricity from 1800, with emphasis on the discovery of the oscillatory discharge of a condenser and the experimental discovery of electromagnetic waves. There are detailed descriptions of Marconi's early experiments to extend the range of radio waves; of early transmitters employing arc and spark gaps, and of early detectors such as the magnetic and electrolytic detector. The first half of the book closes with an account of the theories of the propagation of radio waves from 1900 to 1920.

The most interesting section of the book describes the invention of the diode and the triode, and the impetus they gave to radio development. The chapter on regenerative and other early radio receivers is particularly good; it contains many diagrams of historic importance, including a triode detector patented by de Forest in 1908, a Marconi receiver of 1913, and the earliest neutrodyne receiver by Hazeltine in 1919. As might have been expected the book abounds with names of American pioneers of radio known personally to the author; the relatively few British names mentioned include those of J. A. Fleming, E. V. Appleton, and G. W. O. Howe. There is a valuable name and subject index.

This excellent book is written in a pleasant style, and will be read with interest by everyone engaged in radio.

M. NELKON.

Problems in Electrical Engineering. By S. PARKER SMITH, C.B.E., D.Sc., M.I.E.E. Fifth edition. [Pp. xviii + 378.] (London: Constable & Co., Ltd., 1948. 10*s*. net.)

A PROBLEMS book is of necessity a somewhat pedestrian enterprise. Its compiling is as arduous as the writing of a continuous text, but does not afford the same æsthetic satisfaction. It demands the critical judgment, even the instinct, of the collector of water-colours or china, but yields no particular elegance to appeal to the connoisseur. It needs the meticulous and methodical technique of the lexicographer, but does not aspire to the authority possessed by a dictionary. The student is therefore much indebted to Prof. Parker Smith, not only for compiling such a book, but also for persisting with it through many revisions over a period of twenty years.

This fifth edition contains 20 per cent. more problems than the fourth, making nearly 1700 in all (the numerical result being quoted after each), and the more detailed and logical classification here adopted is a considerable improvement. An extremely wide range of topics in electrical engineering

is covered, including, with certain exceptions, all of the major, and many of the minor, syllabus subjects of the orthodox electrical engineering degree course. The exceptions are those parts of the electronics and telecommunications courses that have developed so rapidly in the last decade: some problems on valves and circuits are included, but they are now not nearly adequate. Another significant deficiency is in the sections on units and fundamentals; the student will search in vain for problems that reflect his growing familiarity with M.K.S. and rationalised systems, and he might be perplexed by the peculiarities of the dimensional analysis implied in these sections.

But these are minor blemishes. The book is of enormous value in providing just that discipline of numerical calculation that is defined by Prof. Moullin as the principal feature distinguishing the engineering student from the physicist. It will continue, deservedly, to be widely used.

H. MARRIOTT.

Photoelasticity. Vol. II. By MAX MARK FROCHT, Ph.D. [Pp. xviii + 505, with frontispiece and 425 figures, including 1 coloured plate.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 60*s.* net.)

As a book of reference for the photoelastician this volume is well worth its cost, for, though it contributes very little that is new to our knowledge of the subject, it contains a wealth of instruction and information which cannot fail to be of value and interest to all workers in the field of stress-analysis.

The first nine chapters contain an amplification and extension of the two-dimensional theory, and include several theoretical solutions for loads applied to semi-infinite plates, rectangular beams and discs. These are compared with the results obtained by photoelastic investigation, and the comparison demonstrates very convincingly the reliability of the practical method in the case of two-dimensional problems. The elastic theory in this section is presented very clearly, the mathematical procedure being explained more fully than is usual in this subject, and in a form which will appeal to the less mathematically-minded reader.

The treatment of three-dimensional stresses in Chapters 10, 12 and 13 is somewhat empirical, and is confined to the consideration of a few special cases. Various methods of examining the fringe-patterns in frozen-stress models are described, and these are illustrated by interesting examples. The chapter on frozen-stress technique is particularly useful, for it contains a number of useful hints, and reveals some of the secrets of the author's striking photographs of frozen-stress fringe-patterns. There are brief accounts of the as yet undeveloped "scattered light" and "convergent light" methods, with illustrations of the use of the former in problems involving de St. Venant torsion. The book is exceedingly well produced, and the many photographs are a monument to the skill of Prof. Frocht and his technical assistants.

H. T. J.

An Introduction to Photoelastic Analysis. By ARNOLD W. HENDRY, B.Sc.(Eng.), Ph.D., A.M.Inst.C.E., A.M.I.Struct.E. [Pp. viii + 152, with 77 figures, including 8 plates.] (London and Glasgow: Blackie & Son, Ltd., 1948. 7*s.* 6*d.* net.)

THIS is an excellent little book for the engineer who wishes to acquire the fundamentals of the photoelastic method of stress-analysis, for in it he will

find a concise account of the principles, methods and technique of two-dimensional photoelasticity presented in a form which should be acceptable to a reader who does not want to delve too deeply into mathematical or optical theory.

A summary of the essential results of elasticity and optics is given in Chapters 1 and 2, the remainder of the book being devoted to an essentially practical exposition of the photoelastic effect in a stressed plate and the method of conducting investigations of stress-distributions. The explanations of this two-dimensional effect are admirably clear, and the descriptions of apparatus and experimental methods are sufficiently full to enable a reader with no previous knowledge of the subject to assemble the necessary equipment and to carry out simple investigations. The book is fully illustrated with clear diagrams and excellent photographs of apparatus and of fringe-patterns in photoelastic models.

In only one instance has the author failed to keep the text within the easy understanding of the non-specialist, and that is in his reference in Chapter 2 to the wave-surface and the double refraction of an oblique ray in a crystal—a piece of theory much too difficult to be dealt with in such a short summary. No application of this theory, however, appears in the subsequent chapters, and failure of a reader to understand this section will not detract from the practical value of the book.

H. T. J.

Engineering Metallurgy. By W. E. Woodward, M.A. (Cantab.). [Pp. x + 176, with 35 figures and 6 plates containing 36 photomicrographs.] (London: Constable & Co., Ltd., 1948. 15*s*. net.)

In the preface, the author, who is University Lecturer in Engineering at Cambridge University, justifies this addition to the numerous books aiming at exposition of metallurgy for students of engineering on the grounds that "no one book contains all the information necessary in a small compass." He also notes his own surprise during recent industrial practice in discovering the reliance of design departments on trade names for their materials and hopes that the book will reach engineers concerned with investigation of failures.

It is doubtful whether the provision of adequate information for design departments and the exposition of the principles of metallurgy for engineers can be combined in a book of small compass. The provision of handbooks does not so far seem to have appealed to British technical genius and the best books of this type have undoubtedly been either German or American, for example: *Hütte*, *Werkstoffhandbuch*, *Handbook of Chemistry and Physics* and *Metals Handbook*, to name some in this and related fields.

Throughout the present book descriptions of metal production processes, properties and treatments are concise and bear the stamp of wide practical experience. On the other hand, only the briefest references are made to cold-working, recrystallisation, grain growth and X-ray crystallographic methods. The photomicrographs are rather small but clear. Legends are unfortunately on the reverse sides of plates. A number of misstatements have been noted. Thus on p. 114, "Like other ductile and malleable metals, copper has a *body centred lattice*"; p. 39, "When the molten metal enters the ingot mould it is a homogeneous solution of carbon, silicon, manganese, etc., dissolved in *γ-iron*." The theoretical treatment is in general inadequate

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in a book intended for students, but this may not be regarded as a disadvantage by designers requiring a small, strictly practical handbook on metals and some aspects of metal technology.

A. B. WINTERBOTTOM.

CHEMISTRY

Spectroscopy and Combustion Theory. By A. G. GAYDON, D.Sc. Second edition. [Pp. xii + 242, with 4 plates and 13 figures.] (London: Chapman & Hall, Ltd., 1948. 25*s*. net.)

SINCE the publication of the first edition of this work in 1942, advances in our knowledge of the mechanism of combustion have been so considerable as to make a revised and enlarged second edition a welcome event. The plan of the book has remained unchanged, although many sections have been re-written and a new chapter on Continuous Spectra, embodying the results of the author's recent researches on the rôle of atomic oxygen in combustion processes, has been added.

Dr. Gaydon has been very successful in presenting the theory of molecular spectra in a simple form easily understandable by the non-specialist, and the succeeding chapters on emission spectra and the hydrogen, hydrocarbon and carbon monoxide flames then serve as a fitting introduction to the more complex aspects of general combustion theory dealt with later.

One of the important applications of the spectroscopic method to combustion enables the progress of oxidation processes in the gas phase to be followed by observations of the changes in the absorption spectrum; a second application, which is also yielding results of great significance, is the quantitative measurement of the infra-red emission from flames.

The author stresses throughout the need for quantitative rather than qualitative observations and, in the concluding chapter on Kinetic Spectroscopy, discusses the objectives towards which he considers further researches might profitably be directed.

A comprehensive Bibliography and an Appendix giving data for the recognition of spectra encountered in combustion work are included.

D. M. N.

Elastomers and Plastomers: Their Chemistry, Physics and Technology. Vol. III. Edited by R. HOUWINK. Elsevier's Polymer Series. [Pp. 174, with 48 figures.] (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. 25*s*. net.)

THIS volume forms part of a much larger work included as No. 3 in the Polymer Series of the Elsevier Publishing Company. These volumes deal with a large variety of matters concerning both scientific and technological properties of high polymers. The book under review forms the third volume of the set on Elastomers and Plastomers edited by Dr. R. Houwink. The first two volumes are promised in the near future. This last volume deals with the very practical business of testing high polymers. In technology the development and correlation of methods of test is vital for every application of a high polymer. Furthermore, it is the aim of high polymer science to effect an equally vital link between mechanical properties and chemical constitution. The book starts with a general introductory chapter by Dr.

R. Houwink. The second chapter, by Dr. J. H. Teeple, is mainly concerned with the methods of test as a whole. After a preliminary survey of the various standard specifications adopted by the leading manufacturing countries the author not only describes in some detail the outlines of the methods, but also illustrates his list with descriptions of the various types of machine elaborated by the engineering firms specially concerned in their manufacture. In addition, such factors as the proper conditioning of the samples, the optical properties and some of the general chemical properties, such as resistance to weathering and ageing, are also included in the survey. The third chapter, by Dr. A. G. Epprecht, is entitled the Chemical Analysis of Polymers. The attitude here, too, is essentially practical. It is clearly not the line of elucidating the molecular structure of the polymer by the well-established methods, but the provision of a guide to the chemical examination of the high polymer substance, in order that some idea of its build-up may be given. This section might well be called qualitative high polymer analysis, for this is precisely the content of the chapter. It will be particularly useful to the practical user of plastics who may wish, by relatively simple tests, to establish the class to which a high polymeric substance belongs.

In Chapter 4 there is a most useful compilation by Dr. B. B. S. T. Boonstra of the mechanical and electrical properties of what are now conveniently termed elastomers, that is, substances exhibiting rubber-like properties. There follows in Chapter 5 a similar useful tabulation by Dr. J. W. F. van't Wout and Dr. R. Houwink of the mechanical and electrical properties of plastomers; again the tables are of especial interest to the user of high polymers and especially to the designers who have to consider what substances fulfil the requirements specified in engineering and electrical problems.

H. W. MELVILLE.

The Electronic Theory of Organic Chemistry. By M. J. S. DEWAR. [Pp. xii + 324, with 7 figures.] (Oxford: at the Clarendon Press, 1949. 30*s*. net.)

IT is a regrettable fact that, although the English school has contributed such a large share to the development of modern theoretical organic chemistry, so few textbooks or monographs dealing with the subject have been published in this country. For that reason alone, Dr. Dewar's book is a valuable and timely addition to the literature. As Sir Robert Robinson points out in an authoritative foreword, Dewar presents the electronic theory of organic chemistry from a contemporary and somewhat subjective, rather than from an historical, point of view. He assumes the reader to be already familiar with the subject, and the emphasis of his book lies mainly in a critical discussion of the most recent developments. During the period 1935–45, the application of resonance theory to the interpretation of the structure and reactivity of organic compounds scored so many spectacular successes that it tended to overshadow other methods of approach. Within the last few years, however, a similar development has taken place in the application of molecular orbital theory, mainly through the work of Mulliken and Coulson and their respective schools. Dewar adopts this latter approach to the exclusion of the former, arguing that "the resonance method . . . is most unsuitable from the organic chemist's point of view since it involves a new symbolism and a novel and uncongenial outlook. . . . Resonance notation

is cumbersome and tends to obscure the general features of a molecule behind a fog of formulæ. All in all, it is a most unsatisfactory rule-of-thumb substitute for a proper understanding of basic principles." There is no doubt some truth in this criticism, but, in the reviewer's opinion, it severely overstates the case and in some respects applies equally, if not more, to the molecular orbital method and the use of partial valency notation. The fact is that any attempt to replace the mathematical formulæ of wave mechanics by pictorial representations is bound to be only partly successful, and that both the resonance and the molecular orbital approximations have their merits and are to some extent complementary. Dr. Dewar has performed a valuable service by giving the first comprehensive account of a point of view which has been somewhat neglected by other contemporary authors, and which is again becoming of increasing importance. The book is written in a pleasantly easy style, but the incompleteness of the literature references is an unfortunate blemish in a work which will be widely read. The printing and binding conform to the usual high standards of the Oxford University Press.

E. A. BRAUDE.

Fatty Acids and their Derivatives. By A. W. RALSTON. [Pp. x + 986, with 56 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 60*s*. net.)

THE appearance of this excellent work, the second to be published on the subject in a little more than a year, emphasises the remarkable increase in interest in a field which could justly be described a quarter of a century ago as "a neglected chapter in Organic Chemistry." Dr. Ralston's book of almost a thousand pages of most readable information, containing some 5000 references, testifies that this neglect has now been remedied.

The subject-matter divides itself naturally into two sections, the first of which comprises a description of fatty acids, their occurrence, synthesis and physical properties (410 pp.), whilst the second deals with the properties and uses of fatty acid derivatives (530 pp.). The author uses the latter term in its broadest sense, and includes not only the formal acid chlorides, anhydrides, amides, esters, nitriles and salts, but also all compounds obtained from acids as starting materials, such as alcohols, ethers, aldehydes, ketones, mercaptans, sulphides, halides, sulphonates, sulphates and amines. The work may therefore be described as a comprehensive survey, not merely of fatty acids, but of long chain compounds in general.

It is hardly possible to do full justice to Dr. Ralston's book in a short review, but mention must be made of the opening chapters, I, II and III, which, in the writer's view, constitute the best and most detailed account of fatty acids so far written. Every known long chain acid, saturated, unsaturated, hydroxy, keto, cyclic and di-basic is described in some detail, and also included in this section is an excellent summary of Adams' work on bactericidal acids.

A large chapter on physical properties of fatty acids gives an adequate account of our present knowledge of this subject, and includes, *inter alia*, discussion of alternation, X-ray structure, polymorphism, heats of crystallisation and surface films. There is considerable merit too in the chapter on esters, which again is probably the best connected account of the subject. The large section of this chapter, devoted to the synthesis and polymorphism

of glycerides, is without reproach except for the "revised" nomenclature for the various polymorphs, adopted by certain American workers, which, unfortunately, can hardly be said to clarify the subject. The reviewer, whose work suffers by this "revision," may be allowed to hope that his American friends will see the light in time for Dr. Ralston's second edition.

Sufficient will now have been said to indicate the wide scope and the high quality of this work, which in spite of the vast literature under review (up to early 1947) is skilfully prevented from becoming a mere compilation. It should be of inestimable value to all chemists, and to the specialist, in particular, it points the way to many gaps in the subject still to be filled.

T. MALKIN.

The Systematic Identification of Organic Compounds. By R. L. SHRINER and R. C. FUSON. Third edition. [Pp. x + 370.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 13*s*. 6*d*. net.)

THE production of a third and enlarged edition of this book is an indication of its established value and of its consequent demand. The chief change which has now been made is that Chapter VI on "Solubility Classes" has been entirely rewritten. The book contains a vast amount of practical information that can be read with interest and profit by any organic chemist, and the extensive tables of Physical Constants will be of great value to students. The one point in the system of classification the successful application of which the reviewer would query is the very chapter which has been rewritten, namely that on "Solubility Classes." In this chapter, the student is instructed to allocate an unknown compound to one of nine classes (p. 59), dependent upon its solubility—or lack of solubility—in various solvents and solutions. The reviewer, from long experience of teaching practical organic chemistry, would have thought that many of the less able (and less careful) students would have got very confused in this attempted allocation, and that, moreover, the fact that there cannot be a sharp line of demarcation between some of these classes would confuse some of the more able students also. The fact that two Classes are described as "Neutral Compounds" and a third as "Miscellaneous Neutral Compounds" indicates the extent to which these Classes must merge into one another.

One minor point: the authors give some "Specimen Reports on Unknowns" to illustrate how a student should write up and present his results. On p. 12, the unknown compound is α-Picoline, and in the third test (elementary analysis) the compound is stated to contain no nitrogen. It is not clear whether this is a slip on the part of the authors or whether it is intended to represent the type of mistake a student might make.

F. G. M.

Reagents for Qualitative Inorganic Analysis. Editors, P. E. WENGER and R. DUCKERT. Joint-Authors, C. J. VAN NIEUWENBURG, J. GILLIS. [Pp. xxii + 379, with 57 micro-photographs.] (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. 39*s*. 6*d*. net.)

THIS publication is the second report of the International Committee on New Analytical Reactions and Reagents of the International Union of Chemistry. It is concerned with reagents for qualitative inorganic analysis

introduced between 1937 and 1943. Description is limited to the reagents which have been found definitely reliable.

The first part of the book deals with reactions for cations, and the second part with those for anions. Not more than five new reactions are given for each substance, but these have been carefully chosen. Each test is described graphically. First the bibliography of the test is given, then, where possible, the mechanisms of the reactions involved. Then follow details of carrying out the test, including the type of apparatus required. Next the sensitivity and the selectivity are described, special emphasis being given to interfering ions, and where possible how the interference may be overcome. Finally the solutions required for the test are given. The book also contains very useful tables for the qualitative separations of elements in the various groups prior to carrying out the specific tests.

The treatise is extremely comprehensive, and describes tests for all the rare elements, as well as those commonly encountered. It is recommended as an extremely useful reference work.

R. F. M.

Organic Reagents used in Gravimetric and Volumetric Analysis. By JOHN F. FLAGG, Ph.D. [Pp. xiv + 300, with 11 figures.] (New York and London: Interscience Publishers, Inc., 1948. 36*s*. net.)

THIS book is Volume IV in the excellent Series on Chemical Analysis issued by Interscience Publishers. It can be truthfully stated that it maintains the excellent standard so far achieved by the previous volumes, in scope of subject, presentation of material and care in preparation, and will doubtless become the standard textbook on the subject.

Organic reagents have during the past twenty years found increasing use in analytical chemistry, and there is no gainsaying their popularity and usefulness. Every day new organic compounds are being prepared and their reactions with inorganic ions studied with a view to achieving more specific precipitation and more sensitive detection. The majority of reagents in use are concerned with colour reactions and spot precipitation reactions, which form the subject-matter of several treatises, but a wealth of information is now available on the use of organic reagents in gravimetric and volumetric analysis. This material is most adequately collected and arranged in the present volume, which therefore fulfils a topical need.

The first part of the book deals with the theoretical aspects of the subject, thus allowing of emphasis on the fundamental similarities of the various reagents. The first chapter is devoted to the organic chemistry of the reagents, special note being made of the reactive groups involved and the effect of other groupings, and rather general conclusions are drawn as to which metals are likely to be precipitated. An attempt also is made to explain adsorption precipitation from the standpoint of colloidal physical chemistry. The second chapter deals with the properties of organometallic precipitates, such qualities as colour, solubility, stability and structure being studied at some length. Very useful information relating to the configuration of choleate complexes is incorporated in this chapter. Separation by means of organic reagents forms the basis for a further chapter, and this subject is treated with much detail. Separation by solvent extraction and by precipitation are separately described and the solubility product data developed under varying conditions of *p*H, etc. A fundamental study of

the solubility problem and the theory of solubility of salts with a common ion is given in a well-written chapter, and this first part is completed with a description of the techniques to be followed in analysis, using organic reagents. This should be of great assistance to workers carrying out developmental investigations into this field.

The second part of the book is called "Special Reagents" and deals with the actual techniques to be used. Firstly, a reagent is described, then the conditions and substances which precipitate with it are listed, and the individual estimations for inorganic ions are fully expounded. This practical portion of the book should have wide appeal to all analysts, since it covers fields from metallurgy to biochemistry. The techniques described are minutely detailed, and the substances and conditions which effect accurate results are given. Occasionally one notes omissions—*e.g.* the marked effect of chloride ions and solvents such as acetone on the solubility of benzidine sulphate precipitates, and the solubility effect of compounds such as urea on the tannates are not emphasised. But these are small things in a book which covers so vast a field and which illustrates the immense command the author has of his subject. R. F. M.

Qualitative and Volumetric Analysis. By J. C. Giblin, B.Sc., A.R.I.C. [Pp. xiii + 175.] (London, New York, Toronto: Longmans, Green & Co., 1948. 6*s*.)

This volume is a new addition to the wide range of textbooks now available for use by Intermediate and University Scholarship candidates. It presents in a single small volume most of the material needed in textbook form by a student of elementary analytical chemistry, but it clearly presupposes adequate lectures on the general chemical background of analytical methods.

The first part of the book outlines the usual separations employed in inorganic qualitative analysis; the separation tables are first given in a simplified form, suitable for use when separations within a group are not needed, and full group separations, largely by conventional methods, are introduced afterwards. The treatment is generally sound, but there is a regrettable tendency to over-generalisation; on p. 9, for example, it is implied that aluminium sulphide is insoluble in alkaline aqueous solutions. The theoretical account of the group separations (pp. 45–51) is most unconvincing; the broad principles of the Ionic Theory are by no means clearly applied, and misleading statements appear (*e.g.* p. 49, "ammonium sulphide . . . (like all salts) *is highly ionised*"). The hydrated oxides of metals such as aluminium and chromium are incorrectly termed "hydroxides" and formulated as such.

The use of organic reagents for detection of both metals and acid radicals receives special attention, although such reagents are rightly confined to confirmatory tests applied in the absence of interfering ions.

The sections on volumetric analysis are conventional in method and scope, methods of setting-out and calculating results being clearly illustrated. Loose expressions are frequent in this part of the book; the term "strength" of a solution, always to be avoided, is unforgivable when it is applied to a solution of an acid (p. 92). The statement that potassium permanganate solution "slowly loses oxygen" (p. 105) is misleading, and the report that a solution was "reduced with nascent hydrogen" is rather bold. The further

assertion that hydrogen peroxide and potassium permanganate solution "mutually reduce each other" will rightly puzzle any student, and the instruction that there is "*no need* to heat" a hydrogen peroxide solution before titration is scarcely adequate (p. 115). These examples indicate lack of attention to detail in compiling the book, which is unfortunately accentuated by poor editing; thus the spelling "radicles" is used instead of the generally accepted "radicals."

This book merely adds one more title to the many existing works of similar standard, and it has no special feature to justify its production. Such a new book will scarcely displace established works when a casual inspection reveals disquieting misstatements and inconsistencies, even if these are more evident in detail than in broad principle.

A. J. E. WELCH.

The Modern Metallurgy of Alloys. By R. H. HARRINGTON. [Pp. xii + 209, with 49 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall Ltd., 1948. 21*s*. net.)

"IF Shakespeare were alive today, with metallurgy as a hobby, he might well paraphrase the title of this chapter as 'Much Ado about Nothing, or Close to It.'" Thus begins Chapter 8 of the book under review and the quotation contains an expression of what the reviewer thinks about the whole book. In his Introduction the author writes: "Few references to the literature will be made, for the facts therein have been described hundreds of times. Few, if any, photomicrographs will be used, for hundreds have been printed and common structures are engraved on each reader's mind. A definite attempt will be made to eliminate purely theoretical considerations, at least as far as minutiæ are concerned, and to eliminate, so far as possible, confusing definitions."

One begins to wonder what there is left to do, but the author tells us this in his Preface: "For this purpose it has been necessary to formulate simple standard definitions for heat treatments, to relate these heat treatments to equilibrium diagrams, to logically classify and pictorially name the types of equilibrium systems and solid state reactions, to free the Phase Rule from man's inhibitions, to try to understand why metal elements alloy as they do, to supply in convenient form the tools for the reader's own mental engineering, and, finally, to tie together a few loose ends." He might have added that he invents a "Metallurgist's Periodic System," which he says (p. 129) "is really a tremendous blueprint of the structure of the material world. Here lie the keys to the mansions of many sciences. Yet, strangely enough, there is here no clue to the mystery of Life itself."

Many passages (especially on pp. 131 and 156) seem almost juvenile, but it is very difficult to judge for what class of reader the book was intended. The author himself gives a clue, however; the final quotation is from his Preface: "This book is purely a selfish one, written chiefly for the author's use!"

E. S. HEDGES.

Waste-Heat Recovery from Industrial Furnaces. Institute of Fuel. [Pp. x + 384, with 112 figures.] (London: Chapman & Hall, Ltd., 1948. 35*s*. net.)

THIS is a collection of eight papers presented to the Institute of Fuel on waste-heat recovery. The first deals with the calculation of the recoverable

and returnable heat from flue gases. The only data required are the net calorific value of the fuel and the excess air. The calculation depends on the chance fact that the net heat content of a cubic foot at N.T.P. of flue gas is 100 B.Th.U. irrespective of whether the fuel is carbon, hydrogen or mixtures of them both. Confusion is introduced by using the letter *n* for both N.T.P. and excess air factor.

The second chapter is on simplified heat transfer data. Here simplicity takes the form of omitting references to formulæ. In heat transfer calculations the correct use of a formula can sometimes only be determined by reference to the original experimental conditions, especially in the field of radiant heat transfer where finality is far from being attained.

Details of an experimental investigation of a regenerator with various brick fillings extend the German work in this field and should prove a valuable source of information to designers.

Tubular metal recuperators have sometimes to be operated with the fluids in parallel flow to avoid excessive metal temperatures, but there are indications that modern metallurgy is facing up to the severe conditions of operation. A useful discussion on finned tubes is given in this chapter.

Waste heat boilers are discussed from the practical aspect and the three final chapters are devoted to the recovery of waste heat in the carbonising, metallurgical and glass container industries, the last being of a general character and worthy of being read by those outside the industry. These draw attention to the difficulty of preheating the air because it does not intercept radiant heat. They suggest, as an alternative to boilers, the use of gas turbines to extract power.

A useful table of the properties of gases over a wide range of temperature includes Prandtl numbers as well as the characteristic section of the Grashof number.

One result of the increase in price and decrease in amount of coal available is that serious attempts are now being made to ensure that our national heritage is utilised to its full advantage.

The price of the book is very high compared with similar Government publications.

M. B. Donald.

GEOLOGY

Road Aggregates: Their Uses and Testing. By Bernard H. Knight, D.Sc., Ph.D., M.Inst.C.E., and Rena G. Knight, M.A., M.Sc., D.I.C. Second edition. The Roadmakers' Library, Vol. 3. [Pp. xii + 259, with 95 figures.] (London: Edward Arnold & Co., 1948. 25*s*. net.)

This book has been largely re-written in the light of the progress made in the testing of road aggregates during the last decade.

The earlier chapters dealing with classification, mode of origin, and petrological characteristics of the common rock types remain much the same as in the original edition. The tables, dealing respectively with the classification and mineral composition of the igneous rocks, could with advantage have been combined and certain of the more highly technical terms unfamiliar to readers who are not trained petrologists might have been omitted.

The chapter on Petrology in relation to the quality of roadstone is based mainly on the senior author's extensive researches.

The determination of average grain size, percentage decomposition, and degree of fissuring involves a technique which is not readily adaptable to the ordinary testing laboratory, whilst the evaluation of the results presents a problem not easy of resolution even by a skilled petrologist. Moreover, deficiencies in these respects would be revealed under the ordinary standard tests. For these reasons, petrological methods have not been "standardised." In describing typical roadstones it would have been useful to indicate their behaviour under the standard tests.

The chapter dealing with The Physical and Chemical Testing of Road Aggregates has been much expanded, but it would have been rather less confusing to the student if the British Standard Tests had been grouped together at the beginning, followed by the notes on the foreign methods. No table has been given to show the interpretation of the physical test data nor has any attempt been made to relate such data to the petrological characteristics of natural stones.

The new chapters describing and critically examining other Standard Specifications and unstandardised methods of testing constitute a most valuable addition to this work. The remaining chapters on Mechanical Analysis and Concrete Mixes have been completely revised in line with modern developments. EDGAR MORTON.

BOTANY AND FORESTRY

Vernalization and Photoperiodism. A Symposium. By A. E. MURNEEK, R. O. WHYTE, *et al.* Lotsya, Vol. 1. [Pp. xvi + 196, with 12 plates and numerous figures.] (Waltham, Mass.: The Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1948. $4.50.)

THE last two decades have been remarkable for a renewed interest in problems of morphogenesis. After a period of rapid advance in the direction of analysis of the primary physiological functions, attention has turned once more towards the question of integration of function issuing in the behaviour of the plant as a whole, and its adjustment to the environment; this, in a sense, marks a return to the pioneer work of Sachs and the experimental morphologists of the last century, but with the difference that the rise of biochemistry and enzymology have in the meantime furnished new weapons for the attack. The new impetus arose from two major discoveries: the work of Gassner in 1918 on the after-effects of temperature during germination on the flowering behaviour of winter cereals, and of Garner and Allard in 1920 on the control of flowering by length of day; these have led to the conjoint problems of vernalisation and photoperiodism. Many workers were attracted to the new fields of study and a vast literature has appeared setting out a wealth of empirical data. The symposium is intended to meet this situation and to supply an authoritative account of the present state of knowledge. Unfortunately delay in publication owing to the war has somewhat detracted from the desired end; nevertheless, the book is a valuable introduction to the subject, and is amply documented to assist readers in further study and to acquaint researchers in this field with previous work.

The symposium opens with historical surveys of vernalisation and photoperiodism by Whyte and Murneek. The work on vernalisation is dealt with regionally and the contributions from U.S.S.R., Germany, U.S.A., India and Great Britain are severally discussed. The technique of vernalisation, as worked out by Lysenko, is described and current views on the value of the

technique in agriculture are fairly stated. The research carried out in this country on the elucidation of the physiological processes accompanying vernalisation is described in detail, but, perhaps wisely, the theoretical aspects are not unduly stressed. The only serious omission is the work of Melchers and Lang on the vernalisation of biennials, but this is referred to in a later contribution by Hamner. The historical survey of photoperiodism is supplied by Murneek and discusses most of the salient features of the problem, including a summary account of theories on the mechanism of the length of day effect.

After this introduction to the main issues later papers deal with particular aspects of the subjects. Hamner discusses the mechanism underlying both vernalisation and photoperiodism and presents evidence of its hormonal nature derived from transmission and grafting experiments. Borthwick, Parker and Hendricks contribute an interesting paper on the photochemistry of the photoperiodic effect, stressing the similarity of the " action spectrum " of flower initiation in long and short day plants to that in leaf and internode growth of etiolated seedlings. A new hypothesis is suggested, dependent upon a single hormonal substance which is formed in the leaves and destroyed in light ; the specific reactions of long and short day plants are attributed to suboptimal and supraoptimal concentrations, the former promoting and the latter inhibiting flower initiation. The complexity of the relations to wavelength of light is discussed by Funke. Murneek deals with the nutritional aspects of photoperiodism and appears to dispose of the assumed importance of the carbohydrate/nitrogen ratio in flower initiation ; these changes in metabolic balance result from flower initiation and not vice versa. Roberts and Struckmeyer have contributed a fascinating paper dealing with visible anatomical changes preceding flower initiation ; evidence, based upon 70 species of dicotyledons, is presented for a reduction in cambial activity accompanied by " maturation changes " in phlœm and xylem, and such changes are also found before flower initiation due to grafting, " temperature girdles," and compression of the phlœm. Allard has contributed a speculative paper on the rôle of length of day in past geological epochs and its relation to distribution of plants in fossil floras. The bearing of photoperiodism and vernalisation on behaviour of crop plants in India is discussed by Sircar, and the value of phenological data in determining daylength and temperature requirements of crop plants is raised by Nuttonson. Finally a valuable contribution by Went discusses more fully the importance of temperature and the part played by diurnal variation of day and night temperature, to which effect the name thermoperiodicity is given.

Supplements in German by Bünning deal with photoperiodism in the tropics, and present a very welcome summary of his views on endogenous diurnal rhythms in plants in relation to a theory of photoperiodism. Lang has provided a further supplement on the genetics of photoperiodism and vernalisation. The symposium is introduced in a foreword by Thimann, who from the viewpoint of an authority on plant hormones discusses the present position and future prospects in the physiology of flowering.

Where much is admirable it may be ungracious to point out a serious error on p. 24 in discussing the work of Hatcher ; the statement on auxin contents of the rye grain should read " *No* auxin is detectable in the embryo . . . etc.," which completely reverses the sense of the statement and resolves a flagrant contradiction in the text.

F. G. GREGORY.

The Economy of Timber in Building. By R. G. BATESON, M.A. [Pp. xii + 96, with 11 figures.] (London: Crosby, Lockwood & Sons, Ltd., 1948. 7*s.* 6*d.* net.)

THE author is to be congratulated on producing a book that is technical and instructive, yet is still interesting to both layman and professional. He has combined the knowledge and experience gained at the Forest Products Research Laboratory with a great understanding of the practical aspects of various problems; his summary of the present and future need for economy of timber is concise but embracing.

The few faults that can be found depend to a large extent on personal opinions, which can always be at variance. For instance, the term "stressed skin" is easily accepted, but can be applied strictly only to those constructions where the stress in the "skin" approaches the normal working stress. Stress grading is said to have no great part to play in ordinary house construction, but even the half-way measures proposed in the Ministry of Works Timber Economy Memorandum show great savings in the timber required in roofs and floors.

The author obviously appreciates the value of preservation, but presents his case in a haphazard way. Having discussed the decay hazard and the possibility of attack by wood-destroying insects, and having stated that "by proper preservative treatment, all timbers can be made resistant to rotting fungi so that they will last almost indefinitely in damp conditions," he then limits the places where the preservative treatment is necessary by suggesting that pressure treatment should be employed for sub-floor construction and flooring. Adequate preservative treatment should also be given to items such as door frames, window frames, sills, and any other timber in contact with brickwork or masonry, and even to such items as windows, with their small dimensioned timber and multiplicity of joints.

In discussing the economical use of timber in carcassing, he makes his first practical mistake. Gable-ended roofs require all one length of rafter, which would be unobtainable if large numbers of houses were concerned. In the hipped roof, varying lengths are utilised, and in practice probably saves more than wastes more, as suggested by the author.

His analysis of the uses of timber in joinery and decorative work is extremely thorough, and his interesting exposition on the "one standard" house is excellent, but I consider it is far too short.

Timber substitutes are discussed, but their use depends as much on reactions of the inhabitants of the building, particularly with reference to coldness, slipperiness, noise, etc., as on the mechanical properties of the substitutes. His attitude towards substitutes is aptly summarised by his quotation: "Like the little girl in the rhyme, when they are good, they are very very good, and when they are bad they are horrid!"

The final chapter on the future use of timber in building may be startling to many familiar only with the traditional style. To those acquainted with the great discoveries and developments of glues during and since the war, however, this forecast is just common sense.

The final paragraph should be taken to heart by all those connected with timber and its uses—"If timber is to conform to modern conceptions of efficiency and precision, we must learn to use it in the best possible way, and this we cannot do without according to it the same care and attention that we devote to other materials."

E. H. N.

BIOLOGY

Laboratory Guide in Animal Biology. By R. H. WOLCOTT and E. F. POWELL. Second edition. [Pp. x + 113, with 1 figure.] (New York and London: McGraw-Hill Book Co., Inc., 1948. 9*s*. net.)

To anyone who has experience of practical biological teaching in this country, it is interesting to learn how such work is approached in America. It is inevitable that the content of such courses will differ from ours, though there are many points of contact, since in the "type method" certain animals are always favoured. It is also comforting to note that it is emphasised that structural knowledge of animals "should be gained in the laboratory," and that observations on living animals are encouraged.

The first thing which strikes one about this book is the complete absence—apart from a diagram of the microscope—of figures. Surely a beginner in practical zoology will be saved much time if he knows what the object he is seeking looks like. On p. 67 the student is directed to remove the dorsal walls of the median vesiculæ seminales of the earthworm and wash out the contents. Then "with a hand lens observe a pair of testes attached to the anterior walls of each." Without some further description or a diagram it is doubtful whether the student will really see the testes. Similarly with mitosis (pp. 19–20) much more could have been said of what is actually discernible.

The definite directions for drawings are welcomed, though again more exact details of what should be drawn and particularly what should be labelled would be useful.

On the whole the book is interesting, but not likely to be of great use in this country, owing to the differences in content and emphasis in the course. With so much useful instruction given it is unfortunate that, when examining *Hydra* (p. 32), the student should be simply told: "Place the watch-glass on the stage of the microscope." Anyone accustomed to working with the microscope knows that if this is done the watch-glass will simply lodge in the aperture in the stage and its movement be restricted. If, however, the watch-glass is placed on a thin micro-slide then it can be moved freely and the whole of its contents examined.

A. J. G.

Animals Alive. By AUSTIN H. CLARK, F.R.G.S. [Pp. viii + 472, with 26 pages of figures.] (New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1948. 22*s*. net.)

THE author sets out to survey the whole of the animal world and has produced a book that is a joy to read. More than two thousand animals or types of animal are named, and something is said of each one to indicate its position in the animal kingdom, its habitat and mode of life, its relations with other animals, and its interest to man. It is delightful to find a work of this kind, directed at the general reader, written with masterly clarity in a prose free of slang and jargon, and untainted by the "pathetic fallacy." The book is planned in four sections: Man and the Animal World, Land Animals, Fresh-water Animals, and Sea Life. The only doubt that enters the reviewer's mind is that the abundance of forms of life mentioned might stun those readers who had not already some idea of the general plan of the animal kingdom; it is perhaps the zoologist's idea of the right sort of book for the

general reader. Certainly the illustrations, line blocks, are of the museum catalogue type.

But the zoologist, anyway, will find it full of new information; that is, if he did not already know such things as the story of a tropical American bot-fly, a fly which catches a female mosquito and attaches eggs to its underside. The bot-fly's larvæ are dropped into the puncture made when the mosquito bites a human being and live inside the swelling caused, until pupation. And of the giant squids, such as one taken off the Irish coast whose eyes were 15 inches in diameter.

The book is beautifully printed and bound, and the index gives it the final stamp of quality. Every animal mentioned is listed by its popular name or, failing a popular name, by a brief descriptive phrase. This is followed by its generic name as, for example: "Shark, with saw-like snout (*Pristiophorus*)."

This volume is one of a series on Natural History, to be written by scientists associated with the Smithsonian Institution; it is excellent value for its price.

W. H.

The Lungfish and the Unicorn. By WILLY LEY. [Pp. 254, with 20 figures.] (London: Hutchinson's Scientific and Technical Publications, 1948. 10*s.* 6*d.* net.)

THE author has made of this "excursion into romantic Zoology," as he describes it, a fascinating and well-written book which will be enjoyed by the general reader, and read with profit by the serious student of Zoology and Palæontology.

In his first part he discusses such mythical creatures as the Unicorn, Dragons, Giants, Sea Serpents and the like, approaching the study of them in an unbiassed scientific spirit. He takes the large volume of information he has collected from records and descriptions scattered through all kinds of literature, and examines it in the light of known zoological facts. As the outcome of this study he has been able to make interesting and, for the most part, reasonable suggestions as to the phenomena which may lie at the back of these myths.

The author next goes on to the consideration of animals that have become extinct during man's time on earth, such as the Dodo, Urus, Ground Sloth. By a careful study of records from many sources, both historic and scientific, he provides material for producing a picture of the late stages leading up to extinction, and of the conditions which brought this about. Especially interesting is his account of the recreation of the extinct Urus by breeding from cattle which had strains of the Urus in their heritage.

In the final part he deals with living "witnesses of the Past," so-called living fossils, primitive forms belonging to main stocks that have otherwise been extinct for ages. These include Limulus, Latimeria, Ceratodus among aquatic forms; Hatteria, Platypus, Okapi and a number of other terrestrial forms. He gives interesting accounts of the circumstances which led to their discovery and of the conditions which have resulted in their survival.

Despite the title of his book the author has produced a valuable contribution to zoological and palæontological literature, a contribution which every student of these subjects should welcome.

H. H. S.

Introduction to Genetics and Cytogenetics. By H. P. RILEY, [Pp. xii + 596, with 154 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 30s. net.)

IN his preface Dr. Riley states that he has endeavoured to explain the basic principles of biological inheritance and to present the material sufficiently simply for it to be grasped even by a reader of limited biological training. It is this latter aim that sets the keynote of the whole work.

The book has much to commend it. Its thirty chapters cover nearly every topic with which genetics has concerned itself up to the present. Sometimes the topic is given little more than a mention, even where it may be one of central interest to-day. Such are Beadle's work on biosynthetic processes in *Neurospora*, and the modern work on cytoplasmic inheritance. Indeed *Paramecium*, with all its important consequences for the understanding of cytoplasmic behaviour, is not even listed in the index. It should be stated, however, that whatever its deficiencies in this respect may be, Riley's book is much better balanced than others published in the recent past. His coverage, especially of cyto-genetic material, is more praiseworthy than most. He has even introduced a short discussion of probability in relation to testing ratios, and of statistical methods as they bear on quantitative inheritance. We may note, too, that the diagrams are well above the average both in design and execution.

The real weakness of the book arises from the author's avowed aim of making it self-contained. Much is included which is not essential to a genetical treatise. We find, for example, an account of plant life cycles which, even though brief, is surely better left to more purely botanical texts. Also the treatment of essential genetical ideas is so prolonged as to be tediously diffuse; and even then, as with the discussion of allelomorphism and homology, it often fails to make the principles at issue fully clear. In fact, one is left with the feeling that Riley has spent so much time covering all the ground, that he has had none left for getting fully clear the logical structure of his subject.

In brief, the book fills many of the students' needs, but it is so long that the demands it makes on reading time and, perhaps more important still, on the pocket, may well be beyond the resources of most students.

K. MATHER.

Dictionary of Genetics. Compiled by R. L. KNIGHT, D.Sc., Ph.D. Lotsya, Vol. 2. [Pp. xii + 183.] (Waltham, Mass.: The Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1948. $4.50.)

WHETHER a Dictionary of Genetics is at present required is a debatable question. So far as the living terminology of genetics is concerned, it would scarcely fill even a very slim volume. To disinter the bodies of dead words, as the author has done, involves the obvious danger that they might get back into circulation again; however, the author hopes that it "will help the coiners of new words to avoid putting an entirely new meaning on an old-established word." Even with the inclusion of all the dead wood, the volume still has much padding from botany, animal breeding and evolution, together with nine somewhat mixed appendices, some of them useful.

Any new dictionary will contain errors and omissions, and the volume

under review is no exception to this rule. Nonetheless, some shortcomings are too serious to be glossed over like that. To describe the "Homotypic Division" as "The second, or non-reductional, meiotic division" just will not do in 1948. "Discontinuous Variation.—A mutation, or sudden heritable variation," fails to mention discontinuous variations due to environmental conditions. Lethal factors are not all recessive, as might appear from the author's definition. "Inbred Line.—A line showing a considerable degree of homozygosity as a result of continued inbreeding *and selection*" (italics mine) contains a statement which is not only irrelevant but positively misleading, as selection may well undo the effect of inbreeding. Many definitions are one-sidedly botanical. Thus the author's definitions of Clone and of Polyembryony omit to mention animals altogether and that of Incompatibility fails to mention the important human cases of the Rh type. The definition of hypophysis is completely inadequate, nor is it excusable to credit only mammals with the possession of penis and vagina. This list could easily be extended. However, to compile a dictionary for the first time is a difficult task, and the book may ultimately grow out of these shortcomings.

H. G.

PHYSIOLOGY, BIOCHEMISTRY AND BACTERIOLOGY

Vision and the Eye. By M. H. PIRENNE, Dr. Sc. (Liége), Ph.D.(Cantab.). [Pp. xx + 187, with 4 plates and 99 figures.] (London: The Pilot Press, Ltd., 1948. 12*s*. 6*d*. net.)

IT is a pleasure to read a scientific book, where, owing to the author's intimate knowledge of his subject, it is difficult to find a superfluous word. It reminds one of a Phil May drawing which the artist is said to have constructed by making innumerable lines and then by pruning leaving only those that were absolutely necessary.

Vision is a very wide field of study and the literature huge. Yet here in a small book of 187 pages Dr. Pirenne has concentrated matter which might easily have been expanded into a good-sized volume.

The work starts with a description of the eye and formation of the retinal image and passes on to the structure of the retina, properties of the rods and cones, spectral sensitivity curves and the nervous activity produced in the eye by the action of light. There is a very clear exposition of the quantum theory.

From an account of the eyes of insects the author goes on to assess the visual acuity of man. The controversial problem of colour vision is dealt with clearly and impartially. The book ends with an account of the relation of paired eyes with the brain and of physics with the phenomena of life.

As Dr. W. S. Stiles says in his foreword: "Two features of the exposition appeal to me strongly. These are, firstly, the very clear formulation, wherever possible in quantitative terms, of the basic notions essential to a proper understanding of the working of the eye, and, secondly, the avoidance of arguments founded on introspective descriptions of sensations, which are notoriously difficult to interpret correctly. Although addressed primarily to the non-specialist interested in vision, Dr. Pirenne's book will also prove, I believe, a valuable introduction for those commencing research on the subject."

EUGENE WOLFF.

Comparative Physiology. By BRADLEY T. SCHEER, Ph.D. [Pp. x + 563, with 72 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 36*s*. net.)

THE English-speaking teacher and student of Zoology have been waiting for years for an adequate textbook of comparative physiology; that is, for a work which surveys what is known of the whole animal kingdom and relates it to the most substantial corpus of physiological knowledge, that which refers to the Mammals. In a sense they will always wait in vain, for the subject is, of course, so vast that no single volume could contain even an outline of its subject matter. But what we can hope for is a general sketch of the problems accompanied by a full and up-to-date bibliography. So far as the reviewer knows, Scheer's book has no serious competitors, but it can be recommended not only for this reason but also because it does fulfil, to a very useful extent, the requirement named above.

It is easy to find fault with a book of this kind. There are many omissions in the bibliography; to take two at random, there is nothing of the important work done since 1934 on the Oligochaet excretory system, nor of the advances made in the same period in our knowledge of the functioning of the acoustico-lateralis system of fishes. The index is incomplete: it does not, for example, contain the word "adrenal," nor "suprarenal," nor "ear," nor "cochlea." The author, who emphasises the importance of the "comparative approach," founded on the principles of comparative anatomy, gets himself into a ludicrous muddle over the morphology of the vertebrate kidney (pp. 443-4) and has a figure of the "nephridium" of a crayfish (p. 273). He is sadly out of date in his knowledge of moulting hormones in insects, perhaps because his reading of the British literature is patchy.

The author subdivides his book into chapters dealing with individual phyla or groups of phyla. Consequently physiological topics are repeated many times; thus a reader wishing to find out about respiratory pigments would have to refer to every chapter in which he expected to find animals containing such pigments, and he would never be given a general view of the problem as a whole. The difficulties of this method of arrangement are particularly noticeable when the subject of Animal Behaviour is being considered; the treatment phylum by phylum is almost useless here, and this branch of physiology is perhaps least well dealt with of all.

With all its defects this is a book well worth having. Any teacher or undergraduate who has not already got a full card-index of the literature of comparative physiology will be greatly helped by its references, and younger students will be given a reasonable summary of a vast subject.

W. H.

Practical Methods in Biochemistry. By FREDERICK C. KOCH and MARTIN E. HANKE. Fifth edition. [Pp. x + 419, with 22 figures.] (Baltimore: The Williams & Wilkins Co.; London: Ballière, Tindall & Cox, 1948. 16*s*. 6*d*. net.)

THIS book is primarily written for medical students, and it should be read in conjunction with a textbook on Physiology. It is, in effect, a very comprehensive biochemical laboratory manual. The first part of the book is concerned with the characteristic tests of the various cell constituents. It is an improvement on the usual book of this type, since the qualitative reactions which it so fully describes are treated more from a quantitative

point of view, so that the elementary student will be better fitted to carry out the micro-estimations which are described in the latter part of the book.

The methods given for blood and urine analyses have been carefully chosen, so as to illustrate modern technique, and are described lucidly, but with all essential detail. Very useful sections on manometric methods of analysis, colorimetric and fluorometric tests for vitamins, and new and revised colour methods for hormones are included.

There is a section also of micro-biological methods for vitamins and amino-acids which should give the student an insight into this rapidly expanding analytical tool.

Finally, there is an appendix which describes standard laboratory technique and apparatus, and gives lucid instructions for the preparation of standard solutions, and the specific reagents used in the estimations described.

The book is essentially American, and the references are almost entirely so.

R. F. M.

The Biochemical Reactions of Chemical Warfare Agents. Edited by R. T. WILLIAMS. Biochemical Society Symposia, No. 2. [Pp. vi + 73, with 9 figures.] (Cambridge: at the University Press, 1948. 5*s*. net.)

IN 1939 chemical warfare was taken very seriously in this country, and consequently at the outbreak of hostilities an elaborate organisation was brought into being to consider the problems involved from all aspects. The biochemical work was largely carried out by extra-mural workers in the departments of the various Universities and Colleges, where the workers had very wide scope for research within the framework of their particular subjects. Co-ordination was maintained by frequent interchange of reports, by formal and informal meetings, and discussions arising therefrom, in which Prof. R. A. Peters and Dr. Malcolm Dixon played leading parts.

The Biochemical Reactions of Chemical Warfare Agents is a publication following a Biochemical Society symposium on the subject. It is capably edited by R. T. Williams, who has been a very active worker in the background of Chemical Warfare Research. The symposium represents in effect an extension of those wartime meetings, brought into the open and widened in scope. In point of fact, the publication is a short exposition of work carried out on the subject during the war by the various Biochemical Schools. The output of these workers (and similar organisations in the U.S.A.) from 1939 onwards was prodigious, and in consequence this booklet does no more than summarise the position of the various lines of investigation to date.

In the introductory remarks Dr. Malcolm Dixon outlines the scope of the symposium as emphasising the Biochemical Reactions of the agents rather than the mechanisms by which physiological effects are produced. Special interest is given to reactions with tissue constituents, especially enzymes, since, in common with other poisons, damage to tissues by Chemical Warfare Agents may be largely traced to impairment of action of essential intracellular enzymes.

The first section is on the chemical reactions of Mustard Gas in aqueous solution. The kinetics of hydrolysis are outlined and the possibility of formation of the several hydrolysis products is discussed. The reactivity of Mustard Gas in dilute aqueous solution with most substances or groupings

found in biological systems is emphasised, and the idea of general physiological reactions being due to the formation and circulation of stable sulphonium derivatives is propounded.

A chapter on the reaction of Mustard Gas with proteins follows. Apparently all proteins react, and the bulk of evidence points to the carboxyl grouping being mostly affected. Work with radioactive sulphur has allowed of great strides in this study, and the conclusion is reached that reaction with proteins is not necessarily the primary cause of the systemic effects of this gas.

The exposition on the action of Mustard Gas on enzymes gives but an indication of the immense amount of work carried out on this subject, but the general conclusion is drawn that phosphokinases and some proteases are mostly affected *in vitro*. There is some inconsistency when *in vivo* experiments are made with these enzymes, as the immediate inhibitory effect found *in vitro* is not always observed. This may be due to the formation of modified toxic products, or the formation of some enzyme protection system, but the fact remains that vesication seems to run parallel with ability to inhibit hexokinase. The suggestion is therefore made that this points to loss of capacity to utilise glucose as being an essential part in the vesicant action.

The reaction of arsenicals in living tissues is also discussed, and again inhibition of enzyme action is emphasised. The noticed inhibitory effect of arsenicals (Lewisite) on brain pyruvate-oxidase systems caused the search for antidotes, and from the observation that dithiols fulfil this function the discovery of B.A.L. (2–3 dimercaptopropanol) was made.

Reactions of Lachrymators with enzymes and proteins form the subject of a further chapter. The various likely theories relating to lachrymation are outlined and emphasis is given to the theory that lachrymatory properties are due to rapid reaction with the SH groups in the proteins of the nerve endings, although many unreconcilable facts are against this theory.

Enzymes again come into the picture in the chapter on reaction of Fluorophosphonates with esterases. This group of warfare agents are extremely active physiologically and have an eserine-like action in inhibiting tissue cholinesterases. The substances are having some practical application in the treatment of myasthenia gravis, glaucoma and abdominal distension, and other possible applications are envisaged.

The nitrogen mustards (chlorethylamines) are discussed from the pharmacological viewpoint. These substances cause reactions similar to Mustard Gas, but with less severity, and have certain characteristic reactions also. The leucopenia they produce has led to their use in Hodgkins disease and application to treatment of lymphadenoma sarcoma and bronchial carcinoma has also been tried with some success. The likeness of reactions of vesicants with those of ionising radiations is noted and it is pointed out that comparative study may produce useful information.

The final chapter is a summarisation by Prof. Peters of the history of this work, and how it has led to the linking of Biochemistry and Physiology in attempting to explain certain neurological mechanisms caused by these reagents.

To the discerning reader the immense strides made in the biochemical field by the co-ordinated efforts of the various workers must be apparent. It is significant that for the first time a planned research programme has been carried out embracing all the various specialised techniques and facets. At the same time the individual worker was given wide scope within the frame-

work of his terms of reference, and the successful results achieved cannot be gainsaid. It is suggested that this organisation should be a model on which to base co-ordinated work into the physiology and biochemistry underlying specific diseases. Working as isolated units in watertight compartments, relying on chance impingements on other workers' lines of progress, is wasteful, and it should not need the stimulus of war to bring such workers into closer co-operation.

R. F. M.

Les Diastases. By Paul Fleury and Jean Courtois. Collection Armand Colin, No. 251. [Pp. 216, with 10 figures.] (Paris : Librairie Armand Colin, 1948. Frs. 150.–.)

The size of this book—its pages measure only $4\frac{1}{2} \times 6\frac{1}{2}$ inches—gives it the air of a volume intended for the pocket rather than the library shelf. The contents of *Les Diastases*, however, are to be commended as a sound and balanced summary of modern knowledge and theories of enzymes and their actions. It is a book which would be particularly valuable to the student who wished to acquaint himself with the basic principles, the modern developments and the unsolved problems of enzymology. It is to be regretted, therefore, that, although the studies of many investigators of different nationalities are mentioned in the text, the authors have seen fit to restrict the literature references to 17 French publications.

There are nine chapters. The first and shortest reviews the history of the subject, while the second discusses the occurrence and nature of enzymes. Chapter III has for its subject the purification of enzymes, and the methods discussed include precipitation, adsorption, dialysis and ultra-filtration. Little useful purpose seems to be served, however, in attempting to classify the methods as either those which concentrate or those which eliminate associated impurities. Chapter IV discusses the constitution of the enzymes. This is a complex subject and one which still offers numerous points for debate, but it is very ably dealt with in the limited space available. Chapter V deals with the kinetics of enzyme action and includes some useful data on the influence of the source of the enzyme on the reciprocal of Michaelis and Menten's constant, and also on the manner in which this value is affected by pH. Chapter VI surveys the influence upon enzymic reactions of various physical, physico-chemical and chemical factors, while the specificity of enzymes is the subject of Chapter VII, which contains some interesting illustrative data. Chapter VIII deals with the reversibility of enzyme action and discusses bio-synthesis, and the final chapter summarises the properties of the various enzymes and discusses in greater detail those enzymes which are of physiological and pathological importance or have found a use in industry.

A. J. Amos.

Bacteriology. A Textbook of Microorganisms. By F. W. Tanner and F. W. Tanner, Jr. Fourth edition. [Pp. x + 625, with 137 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1948. 27*s*. net.)

The fourth edition of this book is considerably larger than the third edition of some ten years ago and has undergone some rearrangement. Of American origin and specially written for those studying microbiology for the first

time (in America), it is a useful book of its type. Though called *Bacteriology*, it contains chapters on moulds, yeasts and related organisms, protozoa and viruses, which, with the chapter on systematic relationships of plant and animal groups, clarify the position of the bacteria proper *vis-à-vis* the other groups of unicellular or noncellular organisms.

The chapters which deal with bacteria cover the whole field adequately, dealing not only with morphology, classification, growth and metabolism, but also with the part bacteria play in the soil, air, water and sewage. Industrial fermentations by yeasts, bacteria and moulds get a chapter to themselves, and there are three chapters on milk, food preservation and food-borne infections.

In their preface the authors note that the tendency to over-emphasise pathogenic bacteria in the first course in microbiology is too common; while this may be so, there is in this type of textbook an undoubted tendency to under-emphasise pathogens and the whole field of immunity. This criticism cannot be levelled at this book, for the authors allot six chapters to the relationship of bacteria to disease and the problems of infection and immunity. These chapters deal simply and clearly with these subjects in such a way that the elementary student should get a clear picture of this difficult and complicated field.

Two criticisms can be made. For an elementary textbook, 625 pages is rather large, and it is to be hoped that the next edition will be reduced in size by some careful pruning and re-writing, and by the removal perhaps of the pages of detailed classification, which are surely too advanced for the beginner. The second criticism deals with references: an elementary textbook should lead the beginner to the standard textbooks, to original papers and so to the journals and the whole literature of bacteriology via a few carefully selected references to original papers or chapters in advanced textbooks. While a few references are scattered through the text as footnotes, each chapter ends with a formidable list of textbooks, so that most of the known American textbooks in bacteriology and related subjects must be listed by the last chapter.

A useful appendix lists these textbooks once more and it is to be hoped that the short notes on abstracts and journals in the appendix will be expanded in the next edition. All in all, a useful elementary textbook, which can be recommended.

A. F. B. S.

HISTORY OF SCIENCE

Darwin: Before and After. The Story of Evolution. By ROBERT E. D. CLARK, M.A., Ph.D. [Pp. 192.] (London: The Paternoster Press, 1948. 6*s*. net.)

OMITTING minor issues, this book is in fact a denunciation of evolution and natural selection. The following notes indicate the attitude and views of the author. Darwin and Huxley are ruthlessly disposed of. The doctrine of the fixity of species, we are asked to believe, was *not* accepted in the Middle Ages, and far less was it an article of Christian faith—Darwin in fact had nothing really new to add to the conception of evolution. The idea of the survival of the fittest appealed to the industrialists and the upper classes as a justification of their own grasping practices. The working man was half

starved whilst the coddled child of wealth had food enough and to spare. Nevertheless, according to Darwin, the struggle for existence must not be hindered by ameliorating the condition of the poor. If it was true that Darwin himself did not carry his theories into practice, it was under the struggle system that too many rich men drew large incomes from the white slave traffic, from the exploitation of natives in mines and factories and from selling arms to small nations, after deliberately stirring up war. Darwin, the arch-criminal, was responsible for spreading the immoral and false philosophy of the survival of the fittest, which did as much harm as all the manufacturers of arms, and even inspired Hitler. Every doer of evil could thus justify his crooked ways. But observe, the villain did not flourish. His attempt to escape from the Church and from God, to say nothing of the atrocious crime of dropping Paley, produced, as in Mr. Merdle, a growing realisation of guilt, which reacted disastrously on his visceral system. As his religion faded his illnesses grew worse and worse, the world became cold and dead, and even his reasoning powers were blunted. This quaint diagnosis of Darwin's complaint, by the way, is closely paralleled in a review of the *Origin* published in the *Record* of 1863.

Huxley fares little better. When he returned from the "Rattlesnake" in 1850, we are told "he decided to spend his life doing scientific research, though, in fact, he seems to have been endowed with little ability in this direction." Also that when the *Origin* was published in 1859 he embraced evolution, though not immediately, for two reasons: first that it was an excellent opportunity to vent his spite on the clergy, and second that being at the time "scarcely known in the scientific world" it was a means of self-advertisement by no means to be neglected. As regards the first point, the dispute with the clergy came later, and was in fact conducted by Huxley with sufficient restraint and even with some good humour. And if he was "scarcely known" in 1859 he was at all events elected into the Royal Society in 1851 on the strength of his "Rattlesnake" memoirs, and in the collected edition of his scientific works the papers completed by 1860 fill two large volumes. It is true that never at any time did Huxley support Natural Selection as a sufficient explanation of the method of evolution, but his letter to Darwin dated November 23, 1859, shows that he adopted evolution as a fact after his first reading of the *Origin*.

Dr. Clark accepts, but with reluctance, a strictly limited amount of "external transformation" within the groups, but he emphatically rejects large-scale evolution, and even maintains that no evidence can be produced in support of it. He would do well to consult the recent exhaustive work of Rensch on this difficult aspect of the problem, although in any event he would claim that evolution was only the God-guided unfolding of structures already latent in the organism which involved no real rise in its complexity. In this and many other passages the tragedy of evolution culminates in an anti-climax so confused that the author has to invoke the *Deus ex machina* of Special Creation to restore order in a Darwinised Universe. To doubt the remedy would be impious and even alarming, since the Nemesis of Darwin's complaint darkens the prospects and threatens the labours of the unconverted. Dr. Clark himself, however, is not too consistent in his beliefs. He stoutly resists the highly probable and reasonable contentions of the evolutionists, but swallows without qualms the gene theory of inheritance, which, whatever merits the future may prove it to possess, carries speculation to the very confines of human ingenuity. He reminds us indeed of the dauntless philosopher of

Birmingham, who, rejecting the friendly advice of Gibbon, continued to fire away his double battery against those who believed too little and those who believed too much.

F. J. C.

MISCELLANEOUS

Weather Forecasting. By INSTRUCTOR COMMANDER S. W. C. PACK, R.N., M.Sc. [Pp. 192, with 131 figures, including 8 plates.] (London, New York, Toronto: Longmans, Green & Co., 1948. 25*s.* net.)

WHEN a professional man writes an elementary book about his profession he is almost assured of a good reception; but Instructor Commander Pack has set himself an impossible task. Almost the entire field of current practice in professional weather forecasting, including even the observing instruments, the codes employed in weather messages and the cartographical complexities of weather analysis, is taken under review. While assuming his readers to have no significant knowledge of either mathematics or physics (so finding it necessary to say that "air is extremely light but has definite weight," that if the speed of a front is increasing it is "said to be accelerating," if decreasing it is "said to be retarding"), he somehow contrives to discuss such difficult conceptions as adiabatic changes, frontogenesis and frontolysis, convergence and divergence, kinematical deformation, geostrophic and cyclostrophic winds, even temperature-entropy diagrams; and, in the field of forecasting techniques, we are told of three methods of finding height on a tephigram, of constant pressure charts, of isentropic charts, streamline analysis in tropical regions and zonal indices in connection with long-range forecasting.

The above is but a selection from the comprehensive fare and all presented, dare one say packed, into less than two hundred pages of open letterpress with abundant diagrams. If the attempt could be successful the author would certainly have succeeded, for he goes with the sure judgment of a teacher straight to the root of everything he chooses to handle.

The book is specially addressed to "students who are about to specialise in meteorology"; but it would be for the teacher to decide whether he could supply the flesh to such a skeleton course of technical science: few without modern forecasting experience would be happy in this field.

The book is elegantly printed and the diagrams are excellent, but surely, even in these days, the price is unreasonable. R. C. S.

The Merchant Ship: Design Past and Present. By G. S. BAKER, O.B.E., D.Sc. [Pp. 159, with 16 plates and 17 figures.] (London: Sigma Books, Ltd., 1948. 12*s.* 6*d.* net.)

IN this book Dr. G. S. Baker provides for the general reader, and for the naval architect, two interesting opening chapters dealing with the early development of ship design, and with factors affecting modern ship design. He provides also two interesting concluding chapters dealing with machinery and with ship types and construction.

One point of controversy is raised in the first chapter, by a diagram in which Dr. Baker suggests that there were galleys with 30 banks of oars, a bank being taken to mean a more or less horizontal line. He shows a section of a galley with three banks of oars, and the difficulties of multi-banking are readily apparent. He does not face up to the problem of showing how 30 banks could be arranged. It is possible that there has been mis-translation

of original records, and that in some instances the word commonly taken to mean a horizontal line should have been taken to mean a vertical line. On this basis the 30-bank galley, with two or three horizontal lines of oars, becomes a possibility.

The four chapters in the middle of the book are less satisfactory. In them Dr. Baker deals with technical considerations such as the motion of a ship in a seaway, stability, strength and the problems associated with propulsion. He does not quite succeed in combining general readability with technical accuracy. Indeed, he makes a serious error when dealing with the pitching of a ship in a seaway, when he tries to convince himself, and his readers, that the frequency of the pitching motion is not affected by the length of the ship; had he carefully considered his argument he would have discovered the flaw. The general criticism is that Dr. Baker displays the unquestioning faith of the technologist in his own technology rather than the insistent questioning of the scientist. He stresses the influence of the experiment tank in the solution of problems of design and behaviour. But he does not anywhere suggest that even those responsible for the conducting of experiments are now beginning to doubt the complete reliability of their methods. Nor does he indicate that the manner in which a ship is loaded is a much more important factor in determining her behaviour than any which can be introduced as a consequence of tank experiments.

A. M. R.

Statistical Year-Book of the World Power Conference, No. 4: Data on Resources and Annual Statistics for 1936–1946. Edited by FREDERICK BROWN, B.Sc.(Econ.), F.S.S. [Pp. 212, with 23 tables.] (London: Central Office, World Power Conference, 1948. 45*s*. net.)

UNLESS the fuel and power resources of the world are used as efficiently and economically as is practicable, it will be impossible to maintain and improve the standards of living of the peoples of the world. This involves intelligent planning, which can only be achieved on the basis of reliable information on power resources of all kinds in relation to the many requirements to be met. Until the World Power Conference took the initiative in collecting together the available statistics for different parts of the world, an almost impossible task confronted those authorities and individuals who attempted to collect the data necessary to guide them in reaching decisions on how best to meet fuel and power demands.

The first three statistical year-books issued by the World Power Conference gave information on resources and annual statistics for the years 1933 to 1936. During the recent war, the information could not be collected. In the fourth year-book, the latest information on resources is given, together with annual statistics on the production and utilisation of solid, liquid, and gaseous fuels, and of water power and electricity for the eleven years 1936 to 1946. Separate statistics for each of more than sixty countries are presented, together with such continental and world totals as it has been practicable to calculate from individual items. With only a few exceptions, the data were specially reported by the National Committees of the World Power Conference or by Government Departments of the countries concerned, in conformity with standard definitions established to make possible the compilation of comparable statistics.

For some areas, reliable information on resources, production, and utilisation is not available, and the disturbances caused by the war have rendered even more difficult the work of collection and analysis of the information that could be obtained. It is hoped, however, that the resumption of publication of the year-book will encourage all countries to collect and provide the information in a form suitable for future year-books. The tables include areas and populations, as well as the statistics relating to coals, lignite, peat, coke, wood, petroleum, benzole, alcohol, natural and manufactured gas, water power, and electricity.

The introductory and explanatory texts by the Editor are excellent. Both he and the World Power Conference are to be congratulated in so promptly taking up again the preparation and publication of these valuable year-books.

A. P.

Science Outpost. Edited by JOSEPH NEEDHAM, F.R.S., and DOROTHY NEEDHAM, F.R.S. [Pp. 313, with 26 plates.] (London: The Pilot Press, Ltd., 1948. 25*s*. net.)

CHINA owes a great debt to the British Council for the war-time organisation of the Sino-British Science Co-operation Office which, in addition to making arrangements for exchange of personnel and the transmission of Chinese scientific papers for publication in the West, supplied much-needed books, information and apparatus to the isolated Chinese scientists and technologists. *Science Outpost* is a collection of papers from this office, among which the most interesting and informative are the reports on science and technology in all parts of free China. These are based on the personal observations of Dr. Needham and his colleagues, who undertook a 25,000-mile tour in difficult country to get acquainted with Chinese scientific workers and their working conditions. Many excerpts from travelling journals and letters to Europe (as well as poems, original and translated) not only give vivid glimpses of this remarkable country, but also help to place its science in its true geographical and historical perspective, without which it would be impossible to assess adequately the attainment of the Chinese scientific workers. It is apparent that the resourcefulness and ingenuity of improvisation displayed by these workers, under the most disheartening conditions, greatly impressed Dr. Needham during his tour of the country.

Dr. Needham is of the opinion that the Chinese civilisation did not remain stagnant for two thousand years, and he cites a number of instances of the ability of the Chinese people through the ages to reason about nature and to make discoveries empirically. The failure of the Chinese civilisation to develop modern science unaided was due solely to inhibitory economic and social factors arising from geographical and climatic conditions. As a result of the impact of Western civilisation, he finds in China to-day a large number of first-rate scientists and technologists in all fields.

In an article on the Chinese contribution to scientific humanism, there is to be found a brief but enlightening analysis of the various schools of philosophy in China, which is essential to the understanding of Chinese thought.

Dr. Needham expresses the hope that peace will not bring an end to Sino-Western Science Co-operation and proposes that an International Science Co-operation Service should be set up by the United Nations.

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BOOKS RECEIVED

(Publishers are requested to notify prices.)

Divergent Series. By G. H. Hardy, Emeritus Professor of Pure Mathematics in the University of Cambridge. Oxford: at the Clarendon Press, 1949. (Pp. xvi + 396.) 30*s*. net.

Pfaff's Problem and its Generalizations. By J. A. Schouten, Professor of Mathematics at Amsterdam University, and W. v. d. Kulk, Assistant Professor of Brown University, Providence, U.S.A. Oxford: at the Clarendon Press, 1949. (Pp. xvi + 542.) 50*s*. net.

Five-Figure Tables of Mathematical Functions. By John Borthwick Dale, M.A., Fellow of King's College, University of London. Second edition. (Pp. viii + 121, including 39 tables.) 6*s*. net.

Elementary Calculus and Allied Geometry. By J. Harvey, A.R.C.Sc., B.Sc., formerly a Lecturer at City and Guilds College, S. Kensington, and at Rugby College of Arts and Technology. London: Hutchinson's Scientific and Technical Publications, 1949. (Pp. viii + 493, with 231 figures.) 25*s*. net.

Introduction to Dynamics. By Martin Davidson, D.Sc., F.R.A.S. The Winchester Study Library. London: Winchester Publications, Ltd., 1949. (Pp. 128, with 34 figures.) 5*s*. net.

Elementary Statistical Analysis. By S. S. Wilks. Princeton, N.J.: Princeton University Press; London: Oxford University Press, 1948. (Pp. xii + 284, with 35 figures and 43 tables.) 14*s*. net.

Probability Theory for Statistical Methods. By F. N. David. Cambridge: at the University Press, 1949. (Pp. x + 230.) 15*s*. net.

Cybernetics. By Norbert Wiener, Professor of Mathematics, The Massachusetts Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. 194, with 8 figures.) 18*s*. net.

The Pulsation Theory of Variable Stars. By Svein Rosseland. The International Series of Monographs on Physics. Oxford: at the Clarendon Press, 1949. (Pp. viii + 152, with 25 figures and 10 tables.) 18*s*. net.

Practical Astronomy. A Textbook for Engineering Schools and a Manual of Field Methods. By George L. Hosmer, late Professor of Geodesy, Massachusetts Institute of Technology. Fourth edition, revised and rewritten by James M. Robbins, Associate Professor in Civil Engineering, Newark College of Engineering. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xvi + 355, with frontispiece, 93 figures and 10 tables.) 27*s*. net.

The Stars are Yours. By James Sayre Pickering. New York: The Macmillan Co.; London: Macmillan & Co., Ltd., 1948. (Pp. xii + 264, with 23 plates, 5 figures and 27 charts.) 20*s*. net.

Aviation Meteorology of South America. Meteorological Office, Air Ministry, Meteorological Reports No. 1. London: H.M. Stationery Office, 1948. (Pp. 50, with 22 figures.) 5*s*. net.

Aviation Meteorology of the Azores. Meteorological Office, Air Ministry, Meteorological Reports No. 2. London: H.M. Stationery Office, 1949. (Pp. iv + 89, with 33 figures, including 5 plates.) 3*s*. 6*d*. net.

Climate through the Ages. By C. E. P. Brooks, I.S.O., D.Sc., F.R.Met.Soc. Second edition. London: Ernest Benn, Ltd., 1949. (Pp. 395, with 39 figures and 31 tables.) 21*s*. net.

Atmospheric Electricity. By J. Alan Chalmers, M.A., Ph.D.(Cantab.), Senior Lecturer in Physics in the Durham Colleges of the University of Durham. Oxford: at the Clarendon Press, 1949. (Pp. viii + 175, with 36 figures.) 15*s*. net.

Modern Introductory Physics. By Ira M. Freeman, Rutgers University. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1949. (Pp. x + 491, with 266 figures and 7 tables.) 27*s*. net.

An Introductory Course in College Physics. By Newton Henry Black, Assistant Professor Emeritus of Physics, Harvard University. Third edition. New York and London: Macmillan & Co., Ltd., 1948. (Pp. xvi + 800, with frontispiece, 22 plates and 696 figures.) 25*s*. net.

Intermediate Heat. By F. Tyler, B.Sc., A.Inst.P., Ph.D., Second Master and Senior Physics Master, Queen Elizabeth's Grammar School, Blackburn. London: Edward Arnold & Co., 1949. (Pp. xii + 429, with 191 figures and 32 tables.) 12*s*. 6*d*. net.

X-ray Optics. By A. J. C. Wilson, Department of Physics, University College, Cardiff. London: Methuen & Co., Ltd., 1949. (Pp. viii + 127, with 33 figures and 4 tables.) 6*s*. net.

High-Polymer Physics. A Symposium. Edited by Howard A. Robinson, Manager, Physical Research, Research Laboratories, Armstrong Cork Company, Lancaster, Pennsylvania. Published under the auspices of the American Institute of Physics. Brooklyn: Remsen Press Division, Chemical Publishing Co., Inc., 1948. (Pp. xiv + 572, with numerous figures and tables.) 66*s*. net.

Waveforms. Edited by Britton Chance, Assistant Professor of Biophysics, University of Pennsylvania, Vernon Hughes, Department of Physics, Columbia University, Edward F. MacNichol, Department of Biophysics, University of Pennsylvania, David Sayre, Department of Physics, Alabama Polytechnic Institute, Frederick C. Williams, Professor of Electrotechnics, Manchester University. Office of Scientific Research and Development, National Defense Research Committee. Massachusetts Institute of Technology, Radiation Laboratory Series. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1949. (Pp. xxii + 774, with 758 figures and 10 tables.) 60*s*. net.

Streamline Flow. By H. F. P. Purday, B.Sc., A.C.G.I., A.M.I.Mech.E., Chief Designer in the Higher Speed Diesel Engine Department of Harland and Wolff, Ltd. London: Constable & Co., Ltd., 1949. (Pp. viii + 185, with 53 figures and 59 tables.) 18*s*. net.

Elementary Nuclear Theory. By H. A. Bethe, Professor of Physics at Cornell University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. (Pp. viii + 147, with 17 figures and 15 tables.) 15*s*. net.

The Atomic Age. Sir Halley Stewart Lectures, 1948. By M. L. Oliphant, F.R.S., P. M. S. Blackett, F.R.S., R. F. Harrod, F.B.A., Bertrand

Russell, F.R.S., Lionel Curtis, M.A., D. W. Brogan, M.A. London: George Allen & Unwin, Ltd., 1949. (Pp. 149.) 7*s*. 6*d*. net.

Atomic Energy. Being the Norman Wait Harris Lectures delivered at North-Western University, by Karl K. Darrow, Ph.D., Bell Telephone Laboratories. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. x + 80, with 13 figures and 3 tables.) 12*s*. net.

Two Lectures. By W. Heisenberg, Director of the Max Planck Institute for Physics in Göttingen. 1. The Present Situation in the Theory of Elementary Particles. 2. Electron Theory of Superconductivity. Cambridge: at the University Press, 1949. (Pp. 52, with 2 figures.) 3*s*. 6*d*. net.

Theory of Dielectrics. By H. Fröhlich, Professor of Theoretical Physics in the University of Liverpool. Monographs on the Physics and Chemistry of Materials. Oxford: at the Clarendon Press, 1949. (Pp. viii + 180, with 47 figures.) 18*s*. net.

Fundamentals of Electric Waves. By Hugh Hildreth Skilling, Ph.D., Professor of Electrical Engineering, Stanford University. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. viii + 245, with 86 figures and 12 tables.) 24*s*. net.

Metal Rectifiers. By H. K. Henisch, Department of Physics, University of Reading. Monographs of the Physics and Chemistry of Materials. Oxford: at the Clarendon Press, 1949. (Pp. xii + 156, with 55 figures.) 15*s*. net.

Mercury Arcs. By F. J. Teago, D.Sc., M.I.E.E., Robert Rankin Professor of Electrical Engineering (Electrotechnics) in the University of Liverpool, and J. F. Gill, M.Sc., A.M.I.Mech.E., A.M.I.E.E. Second edition. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1949. (Pp. viii + 107, with frontispiece and 50 figures.) 6*s*. 6*d*. net.

Advances in Electronics. Vol. I. Edited by L. Marton, National Bureau of Standards, Washington, D.C. New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1948. (Pp. xii + 475, with 168 figures and 13 tables.) $9.00.

Radio Wave Propagation. Consolidated Summary Technical Report of the Committee on Propagation of the National Defense Research Committee. Chas. R. Burrows, Chairman; Stephen S. Attwood, Editor. New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1949. (Pp. xii + 548, with 601 figures.) $8.80.

Pulses and Transients in Communication Circuits. By Colin Cherry, M.Sc.(Eng.), A.M.I.E.E., Lecturer to Imperial College, University of London. London: Chapman & Hall, Ltd., 1949. (Pp. xvi + 317, with 129 figures.) 32*s*. net.

Fundamentals of Discharge Tube Circuits. By V. J. Francis, B.Sc., A.R.C.S., F.Inst.P., M.I.E.E., Member of the Staff of the Research Laboratories of the General Electric Co., Ltd., Wembley. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1949. (Pp. x + 134, with 40 figures.) 6*s*. 6*d*. net.

Components Handbook. Edited by John F. Blackburn, Massachusetts Institute of Technology. Office of Scientific Research and Development, National Defense Research Committee. Massachusetts Institute of Technology, Radiation Laboratory Series. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1949. (Pp. xviii + 626, with 368 figures and 91 tables.) 48*s*. net.

Principles of Servomechanisms. By Gordon S. Brown, Professor of Electrical Engineering, and Donald P. Campbell, Assistant Professor of Electrical Engineering, Massachusetts Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xiv + 400, with 207 figures and 26 tables.) 30*s*. net.

Rockets and Space Travel. By Willy Ley. London: Chapman & Hall, Ltd., 1948. (Pp. viii + 374, with 8 plates and 49 figures.) 18*s*. net.

Outlines of Physical Chemistry. By Farrington Daniels, Professor of Chemistry, University of Wisconsin. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. viii + 713, with 164 figures and 92 tables.) 30*s*. net.

Advances in Catalysis and Related Subjects. Vol. I. Edited by W. G. Frankenburg, Lancaster, Pa., V. I. Komarewsky, Chicago, Ill., E. K. Rideal, London, England. New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1948. (Pp. xiv + 321, with 82 figures and 58 tables.) $7.80.

Colloid Science. Vols. I and II. By A. E. Alexander and P. Johnson. Oxford: at the Clarendon Press, 1949. (Vol. I: pp. xxii + 1–554, with 208 figures, including 6 plates, and 48 tables; Vol. II: pp. viii + 555–837, with 100 figures, including 1 plate, and 32 tables.) 60*s*. net the set.

Surface Active Agents. By Antony M. Schwartz, Harris Research Laboratories, Washington, D.C., and James W. Perry, Massachusetts Institute of Technology, Cambridge, Mass. New York and London: Interscience Publishers, Inc., 1949. (Pp. xii + 579, with 51 figures.) 60*s*. net.

The Electronic Theory of Organic Chemistry. By M. J. S. Dewar. Oxford: at the Clarendon Press, 1949. (Pp. xii + 324, with 7 figures and 38 tables.) 30*s*. net.

Technique of Organic Chemistry. Vol. II: Catalytic Reactions. By V. I. Komarewsky and C. H. Riesz, Illinois Institute of Technology, and Institute of Gas Technology, Chicago; Photochemical Reactions. By W. Albert Noyes, Jr., and V. Boekelheide, University of Rochester; and Electrolytic Reactions. By Sherlock Swann, Jr., University of Illinois. New York and London: Interscience Publishers, Inc., 1948. (Pp. x + 219, with 66 figures and 9 tables.) 30*s*. net.

Recent Advances in Organic Chemistry. Vol. III. By Alfred W. Stewart, D.Sc., and Hugh Graham, D.Sc., Reader in Organic Chemistry in the Queen's University of Belfast. Seventh edition. London, New York, Toronto: Longmans, Green & Co., 1949. (Pp. xii + 387, with 2 plates and 3 figures.) 38*s*. net.

Organic Reactions. Vol. IV. Roger Adams, Editor-in-Chief. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. viii + 428, with 44 tables.) 36*s*. net.

The Preparation, Properties, Chemical Behaviour, and Identification of Organic Chlorine Compounds. By Ernest Hamlin Huntress, Professor of Organic Chemistry, Massachusetts Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xxvi + 1443.) £8 5*s*. net.

Advances in Food Research. Vol. I. Edited by E. M. Mrak, University of California, Berkeley, and George F. Stewart, Iowa State College, Ames. New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1948. (Pp. xvi + 459, with 39 figures and 51 tables.) $7.50.

Methods of Quantitative Micro-Analysis. Collected and edited by R. F. Milton, B.Sc., F.R.I.C., and W. A. Waters, M.A., Sc.D., Ph.D., F.R.I.C.

London: Edward Arnold & Co., 1949. (Pp. viii + 599, with 169 figures.) 60*s*. net.

Trace Elements in Food. By G. W. Monier-Williams, O.B.E., M.C., M.A., Ph.D., F.R.I.C., formerly Inspector of Foods, Local Government Board, and Chemist in Charge of Food Research Laboratory, Ministry of Health. London: Chapman & Hall, Ltd., 1949. (Pp. viii + 511.) 30*s*. net.

Analytical Chemistry and Chemical Analysis, 1948. Proceedings of the International Congress on Analytical Chemistry, Utrecht, 1948. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. (Reprint of *Analytica Chimica Acta*, Vol. 2, pp. 417–854, with numerous figures and tables.) 25*s*. net.

Thorpe's Dictionary of Applied Chemistry. Vol. IX: Oils, Fatty—Pituitary Body. Fourth edition. London, New York, Toronto: Longmans, Green & Co., 1949. (Pp. viii + 671, illustrated.) 80*s*. net.

The Technology of the Fischer-Tropsch Process. By B. H. Weil and J. C. Lane. London: Constable & Co., Ltd., 1949. (Pp. xiv + 248, with 19 figures and 24 tables.) 22*s*. 6*d*. net.

(This is an English edition, with some fresh material and further references, of *Synthetic Petroleum from the Synthine Process*, published in the U.S.A. in 1948, and reviewed in Science Progress, Vol. 37, p. 165, Jan. 1949.)

Physical Geology. By Chester R. Longwell, Henry Barnard Davis Professor of Geology, Adolph Knopf, Sterling Professor of Physical Geology, and Richard F. Flint, Professor of Geology, Yale University. Third edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. xviii + 602, with 365 figures.) 30*s*. net.

Outline of Historical Geology. By A. K. Wells, D.Sc. Second edition revised with the assistance of J. F. Kirkaldy, D.Sc. London: Thomas Murby & Co., 1948. (Pp. xvi + 356, with 2 plates and 124 figures.) 25*s*. net.

Structural History of the East Indies. By J. H. F. Umbgrove, D.Sc.(Leyden), Professor of Geology at Delft, Holland. Cambridge: at the University Press, 1949. (Pp. xii + 63, with 10 plates and 68 figures.) 30*s*. net.

The Ocean. By F. D. Ommanney, Ph.D., A.R.C.S., F.L.S. The Home University Library of Modern Knowledge No. 203. London: Oxford University Press, 1949. (Pp. x + 238, with 12 figures.) 5*s*. net.

The Soil and the Sea. A Symposium. Edited by Trevor I. Williams. London: Saturn Press, 1949. (Pp. 240, with 8 plates.) 10*s*. 6*d*. net.

Botanik der Gegenwart und Vorzeit. By Karl F. W. Jessen (1864). Republished as Vol. I of the "Pallas" series of reprints by the Chronica Botanica Co., Waltham, Mass., 1948. London Agents: Wm. Dawson & Sons, Ltd. (Pp. xxii + 495.) $6.00.

Lectures on the Inorganic Nutrition of Plants. (Prather Lectures at Harvard University.) By D. R. Hoagland, Professor of Plant Nutrition, University of California. Vol. XIV of a New Series of Plant Science Books, edited by Frans Verdoorn. Second edition. Waltham, Mass.: The Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1948. (Pp. xii + 226, with frontispiece, 28 plates and 44 figures.) $4.50.

The Cytoplasm of the Plant Cell. By Alexandre Guilliermond, Professor of Botany at the Sorbonne, Paris—Membre de l'Institut. Authorised translation from the unpublished French manuscript by Lenette Rogers Atkinson, Ph.D. Vol. VI of a New Series of Plant Science Books, edited

by Frans Verdoorn. Waltham, Mass.: The Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1941 (second printing 1948). (Pp. x + 247, with 152 figures.) $5.00.

Giant Fishes, Whales and Dolphins. By the late J. R. Norman, F.L.S., and F. C. Fraser, D.Sc., F.L.S., both of the Department of Zoology, British Museum (Natural History). Illustrated by Lieut.-Col. W. P. C. Tenison, D.S.O., F.L.S. Second edition. London: Putnam & Co., Ltd., 1949. (Pp. xxii + 376, with 8 coloured plates and 97 figures.) 15*s*. net.

Conseil Permanent International pour l'Exploration de la Mer, *Annales Biologiques*, Vol. IV, 1947. Copenhagen: Andr. Fred. Høst & Fils, 1947. (Pp. 152, with numerous figures.) Kr. 18.00.

A Textbook of Entomology. By Herbert H. Ross, Systematic Entomologist, Illinois State Natural History Survey; Professor of Entomology, University of Illinois. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. x + 532, with 434 figures.) 36*s*. net.

The Spider Book. By J. H. Comstock. Revised and edited by W. J. Gertsch, Ph.D., Associate Curator, Department of Entomology, American Museum of Natural History. Ithaca, N.Y.: Comstock Publishing Co., Inc.; London: Constable & Co., Ltd., 1948. (Pp. xii + 729, with frontispiece and 771 figures, including 1 coloured plate.) 35*s*. net.

Bees and Honey. By George A. Carter, B.Sc., A.R.I.C. Published by "Bee Craft," Bracken Dene, Manor Way, Petts Wood, Kent. (Pp. 115, with 13 plates.) 5*s*. 6*d*. net.

Das biologische Weltbild. Vol. I: Die Stellung des Lebens in Natur und Wissenschaft. By Dr. Ludwig von Bertalanffy, Professor an der Universität Wien. Bern: A. Francke AG. Verlag, 1949. (Pp. 202.) Paper, Swiss Frs. 11.–; cloth, Swiss Frs. 14.50.

The Unitary Principle in Physics and Biology. By Lancelot Law Whyte. London: The Cresset Press, 1949. (Pp. x + 182.) 12*s*. 6*d*. net.

Mathematical Biophysics. By N. Rashevsky. Revised edition. U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1948. (Pp. xxiv + 669, with 193 figures.) 42*s*. net.

Biochemical Evolution. By Marcel Florkin, University of Liége. Edited, translated and augmented by Sergius Morgulis, University of Nebraska, College of Medicine. New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1949. (Pp. viii + 157, with 25 figures and 13 tables.) $4.00.

Biological Applications of Tracer Elements. Cold Spring Harbor Symposia on Quantitative Biology, Vol. XIII. Cold Spring Harbor, L.I., N.Y.: The Biological Laboratory, 1948. (Pp. xii + 222, with 4 plates and numerous figures and tables.) $7.00.

The Hormones: Physiology, Chemistry and Applications. Vol. I. Edited by Gregory Pincus, Worcester Foundation for Experimental Biology, Shrewsbury, Mass., and Kenneth V. Thimann, Harvard University, Cambridge, Mass. New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1948. (Pp. xii + 886, with numerous figures and tables.) $13.50.

Techniques of Histo- and Cytochemistry. By David Glick, Ph.D., Associate Professor of Physiological Chemistry, the Medical School, University of Minnesota. New York and London: Interscience Publishers, Inc., 1949. (Pp. xxiv + 531, with 159 figures and 10 tables.) 48*s*. net.

Bacterial Metabolism. By Marjory Stephenson, Sc.D., F.R.S., Reader in Chemical Microbiology in the University of Cambridge. Third edition.

London, New York, Toronto: Longmans, Green & Co., 1949. (Pp. xiv + 398, with 1 plate, 81 figures and 99 tables.) 30*s.* net.

Anæsthetics and the Patient. By Gordon Ostlere, M.A., M.B., B.Chir., D.A. Sigma Introduction to Science 15. London: Sigma Books, Ltd. 1949. (Pp. 166, with 4 plates and 6 figures.) 7*s.* 6*d.* net.

Your Body and the Way it Works. By Professor Winifred Cullis, C.B.E. Illustrated by Ian T. Morison, D.A. Understanding the Modern World Series. London: George Allen & Unwin, Ltd., 1949. (Pp. 32.) Library edition, 3*s.* 6*d.* net; school edition, 2*s.* 6*d.* non-net.

The Story of Human Birth. By Alan Frank Guttmacher, M.D., Associate Professor of Obstetrics, Johns Hopkins University. English edition prepared by G. Gordon Lennon, M.B., Ch.B., Ch.M., M.R.C.O.G., M.M.S.A., Nuffield Department of Obstetrics and Gynæcology, Oxford. Sigma Introduction to Science 17. London: Sigma Books, Ltd., 1949. (Pp. 214, with 7 figures.) 7*s.* 6*d.* net.

Grow up—and Live. By Eustace Chesser. Pelican Books No. 149. Harmondsworth: Penguin Books, Ltd., 1949. (Pp. 295.) 1*s.* 6*d.* net.

The Psychology of Sex. By Oswald Schwarz. Pelican Books No. A194. Harmondsworth: Penguin Books, Ltd., 1949. (Pp. 296.) 1*s.* 6*d.* net.

World Philosophy: A Search for Synthesis. By Oliver L. Reiser, Professor of Philosophy, University of Pittsburgh. University of Pittsburgh Press, 1948. (Pp. x + 127.) $2.50.

The Logic of the Sciences and the Humanities. By F. S. C. Northrop, Sterling Professor of Philosophy and Law in Yale University. New York: The Macmillan Co.; London: Macmillan & Co., Ltd., 1948. (Pp. xiv + 402.) 22*s.* 6*d.* net.

Scientists and Amateurs. A History of the Royal Society. By Dorothy Stimson. London: Sigma Books, Ltd., 1949. (Pp. xvi + 270, with 20 plates.) 15*s.* net.

Greek Science. II: Theophrastus to Galen. By Benjamin Farrington. Pelican Books No. A192. Harmondsworth: Penguin Books, Ltd., 1949. (Pp. 181, with 2 figures.) 1*s.* 6*d.* net.

Goethe on Nature and on Science. By Sir Charles Sherrington, O.M. The Philip Maurice Deneke Lecture delivered at Lady Margaret Hall, Oxford, on the 4th March 1942. Second edition. Cambridge: at the University Press, 1949. (Pp. 54.) 2*s.* 6*d.* net.

Science in Liberated Europe. By J. G. Crowther. London: The Pilot Press, Ltd., 1949. (Pp. 336.) 18*s.* net.

The Wealth of India. A Dictionary of Indian Raw Materials and Industrial Products. Vol. I in two parts, Raw Materials and Industrial Products. Delhi: Council of Scientific and Industrial Research, 1948. Raw Materials: (Pp. xxviii + 254, with 39 plates.) 24*s.*; Industrial Products: (Pp. xii + 182, with 8 plates and a number of other illustrations.) 12*s.* Insurance and postage, 8*s.*

The Prehistory of Southern Rhodesia. An Account of the Progress of Research from 1900 to 1946. By Neville Jones, lately Keeper of the Department of Prehistory, Ethnography and National History in the National Museum of Southern Rhodesia. Museum Memoir No. 2. Cambridge: at the University Press, 1949. (Pp. 78, with frontispiece and 40 figures.) 7*s.* 6*d.* net.

Oxford Junior Encyclopædia. Vol. III: The Universe. General Editors, Laura E. Salt and Geoffrey Boumphrey. London: Oxford University Press, 1949. (Pp. xvi + 496, illustrated.) 30*s.* net.

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SCIENCE PROGRESS

THE SCIENCE AND PRACTICE OF MILKING

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THE craft of the milker is not only an ancient and honourable one, but it is one that has increased rather than diminished in importance with the modern realisation of the outstanding nutritional value of milk. As to its antiquity, domesticated cows are known to have been milked in India at least 3500 years ago, and probably earlier in ancient Egypt. As to its labour demands in this country, some quarter of a million men and women must spend an average of about two hours daily, say, a total of 150 million man-hours per year, in extracting the 1500 million gallons of milk produced by our 3 million milch cows.

All of these milkers observe every day the fact that a lactating cow requires a definite stimulus of some kind before she will "let down" or discharge her milk at milking-time. All of them will have noticed that if the cow is disturbed or frightened she may "hold back" all or part of her milk. As regards the stimulus to "let down," in India and the East the calf is frequently used and is allowed a few sucks at the teats before it is driven away and replaced by the human hand. But, in most dairying countries, cows have for many generations been conditioned to let down their milk in complete absence of the natural beneficiary, shortly after the milker's hand—or nowadays ever more extensively the milking machine—has been applied to the teats.

No one who has milked a cow either by hand or machine can have failed to notice the very clear-cut nature of this physiological act of let-down. In fact, it is common to all lactating mammals—paralleled, for example, in the nursing human mother by the so-called "draught" experienced by many women when the suckling is put to the breast. Yet it is only within the last few years that real knowledge as to what takes place at let-down has been acquired.

The word "let-down" is, in fact, a misnomer; the phenomenon

is more of an active ejection than a passive function. Thus, in a normal, average cow, if the quantities of milk obtained by any constant milking procedure are measured at, say, half-minute intervals, it will be found that after a slow start in the first half-minute the milk begins to pour out with rapidly increasing speed, reaching a maximum between the first and second minute and then tailing off over perhaps four to five minutes in all. In the present article, it is not proposed to deal with the whole field of the physiology of lactation, in which there has been much scientific progress during the past two decades, but to restrict consideration to the recent advances in the physiology of milking, where discoveries of the past few years have been not only of academic interest, but of practical importance both on the dairy farm and to those concerned with human lactation. It will be necessary to mention something of the anatomy and histology of the cow's udder; the structure of the mammary gland of almost every species of mammal is, in essentials, similar.

Scientific Aspects

Structure of the Udder

The cow's udder is made up of four "quarters"—separate glands in close juxtaposition but having no communication with each other as far as their milk secreting and storage systems are concerned. Let us consider one of these glands. Beginning with the teat orifice and working upward, we have first a narrow canal at the lower end of the teat, which is normally kept closed by a muscular sphincter. The teat canal leads into a teat sinus, which is roughly conical in shape and opens at its upper end, through circular folds of tissue, into the gland cistern, a capacious, sub-divided vessel branching into some dozen large ducts which spread, dividing and sub-dividing, into the depths of the gland. Finally, the ultimate tiny ductules each end in a secretory alveolus, a hollow spherical or often pear-shaped structure, lined with fairly large, almost cubical cells, each with a big nucleus. It is in these cells that the milk is made—all the constituents of milk being made by the same cells—from the materials brought to them by the circulating blood. At intervals the free surface of each secreting cell bulging into the alveolar space gives way, the secreted milk liberated from the cell enters this space, the cell diminishes considerably in volume and the milk manufacture begins again within it.

The system of ducts and alveoli in the mammary gland has been aptly described as roughly similar to an inverted bunch of grapes.

The tiny channels which end in each alveolus are like the small twigs each of which ends in a grape. The alveoli are, of course, very much smaller in size than a grape, and there are many more of them —of the order of scores of millions—in one-quarter of the udder than there are grapes in the most magnificent bunch.

An important fact clearly determined by the recent histological work of Richardson is that each minute alveolus is enclosed in the processes of a number of so-called myo-epithelial cells, as the contents of a basket are surrounded by the interlacing basket work. This myo-epithelial system will be referred to later.

Intra-Mammary Pressure

If a normal cow is milked out, and a cannula, attached to a simple pressure gauge, is then inserted through the teat into the main gland cistern, the pressure registered by the secreted milk in the cistern will be low to begin with, will slowly rise during the few hours between milkings to perhaps 20–25 mm. of mercury. The curve will then tend to flatten out, especially if the cow is a high producer, some little time before the second milking. But within a very short time (of the order of one minute) of the beginning of the milking operation itself, the pressure will rise quite suddenly to about double its previous level. This sudden rise in pressure is the outward indication of the let-down phenomenon, and is, of course, associated with the increased turgidity of the lower part of the udder which is usually observable at the beginning of the rapid flow of milk (Fig. 1).

The cause of the first, slow rise is, clearly, the steady, continuous secretion of milk which is beginning to fill the udder cisterns. What is the cause of the second, very much quicker rise? Is it a rapid increase in the secretion rate, following the sensory stimulus to the teat of the milker's hands or the mouth of the young suckling, or is there some other cause? In any circumstance, it appears to be a reflex—an involuntary reaction to a sensory stimulus.

It has been shown by two or three dairy research groups that all the milk obtained at a milking (and considerably more, as will be seen later) is present in the udder before milking begins. It is now known that secretion of milk is a process going on steadily throughout the whole of the day, only checked when the spongy tissue of the gland becomes full of milk and possibly stopped for a short time during the let-down process. There is certainly no sudden *increase* in secretory activity of the alveolar cells at milking-time.

What happens, therefore, must be some form of contraction of the udder tissue, acting on the milk already present in the udder,

and actively squeezing it out from the spongy udder tissue into the large collecting ducts and cisterns. The size of the alveolar spaces and the diameter of the innumerable tiny ductules are such that

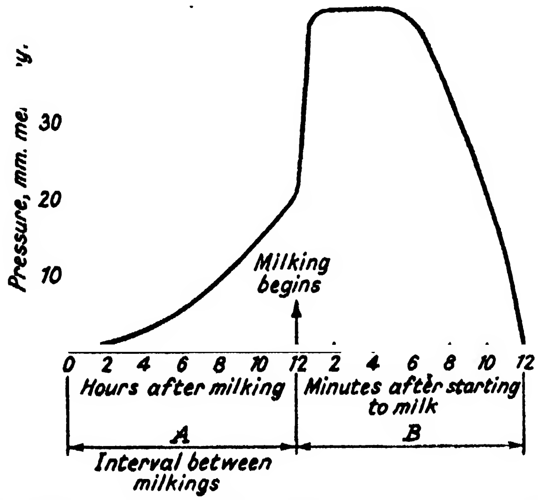

FIG. 1.—Changes in intra-mammary pressure in relation to milking-times.
(*Two curves from* TGETGEL (1926) *put together.*)
Note. One division of scale A equals 60 divisions of scale B.

milk could only be mobilised quickly from them if there was some very widespread, positive pressure diminishing the volume of the alveolar space and hurrying secreted milk out through the small tubules.

THEORIES OF "LET-DOWN"

In many reflex actions, *e.g.* the rapid closing of the eyelid when the near approach of some possibly dangerous object is sighted, there is no doubt that a completely nervous circuit is involved, the message going from eye to brain and the final closure of the eyelid being accomplished by muscular contraction resulting from an efferent *nervous* stimulus from the central nervous system. But in relation to milk, there is, between the appropriate stimulus and the actual let-down, a distinct interval of up to a minute or so before the reflex activity begins.

Several theories have been advanced to account for this interval and the other peculiarities of let-down. It has been suggested that the squeezing mechanism is brought about by the reflex contraction, often a rather slow process, of unstriated muscle of which

there is some in the teat and in the udder. It has further been suggested (Hammond, 1936) that the udder tissue suffers a type of erection, similar to that following tactile stimulus of the penis, brought about by reflex nervous contraction of the plain muscle associated with the veins. This would occlude the latter vessels and lead to their engorgement with blood, thereby causing the increase in mammary turgidity observable at let-down. But it has been shown that there is no difference in the milk yield of the two halves of the udder of a cow when the *efferent* nerve trunk to one of the halves has been cut.

The modern view, supported now by a great deal of evidence, may be briefly described as the *hormonal* theory of let-down. It is now more than a theory; it must be regarded as a well-founded and well-established explanation of this phenomenon. Many research workers have contributed to it, but only a few names will be specifically mentioned here. It takes its origin from an observation made nearly forty years ago (1910) that injection of an extract of the posterior portion of the pituitary gland into the circulating blood causes a flow of milk from a lactating animal whose mammary gland has been cannulated through the teat. But at that time the flow, which is of relatively short duration, was—as has previously been mentioned—supposed to be the result of a temporary stimulus to increased activity of the secreting cells of the udder.

The next very long step was taken by Gaines in America, working with several species of laboratory mammals. He showed in 1915 that in the lactating goat the injection into the circulating blood of a posterior-pituitary gland extract caused an increase in intra-mammary pressure quite similar to that produced in the natural let-down of milk. After the injection there was a pause of a fraction of a minute, then a sharp rise in milk pressure, which then declined steadily over several minutes. If in the guinea pig the tip of the teat of a lactating gland isolated from the body was cut off, injection of the extract into the tissue of the gland caused a rapid flow of milk from the stump. He showed also that, while anæsthesia prevented a bitch from letting down her milk to actively suckling pups, after she had been given an injection of posterior-pituitary extract, though she was still anæsthetised, the pups were able to obtain the normal weight of milk. Gaines, in fact, more clearly than any previous investigator, demonstrated the two distinct halves to the act of milking: (1) the lactating animal's part, by which she brings about an increase in intra-mammary pressure and which could be simulated by injection of posterior-pituitary extract, (2) the part played by some other individual, *i.e.* the suckling calf or the hand

(or machine) of the milker, in overcoming the control of the teat sphincter over the discharge of milk from the udder. Gaines was also one of the first to show that there was more milk in the udder than was obtainable at any one milking. Though much of the evidence was before him, he did not go so far as to make the definite claim that the *normal* mechanism which brings about the increase in mammary pressure at milking is hormonal in nature, though he went very close to such a statement. He also considered that the efferent agent, whatever it was, directly stimulated some type of muscular tissue closely associated with the secretory portion of the gland. Gaines had not the benefit of the large amount of work done in the next twenty-five years on the hormonal control of lactation generally, nor had Pavlov's book on conditioned reflexes been published.

Turner and his colleagues in the 1930's further studied the effects of pituitary extracts on the let-down of milk. They came to the conclusion which Gaines just failed to grasp, namely, that the let-down of milk is a reflex normally brought about by sensory stimulation of the teats (though other sensory stimuli, as will be mentioned later, may also bring about the same result) and implemented through the central nervous system *by reflex liberation of a hormone from the posterior pituitary gland*, which is discharged into the blood and causes some contractile mechanism in the mammary gland to drive secreted milk from the alveolar spaces and small ducts into the gland cisterns. Petersen and his fellow-workers, on the basis of additional experimental evidence which they have published since 1941, have come to still more definite conclusions. They have stated that the normal let-down of milk is caused by the action of the oxytocic hormone from the posterior pituitary and can be inhibited (or at least the effect of the pituitary hormone on the gland prevented) by adrenalin. With his colleagues, Petersen reported (*a*) that, in cows in which the motor nerves to one-half of the udder had been cut, the let-down follows equally in both halves of the udder following the intravenous injection of oxytocin; (*b*) that about 45 seconds intervene between the application of the sensory stimulus to the gland and the let-down of milk, which agrees with the time required for the hormone to be carried by the blood from the pituitary to the mammary gland in the cow; (*c*) that, in experiments on isolated lactating udders kept alive by perfusion with oxygenated blood from the donor of the gland, the replacement of this blood by blood taken from cows just previously stimulated to let down their milk causes a copious flow of milk from the isolated udder, whilst blood taken from cows not so stimulated has no effect;

(*d*) that if blood from cows stimulated to let down stands for half an hour before it is perfused, its activity in causing a copious flow of milk is destroyed (this is in line with the observation that if cows are stimulated to let down and subsequent milking is delayed, incomplete evacuation often results, though this may equally be due, as suggested by Schnickel, to a falling off in sensitivity of that contractile mechanism in the gland which is stimulated by oxytocin); (*e*) that blood taken from "markedly excited" cows does not cause a let-down, but has a very pronounced effect in causing constriction of the blood vessels in the perfused udder (this is in line with the observation that the intravenous injection of adrenalin into lactating cows interferes with the let-down of milk even when the cows have just previously been stimulated to let down).

Though from these investigations (which still await complete confirmation) it seemed clear that oxytocin plays a major rôle in the let-down reflex, the exact way in which the hormone, circulating in the blood, brings about a rise in intra-mammary pressure was still uncertain. To obtain this rise in pressure something must squeeze the mammary sponge in such a way that the secreted milk is forced out of the alveoli and smaller ductules into the large collecting vessels of the gland. Oxytocin is known to cause the contraction of smooth (unstriated) muscle in various organs of the body, but the smooth muscle of the udder appeared insufficient in quantity, and what there was of it was in the wrong places, *i.e.* not associated with the alveoli.

The Function of the Myo-Epithelial Structure

This uncertainty seems now to have been resolved by the recent outstanding histological work of Richardson (1949) on udder tissue before and after let-down, particularly on the so-called myo-epithelial cells associated with the alveoli and ducts. Of these cells, the big nuclei, lying close to the large secreting cells of the alveoli and appearing almost to form a part of the epithelial membrane to which the secreting cells are attached, had been noticed many years ago, but the rest of their structure had proved more elusive. By improved methods of staining, Richardson has clearly demonstrated that each myo-epithelial cell (of which there are many associated with a single alveolus) recalls in shape a starfish with several very thin arms. These arms or processes interlock with those of similar cells and surround each alveolus with a kind of basketwork. He has produced evidence that in the udder of the goat after let-down these processes have actually contracted (and not collapsed) with the squeezing-out of the milk from the previously distended

alveolar lumen through the ductule. There is, he says, in the udder tissue "an outstanding abundance of myo-epithelium, and its orientation is everywhere consistent with its functioning as a contractile tissue expressing milk from the alveoli and ducts."

If these findings are confirmed, the principal chapters of the let-down story are approaching completion, though there are still important aspects which require further elucidation.

Practical Aspects

From the practical point of view, the obvious object of the good cowman, whether working by hand or machine, is to obtain from each cow as much milk as possible, of as good quality as possible, as quickly as possible and with no damage to the cow. It has been known for a very long time that the last milk drawn from the udder is the richest in fat, and that it is, therefore, highly desirable to obtain these "strippings," even if a little extra time and effort is needed to secure them. On the other hand, it is also known that if the milking-time is unduly prolonged, the amount of milk obtained at any one milking tends to fall off. Thus Crowther (amongst others) showed over thirty years ago that there was in general a progressive decrease in the amount of milk and fat drawn from the four quarters if they were milked in succession. This phenomenon was at one time thought to arise because the first quarters milked were able to drain some milk at least from the other quarters, but it is now well-established that the four quarters are quite milk-tight with respect to each other.

For some years the advances in knowledge of the physiological basis of the secretion and let-down of milk did not have much direct effect on the art of milking the cow, or on the not less important technique of obtaining as complete and adequate a lactation as possible in the human mother. Gynæcologists, pædiatricians and physiologists interested in human lactation are now beginning to apply the findings just outlined (which have largely been derived from farm animals), with what modifications are necessary, to this highly important but hitherto rather neglected function. Waller (1947) has described the endeavours now being made to utilise this knowledge in the prevention of lactational failure in women. (This aspect of the subject will not be further discussed in the present article.) On the dairy farm, the steady improvement in average milk yield of the dairy cow, the associated slow changes in the size and shape of the udder, the ever-present menace of udder infection and the rapid spread of machine milking have made it increasingly important that physiological knowledge of udder function should be effectively

applied. As Foot has put it, "the milking machine operator is urgently in need of reliable data on such matters as the influence of common environmental factors on the rate of flow, the scope for training cows to a quicker routine, and the effect on yield of reducing milking-time at the expense of leaving small quantities of milk in the udder. Apart from considerations of yield and economy, there is also the very important matter of the effect of milking technique on mastitis."

There has, in fact, been much thought given of late to these questions. Some of them have recently been, at least partially, answered, and a number of wrong ideas have been dislodged.

"Super-Strippings"

Thus, the belief that by careful hand- or machine-stripping it is possible to empty the udder of milk is one which the use of pituitary extract has shown to be very far from the truth. Some recent experiments of Dodd and Foot may be quoted. Using a group of eight normal Shorthorn cows in mid-lactation which were being regularly "completely" milked out and stripped by machine by a competent cowman, they injected all of the animals, immediately after one morning milking per week, with oxytocin, and repeated this on one day each week for sixteen weeks. The extra milk obtained after the injection may conveniently be called the "super-strippings." It comprised, on the average, some 22 per cent. of the "total" milk, *i.e.* of the total amount obtained at the normal + the oxytocin-induced let-down. It was very rich in fat, being, in effect, thin cream with an average fat content of 13·4 per cent., that of the normal milking being only 3·23 per cent. It thus contained 56 per cent. of the "total" fat present in the udder when milking began. It must be news even to most dairy farmers that on the average only 46 per cent. of the "total" milk-fat present (and actually secreted) in the udder is obtained by the ordinary methods of milking. The discharge, at a normal let-down, of hormone from the posterior pituitary causes a contraction of the myo-epithelial system which is evidently insufficient to squeeze out, with any approach to completeness, the secreted milk, and particularly the fat, from the alveoli and ducts. The residual milk and fat closely held, probably by capillary forces, in these very fine vessels require another dose of the hormone to squeeze them out effectively. A cow "milked dry" by ordinary methods on the farm is, in fact, still quite "wet"—and that irrespective of whether hand or machine is used.

It was further found that there was a strong tendency to compensate for the increase in yield at the oxytocin milking by a diminu-

tion at the next. This is illustrated by the average yields of eight cows on the day before injection (34·0 lb.), on the day of injection (39·0 lb.) and on the two following days (29·1 lb. and 34·8 lb.). This fairly exact compensatory effect was observed in all eight cows.

Butter-fat percentage in the milk is a common criterion of milk quality. Whilst the ordinary strippings, which contain more fat than the main bulk of milk, are of importance in maintaining overall butter-fat percentage, the effect on milk quality of obtaining even a little of the super-strippings is great, and may be the main reason for the large variations frequently found between successive morning milkings (or successive afternoon milkings) in cows undergoing an apparently constant routine. A slightly more efficient pituitary discharge will lead to a marked increase in fat percentage in the milk and, of course, a small increase in the total amount of milk. It is, therefore, one of the desiderata of any good milking routine in the cowshed to produce as complete a let-down as possible. As Dodd and Foot remark, "the cow's co-operation by improved 'let-down' rather than her hostility induced by excessive stripping will be much more effective in reducing the residuum (of milk) over a period". They point out that the amount of milk normally left in the udder affects not only the current output, but has a cumulative effect in reducing future production. Finally, they warn both advisers in dairy husbandry and those engaged in experimental work on factors affecting milk yield and quality that an experimental treatment may itself affect the completeness of let-down of milk, and that for this reason there is a danger of attributing to a treatment an effect on secretion *into* the udder which, in fact, should be attributed to its effect on our efforts to get milk and fat *out of* the udder.

Improving Efficiency of Let-down

Much of the recent experimentation in the cowshed has been to devise the best means for ensuring under practical conditions the most complete and efficient let-down. To begin with, it is necessary to have some means of measuring the effect of this or that change of routine on the milk-flow curve after let-down.

To do this, a constant method is necessary for actually removing the milk from the udder. Hand-milking, even by the best of milkers, is irregular (and one milker can, of course, only milk two teats at one time), but with a good milking *machine*, used under standardised conditions, it is possible to obtain accurate milking curves by weighing the receiving bucket at short intervals during the milking. In this way, a curve (see Fig. 2) is obtained, usually

with a maximum between one and two minutes after putting on the teat cups. The remarkable feature of this curve is its constancy of shape for any one animal from one day to another, and the wide variation in shape of curve from one animal to another (Dodd and Foot (Shinfield); Korkman (Sweden)).

On the general basis of Pavlov's findings that a sharp, rigidly constant afferent stimulus is the most effective in obtaining a maximal efferent result in a conditioned reflex, endeavours have been made to provide such a stimulus in the cowshed, so that the maximal

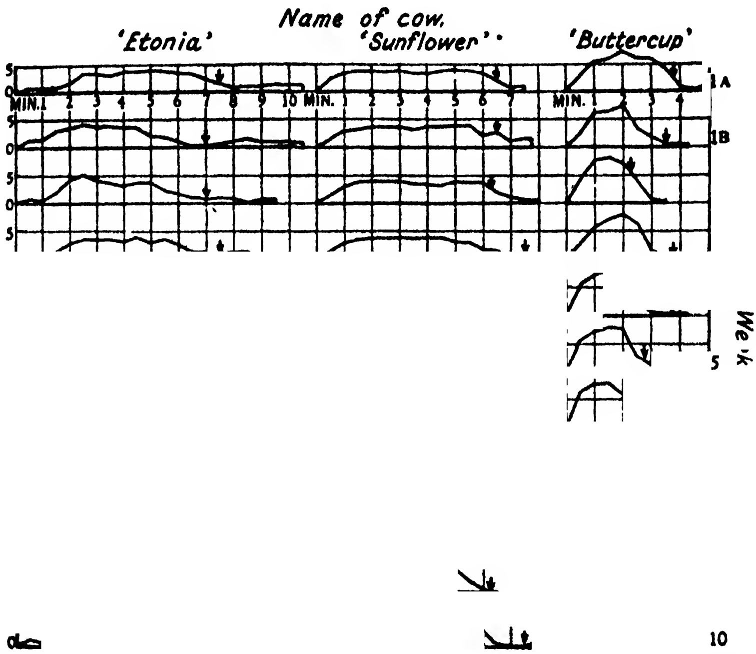

FIG. 2.—Constancy of shape of rate-of-flow curve in individual cows (one milking of each cow each week).

(*Taken from* DODD *and* FOOT (1947) *by permission of the Editors of the "Journal of Dairy Research."*)

Arrow = point at which teat cups were manipulated and "machine-stripping" began.

release of oxytocin will take place at the right time in relation to the act of milking. Some have claimed that if the cow's udder is regularly washed with a cloth soaked in fairly hot water (say, at 115° or 120° F.) a short time—say one to two minutes—before milking begins, then she can soon be trained to let down her milk much more freely—both more quickly and with a greater yield. This is contested by others (*e.g.* Dodd and Foot) who find relatively little effect of the hot-water routine on yield or quality of the milk in the great majority of cows. Knoop in U.S. also finds that varia-

tion in the temperature of the water used for washing (massage) of the udder of from 50° to 130° F. does not affect the rate of machine milking.

Tactile stimulus of the teats and udder by the calves' mouths, by the hand of the milker, or by hot-water treatment, is not the only type of stimulus to let-down to which cows may be trained or become accustomed. In some Indian herds, for example, the sight or smell of the calf (even the sight of the stuffed calf-skin) may be the normal stimulus to let-down; in some herds in this country the rattling of milk buckets on the cowshed floor is said to be the usual stimulus. There is little doubt that with care and persistence cows could be trained to let down their milk by almost any slightly unusual sensory stimulus, visual, tactile, olfactory or aural, which they came to associate strongly with the act of milking.

Reduction of Time needed for Milking

It has been further stated that using such a regular and clear-cut stimulus as the hot-water one, the average length of time during which the teat cups are usually applied (say, 4–8 minutes depending on the cow and the period of lactation) may be reduced to, say, 3–4 minutes, and that this procedure gives at least as good yields and quality of milk, is much less likely to cause udder damage by leaving the cups on the teats too long and, of course, permits a herd to be milked with appreciable economy in man-hours. This again is contested by others who say that in actual practice the reduction of the milking-time to 60 per cent. of the original time has no great influence on the rate of milk-flow, even if hot water is used as the stimulant (Dodd and Foot). However, the restriction in milking time causes only a small reduction in yield, as the flow of milk during the latter part of the flow period is in most cases small. It is nevertheless found that the fat content of the milk of the cows whose milking-time is restricted falls somewhat. Another interesting finding is that the incidence of mastitis—perhaps the most serious disease in dairy herds in all the more important dairying countries—appears to be unaffected by using either the hot-water technique or restricted milking-time. When, instead of restricting the milking-time, the teat cups are left on at each milking for *twice* as long as is needed for a normal milking, neither the yield nor the quality of milk, nor even the rate of milking, is affected (Dodd and Foot). American workers find that if cows accustomed to washing immediately prior to milking are milked without the washing, the start of let-down is delayed by 30–60 seconds. Once the unwashed cows

have let down their milk, however, they milk at the same rate as the stimulated group, the total duration of milking being somewhat longer because of the slow start. There is no difference in yield between the two treatments.

In recent (unpublished) experiments of the Shinfield group, cows accustomed to a routine of hot-water preparation followed by milking one minute later were subjected on single mornings to a sudden change of technique. These changes consisted in (*a*) milking without any preparation; (*b*) milking 3 minutes instead of 1 minute after preparation; (*c*) milking 6 minutes instead of 1 minute after preparation. Using a technique they have devised for increasing the exactness of measurement in this rather unprecise field, these workers have shown that, whilst treatment (*a*) delayed let-down, a reasonably normal milking rate was given thereafter. Treatment (*b*) had no pronounced effect on milking efficiency, but treatment (*c*) resulted in a *slightly* lower rate of milking and a reduced yield of milk and butter fat.

In a second experiment, a comparison was made between two *established* routines, in one of which udder washing with hot water and the giving of a feed of concentrates took place less than 1 minute before, and in the other more than 20 minutes before the beginning of machine milking. The findings indicate that milking immediately after preparation is the more effective routine, though again the differences were not great.

Korkman in Sweden has also examined this question recently in some detail and has confirmed the general picture just outlined. He finds, however, that low-yielding and high-yielding cows differ somewhat in their reaction to the warm-water stimulus to the udder. It may well be that when experiments are carried out in the better-managed herds, where regular methods of milking have been in force for a long time, the effects of introducing another routine—such as the regular stimulus to let-down by the warm-water technique—will give little or no improvement. It may well be—and there is some rather vague evidence to support this view—that, in many commercial herds with poorer milking methods to begin with and lower yields, the effects of introducing a regular technique of stimulating let-down, and thus, in effect, paying more attention generally to methods of milking, may have quite marked results in improving yields, and even quality, of milk.

Stripping by Machine

A serious difficulty in machine milking, and one that has undoubtedly been responsible in the past for a fall in milk yield when milking

machines have replaced hands, is that often, towards the end of milking, as the rate of removal of milk diminishes and the amount of "vacuum" exerted on the teat itself increases, the teat cup will creep up to the top of the teat and by pressing hard against the junction between udder and teat will occlude the rather narrow channel with its delicate folds of tissue, prevent the last portions of milk being withdrawn and may subject the delicate tissue to such severe strains as to cause serious damage, often followed by infection of the udder. What is now the usual practice is to draw down the teat cups a little towards the end of milking and assist the removal of the last portions of milk by massaging each quarter of the udder by hand with the teat cups still in position. In some cows, the increased flow of milk (for a few seconds only) following this procedure may be seen in the "sight glass"—a small glass witness tube inserted between the teat cup "cluster" and the receiving bucket.

Avoiding Casual Stimuli at Milking-time

As a result of the recent physiological findings, more attention has been focussed on the need for preventing unnecessary disturbance of the cow before and during milking. It is no news to the average dairy farmer that a quiet, contented cow, handled with gentleness, gives more milk than an excited animal, though the knowledge has not always been implemented. Controlled experiments have shown that quite minor disturbances—unfamiliar surroundings or unusual sights, such as the presence of strangers or strange animals at milking-time, or unexpected noises—interfere with let-down or may even prevent it completely. The effectiveness of the conditioned reflex may not only be lowered by small disquiets, but if the disturbance is of sufficient magnitude to cause real anxiety or even pain, then there may be complete inhibition of let-down resulting from the secretion of adrenalin (Petersen). Cows vary very much in their coefficient of irritation, and the art of the good cowman includes a real appreciation not only of the psychological make-up of Dorrit, Flora and Rosebud, but also of changes in their temperamental stability from day to day.

Variations in Udder Anatomy in Relation to Milking

Another important factor in milking rate (if not in the efficiency of let-down) is the variation in the anatomy of the udder from one cow to another. The teat canals in some cows are definitely smaller than in others, not infrequently resulting in "hard milkers"

which require far longer than the average time of milking before all the available milk is removed. Some cows regularly leak milk at milking times, often as a result of defective teat sphincters (or in some cases too easily stimulated let-down reflex). In some udders, the folds of tissue which divide the main collecting cisterns of the gland into several parts are larger than in others, and often continue on into the inner lining of the teat itself. They may in some animals be so large as to interfere with the removal of milk from the gland.

A significant variation of the texture of the teat as between cows that appears to be of importance in relation, if not directly to milking efficiency, then to the risk of loss of milk as a result of udder disease, is the marked difference that occurs in the smoothness or otherwise of the *exterior* of the teat. In some cows the teat is quite smooth, in others fairly deep folds criss-cross over the surface, and in others again there is an intermediate condition. Taylor has shown recently that roughness of teat surface is correlated positively with increased infection rates, both of the skin itself and of the udder, with the micro-organisms causing mastitis. Here then is yet another variable that the farmer who wishes to apply recent scientific findings to the prevention of milking troubles in an animal where udder infection is a constant threat must bear in mind.

To sum up. During the past two decades there have been marked advances in our knowledge of the physiology of the let-down of milk. The major factors concerned in this process seem now to have been established, namely that let-down is a conditioned neurohormonal reflex, initiated by sensory stimuli, usually, but not always, tactile from the udder and teats. The stimuli pass from the sense organs to the central nervous system. Under normal conditions, this causes an immediate liberation into the blood stream of oxytocin from the posterior-pituitary gland, the hormone circulates to the mammary gland, and on reaching the myo-epithelial cells which form a basketwork of processes round each secretory alveolus (which is at that stage, presumably, distended with already-secreted milk) causes these cells to contract. As a consequence, milk is squeezed out of the alveoli into the main collecting ducts and cisterns of the gland and this leads to the rise in milk pressure characteristic of let-down.

It is likely that this general mechanism, with minor modifications, is common to most mammals. Endeavours are being made to apply the new knowledge—already being successfully used as a research tool—to the practical questions of improving milking efficiency in

farm animals and in the human mother, and some progress has been effected.

SOME IMPORTANT REFERENCES

1. Dodd, F. H., and Foot, A. S., *J. Dairy Res.*, **15**, 1, 1947 ; **16**, 14, 1949. *Agriculture*, **55**, 238, 1948.
—— and Henriques, E., *J. Dairy Res.*, **16**, 1949 (in the Press).
2. Gaines, W. L., *Am. J. Physiol.*, **38**, 285, 1915.
3. Hammond, J., *Vet. Record*, **16**, 519, 1936.
4. Korkman, N., *Kungl. Lantsbruks. Tidsk.*, **87**, 161, 1948.
5. Petersen, and others, *J. Dairy Sci.*, **24**, 211, 1941 ; **26**, 752, 1943 ; **27**, 449, 1944. *Physiol. Rev.*, **24**, 340, 1944. *Minnesota Agric. Exptl. Sta.*, 361, 1945.
6. Richardson, K. C., *Proc. Roy. Soc.*, **B.136**, 30, 1949.
7. Tgetgel, B., *Schw. Arch. Tierheilk.*, **68**, 335, 369, 1926.
8. Various authors (Richardson, K. C., Folley, S. J., Waller, H., and others), *Brit. Med. Bull.*, **5**, Nos. 2–3, 1947.

SUN, EARTH, AND ICE AGE

METHODS FOR CRITICISING SUGGESTED CAUSES OF THE QUATERNARY COLD PERIODS

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1. Introduction

Lengthy controversy has failed to decide the physical peculiarities which led to the past glaciation of Britain, Europe, and America. The mapping of ice-cut valleys, ice-scratched and ice-borne rocks, reindeer bones and arctic plant remains, etc., leave no doubt whatever that in the Quaternary geological age the northern ice-sheets penetrated as low as the latitude of the English south midlands. The accurate tracing of such facts has stimulated numerous but unconvincing theories of the cause for this striking episode of our past climate; discrimination between these has not clearly separated the problem of whether a particular agency of cooling existed at all from the problem of whether it was sufficient for its supposed consequences. We are suggesting here some novelty in the method of classifying and criticising glacial hypotheses.

The main requirements, in time and temperature, for any physics of the Ice Age, are as follows. This particular glaciation began a million or more years ago and "ended" between 20,000 and 50,000 years ago, succeeded down to to-day by much slighter climatic fluctuations, whose causes are also obscure. The glaciation itself was subdivided into three or four acute phases, separated by long, warmer interglacial periods when the ice sheets receded. The total duration covers most of human prehistory, but only a minute fraction of the history of fossil life, the Quaternary period being preceded by warm climates covering hundredsfold its own total length; in fact, the only previous instances of severe glaciation in the temperate zones appear in the upper pre-Cambrian (say, 700 million years ago), the Cambrian (say 500 million), and the Permo-Carboniferous (say 230 million)—the latter presenting the specially intriguing problem of ice-scratched rocks in regions now tropical. The amount of cooling, to separate glacial from inter-

glacial or from today's climate, is surprisingly small; estimates in Flint's treatise [1] on the basis of Brückner's and Penck's studies of Alpine snow-lines, suggest that average falls of 5° or 8° C. would be sufficient, according to the kind of region glaciated.

After a full century of explanations offered, it is becoming evident that no one of these alone is likely to account for all the facts. Theories have usually been classified according to whether they locate the cause of the Ice Age near or far, for instance in (*a*) geographical circumstances; (*b*) peculiar states of the atmosphere; (*c*) changes in the whole earth's orbit or axis; (*d*) agencies completely external, such as solar changes. If, however, it has to be decided that two or more of these causes must have reinforced each other, no single one being sufficient, we suggest a reclassification of all possible physical agencies of cooling, according to whether they are (A) known from independent evidence to have been present at the relevant times; (B) conceivable but indecisive or neutral through lack of any evidence for or against; (C) unlikely because of some independent contrary evidence. In assessing the adequacy of any suggested cause of the glaciation, it may become important to grade any theory according to whether an agency of type A is supposed to be assisted by another also of type A, or (as in much of the literature) could only have been effective if combined with some agency whose probability falls in category B or even C. Examples, perhaps, are seen when Simpson's indisputable meteorological argument requires to be accompanied by totally unknown solar phenomena before it can refer to actual history, or when Köppen and Wegener reinforce indisputable astronomy with inadmissible geophysics.

For this grading we propose here to assess some common theories of the cause of glaciation, on the following basis:

A: astronomical theories, where change in inclination of earth's axis, precession, eccentricity of orbit, and a definite limited amount of pole migration can be quantitatively inferred for particular times.

A: geographical theories of land elevation and change in sea-land ratio, qualitatively inferred but difficult to assess in their quantitative effect upon climate.

B: theories of solar variation, either by extrapolation of sun-spot fluctuation, or a hypothetical change in source of energy, or effects due to surrounding nebulosity; in all of these there is no conclusive evidence for or against actual occurrence.

B and C: atmospheric theories of cooling by temporary excess or defect of some substance, particularly carbon dioxide or volcanic dust. The contribution is here neutral, or even becomes negative where the effect is shown to be most likely at the geological epochs least relevant to glaciation.

C: theories of migration of the earth's pole beyond the limited movements known in A to have occurred. The elastic-plastic properties of terrestrial material, the forces available from buoyancy of floating land-masses, and the actual parallelism of snow-lines during the interglacial recessions seem inevitably to place this very popular agency in a rather low category, dangerous to combine with evidence of type A.

In the light of this classification, we now abstract the essential argument of the various traditional theories, in such a way as to suggest a critique of any contribution to be made by combining them. We shall first contrast the A and C categories of astronomical argument, then consider the geographical arguments of A category, before descending through neutral and doubtful solar and atmospheric theories.

2. Changes in the Earth's Axis and Orbit known to have occurred and therefore Evidence in Category A

(i) Inclination of Earth's Axis

The variation of climate according to latitude and to time of year depends primarily on the fact that the axis of the earth's daily spin is not perpendicular to the plane in which the earth

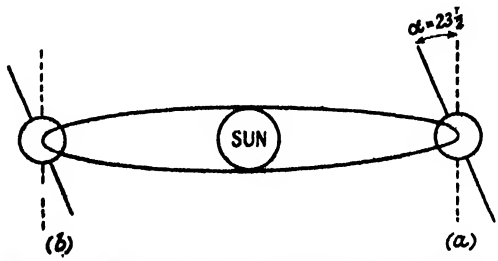


Fig. 1.—Seasonal climates due to earth's axis not being perpendicular to its orbital plane.

revolves annually round the sun. This fact is known as the obliquity of the ecliptic, an angle of about 23½° (α) in the diagram (Fig. 1). (*a*) represents summer in the northern hemisphere, owing to exposure of larger northern than southern areas to sunlight,

whereas in the portion of the year at which the earth is near (*b*) there is northern winter and southern summer. This distinction obviously becomes accentuated the higher the observer's latitude is. In considering past climates at any particular latitude, it is indisputable that increase of the angle α must increase the contrast between summer and winter. But only under particular meteorological assumptions does this favour glaciation, through the cooler winters and in spite of the hotter summers.

There certainly is a small variation in α, due to perturbation of the plane of our orbit by other planets; the fact that the latter are not far from the ecliptic plane precludes the imaginative suggestion that very drastic changes in α are likely. The present variation can be expressed by

$$\alpha = 23° \; 27' \; 8{\cdot}26'' - 0{\cdot}4684'' \; (t)$$

where t is the number of years since 1900, so the angle may well have been 1° 18′ greater than now 10,000 years ago. But the rate of change is not necessarily steady; Langrange estimated a maximum of about 27¾° for 30,000 B.C. and minimum 20¾° about 15,000 B.C.; Newcomb gave 24° 13′ and 22° 35′ respectively 9,000 years before and after his time; Stockwell gave a range of about 22°–24°, with a maximum 24° 17′, in 8,150 B.C. Milankovic's famous climatic calculation was based on an oscillation $22° \rightleftarrows 24\tfrac{1}{2}°$ with a period of 40,000 years.

The effect on climate is obviously not large, unless combined with other phenomena which we consider next.

(ii) Precession and Shift of Perihelion

(*a*) *Precession.*—The earth, in common with all rotating bodies, responds as a gyroscope to an external force. If we had no diurnal spin, the near moon and distant sun would drag the earth's equatorial bulge into the plane of the earth's orbit (Fig. 2), but, just as a spinning top precesses in a slow shift of its axis around the vertical instead of falling down, so the spinning earth's axis drifts around the axis of our orbit. The period of complete revolution is nearly 26,000 years; it shows in the discrepancy between the "sidereal year" measured relative to stars, 365·2564 mean solar days in 1931, and the "tropical year" of seasons measured relative to the position of spring equinox, 365·2422 mean solar days, about 20 minutes less.

The annual angular shift, $50\tfrac{1}{4}''$ at 1900, is subject to a small effect of disturbance by planets, as was the ecliptic angle, but amounting here to only 0·00022″ per year. The accumulated total

has displaced the spring equinox, as known to the Greeks, right out of their constellation Aries, and is even detectable on globes manufactured a century or more ago.

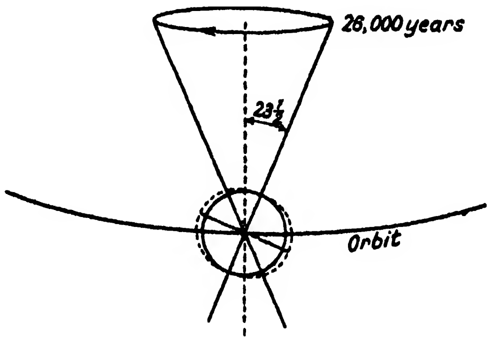


FIG. 2.—Gyrostatic precession due to attraction of earth's equatorial bulge into plane of orbit.

(*b*) *Perihelion.*—The major axis of our annual elliptic path round the sun defines aphelion and perihelion, our positions of greatest and least distance from the sun; but, as seen in Fig. 3, these do not coincide with midsummer and midwinter as decided by the sun's greatest and least elevation in the noon sky. The year as measured by our journey from perihelion to perihelion, the

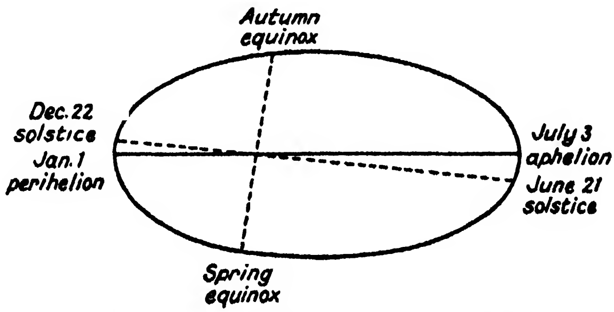
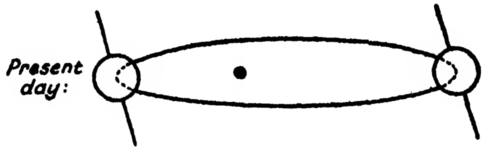


FIG. 3.—Solstices do not coincide with near and far ends of orbit, the lines drifting apart at 61¾″ per year.

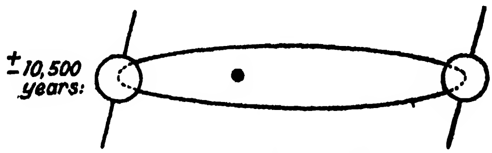
"anomalistic year," is 365·2596 mean solar days, exceeding the sidereal year by about 4½ minutes, there being a shift of the ellipse by 11¼″ compared with the precessional shift of our axis by 50¼″ in the opposite direction. This swing of the apse of our orbit completes itself relative to the stars in a cycle of longitude taking about 108,000 years.

Taking (*a*) and (*b*) together, the line of solstices and the line of

apses are separating at a rate of $50\frac{1}{4}'' + 11\frac{1}{4}'' = 61\frac{1}{2}''$ per year; the consequence relevant here is that the situation of our present northern winter nearer the sun and summer further from the sun will change over to the southern hemisphere in 10,500 years, and return to us in 21,000 years. The contrast between aphelion and



Northern summer in aphelion: least summer to winter contrast.
Southern summer in perihelion: greatest summer to winter contrast.



Northern summer in perihelion: greatest summer to winter contrast.
Southern summer in aphelion: least summer to winter contrast.

Fig. 4.

perihelion is of course exaggerated for the sake of the diagram (Fig. 4), and is actually a $3\frac{1}{2}$ per cent. difference in solar distance, or about 6 per cent. difference in sunlight intensity; this is sufficient to cause considerable climatic change, other circumstances being equal.

(iii) Eccentricity

It is obvious that this distinction between aphelion and perihelion winters will be accentuated, the more the ellipse is lengthened, and will vanish if the ellipse degenerates into a circle. The eccentricity, defined as distance between centre of ellipse and sun at the focus, as fraction of distance between perihelion and aphelion positions of the planet, does vary, with a probable period of about 90,000 years. It is at present 0·0167 and may fall to near zero after 12,000 years. It was probably about 0·077 or four times its present value about 33,000 years ago.

(iv) Migration of Earth's Poles

This is a phenomenon often invoked to explain past climatic crises; but the term can have more than one meaning. Among evidence of category A, which we are here isolating, the change of

the ecliptic angle at the present definite rate of 1° 18′ in 10,000 years is a true shift of the direction of the pole relative to the plane of our orbit. There is also definitely known to occur a small shift, about 0·05″, with two periodic terms of a year and of about 14 months, recognised as a minutely detectable oscillation in latitude, investigated very thoroughly by Kimura and others. The earth's Moment of Inertia can alter slightly, by snow or ice which accumulates on one side of the poles without being completely compensated on the other side. Sir George Darwin showed that such changing discrepancy between axis of "figure" and axis of rotation is essentially confined within extremely small limits.

But there have been many theories of "pole migration," from Schiaparelli (1892) to Hann (1903), and developed especially by Wegener (1924) [2], and some have associated climatic change with very much larger displacements, postulating not a change in the axis of earth's rotation but a migration of the continents about an axis fixed relative to stars or sun. The pole would appear in a fresh land merely because the latter had itself drifted. Such theories are of a quite different origin from anything in category A, and we turn next to examine why they can only claim the degree of validity of B or C.

3. Pole Migrations not independently known, and Evidence mostly of Category C

It would be very pleasing to believe, as some have, that glaciation of a region now temperate merely meant that the North Pole was actually once situated in that part of the world. The most decisive evidence, relegating all such hypotheses of the Quaternary Ice Age to the very low category C, is that during the warmer interludes shrinking the ice sheets the snow-lines which can be traced were never far from parallel to earlier or subsequent positions. Glaciation was not a temporary removal of the polar cold region to some other spot, but a cooling which spread down to lower latitudes than usual, and in any circumstance a pole is not necessarily the centre of glaciation. It is hardly even necessary to reinforce this fact by emphasising the enormous forces which would have to be invoked to shift the direction of earth spin during the Quaternary Age, when the main geographical outline of the preceding time resembled so closely the present day, except for submergence and emergence of land bridges.

To explain by migration of continents the *earlier* glaciations, when the distribution of land and sea was not so like the present, seems not so impossible a task; in particular, Wegener's account

of the Permo-Carboniferous glaciation in territories now tropical, and of hot coal forests in regions now polar, has called for serious scrutiny however irrelevant to the more recent Ice Age. It must be noticed, however, that the most definite evidence for Wegener's migration of continents is the "fit" of Africa to America, etc., suggesting they have drifted across a line of separation common to both coasts; this East–West drift would not have helped Africa to be ice-bound, without a North–South drift for which the corresponding geographical evidence is lacking. There are other possible explanations, one of which we remark later.

It is, of course, fatally easy to account for any glaciation anywhere if we are allowed merely to postulate a change of axis of the required amount in the required direction; but to reach the status of a hypothesis and be of any service to knowledge, the suggestion must carry with it independent evidence of occurrence, and also some not impossible source for the very large forces required for such earth movement. The buoyancy of the aluminium compounds ("Sial") on the magnesium ("Sima") silicates involves forces quite inadequate to rapid continental migrations, whatever use is made of isostatic compensation of gravitation, and Jeffreys [3] has shown conclusively that the plasticity of earth materials cannot warrant supposing small forces to overcome large resistances merely by long duration of their application.

4. Certain and Uncertain Results of the Axis and Orbit Changes

The following consequences of the behaviour of the earth as a planet are definite, with the proviso of other circumstances being equal. In particular, we assume sunlight only alters in distribution and not in total amount, and we neglect for the moment all meteorological and geographical secondary effects. The latter may overwhelm the purely astronomical, recollecting that our winter cold lags long after winter solstice through slow adjustment of the atmosphere, and that our northern perihelion winters fail to reduce the summer and winter contrast compared with the southern hemisphere, because the latter contains more sea areas of high heat capacity. Subject to such overriding, it can be said: (i) the fluctuation of ecliptic angle confers greater contrast between summer and winter at its maximum, with periodicity 40,400 years or 37,400 when precession itself is affected, but with an uncertain range of the contrast; (ii) the present perihelion winters did undoubtedly reverse to aphelion winters in the north 10,500 years ago, giving considerably more acute summer–winter contrast. It must be

remarked that up to about 1920 glaciation was commonly referred to cold winters, *i.e.* the maximum in this contrast, but recent writers have emphasised more the tendency to glaciation resulting from cool summers, *i.e.* the minimum contrast; (iii) larger eccentricity accentuates the aphelion winter effect, shortening the hot summer and lengthening the cold winter. Eccentricity has probably been four times the present value, with likely period of 90,000 years; (iv) except for the effect allowed for under (i), there can be no definite expectation of pole-migration, and evidence against it relegates associated climatic theories to category C.

5. Historical Critique

In the light of these definite but limited results, and having regard to our distinction between evidence of A and C categories, it is useful to list the ways in which these astronomical considerations have been utilised in theories of the Ice Age. The earliest suggestion seems to have been by Adhemar (1832) that aphelion winters might have caused glaciation. The introduction of the ecliptic angle seems to have been by Meech. The first attempt at combining aphelion winters with a time of greatest eccentricity was by Croll (1875) [4], whose methods of calculation were supported by the great contemporary authority of Sir Robert Ball. It was at once objected that Croll's theory, by itself, would give alternating ice ages in northern and southern hemispheres 10,500 years apart through all geological time, whereas these are missing throughout the 200 million years between Permo-Carboniferous and Quaternary, and even in the latter there seems no evidence of a North–South alternation. The most elaborate and careful use of perihelion, ecliptic angle, and eccentricity was by Milankovic (1920, 1930, 1938) [5]. He calculated the latitudes whose normal climate would represent an equivalent of the change inflicted upon certain northern temperate regions when these three periodicities of 21,000, 40,400, and 90,000 combined to reinforce cooling. That is to say, he represented cooling by stating how far north we would travel today to experience an equivalent cooling. The graphs plotted by Milankovic show four groups of periods characterised by cool summers which could prevent ice from melting; many investigators were attracted by the hope of identifying these four groups with the glacial-interglacial oscillations of the Quaternary. The severest critics of Milankovic have been Penck [6], who emphasised that the total time covered by the peaks of Milankovic's graphs is only 600,000 out of the million or more years required by geological evidence, and Simpson [7] who found the resulting temperature

fluctuation too slight to be convincing reason for the glaciation. Spitaler (1921) [8] used similar astronomical periodicities, but included terms allowing for the differing effects over a mainly-land and mainly-sea hemisphere; he depended on the notion, first due to Murphy (1876), that a long cool summer is the decisive factor, *i.e.* an aphelion summer at greatest eccentricity, rather than Croll's aphelion winters. Soergel (1937) [9] and Zeuner (1937, 1945) [10] use similar methods and data, but produce rather different time-scales: actually the assignment of different meteorological emphasis to the several astronomical phenomena can influence considerably the way in which the peaks of the separate periodicities contribute to a total rise and fall of cooling agency—the present writer has found that he can shift the peaks of Milankovic out of all recognition by varying the coefficients of each term in the periodic series.

A very thorough calculation on the Milankovic method was made by Köppen and Wegener (1924) [11]; but the earlier puzzle of why ice ages were missing for so long, while the orbit and axis changes were presumably functioning, was avoided by introducing a large pole migration peculiar to the Quaternary. Given this assumption, which we have here degraded to category C, as supported by no evidence and opposed by a great deal, it was attractive to use the Milankovic periodicities to control the phase of an episode which had really been initiated by pole migration. Recently (1942) Blanchard [12] has attempted another combination of the Milankovic periodicities with pole migration, which has also been severely criticised by Simpson; Blanchard has a novel way of avoiding the total lack of any cause for pole migration, in that he traces it as a *consequence* of the eccentricity, precession, and apse swing.

It is an unavoidable conclusion that the behaviour of the earth as a planet, its changes in orbit and axis, do provide definite agencies for initiating climatic change; but it is also clear that, left to themselves, they are inadequate causes of glaciation, or else too adequate in the sense that they would have provided ice ages more often than actually occurred. The problem of the future is more probably to select the correct geographical, meteorological, solar, and other agencies whose coincidence with the axis and orbit changes enabled them for once to become effective. This necessitates again the classifying of other proposals according to their A, B or C validity.

6. Geographical and Geological Causes of Glaciation

From researches into the state of land and sea and mountains immediately before the Ice Age, it is recognised that uplift of land masses on a vast scale did accompany or closely precede the glacia-

tions, not only of Quaternary but of the earlier Permo-Carboniferous and Cambrian. The association is actually causal, in that the elevating of land masses does produce, other things being equal, some lowering of surface temperature, although it is even harder to decide when this agency becomes quantitatively adequate than for changes of orbit and axis. Elevation (of "Gondwanaland") is one of the features set to explain the Permo-Carboniferous glaciation without the impossible North–South pole migration, but the success is not certain. For the Quaternary glaciation, different investigators have stressed (*a*) the geological facts of uplift and mountain-building; (*b*) the geographical facts of "continentality" or percentage of land compared with water within a given proximity to any area. The two are not, of course, independent of each other.

(*a*) Suggestions that growth of mountain masses is a cooling agency are traceable back to Dana (1856), but the idea by itself is inadequate to explain the oscillating character of the interglacial warmings. In the pre-Cambrian, Cambrian, and Permo-Carboniferous glaciations, the record of uplift is difficult to disentangle under the subsequent deposits; but in the mapping of the Tertiary Age just before the Quaternary there emerges far clearer detail, which is summarised by Flint (1947) [13]. The feature was world-wide, one of the requirements of glacial pre-conditioning rarely satisfied: *e.g.* in North America, from Alaska to southern California, there occurred late Pliocene and early Pleistocene elevations of thousands of feet, and in Greenland and Labrador similar late Tertiary uplifts. In the Scandinavian mountains, and in Iceland and in the Alps, there were risings of 6000 feet. In Asia, Turkestan and the Pamirs, the Caucasus and the Himalayas, and in South America the Peruvian Andes, rose at least 5000 feet in post-Pliocene times, and other uplifts occurred in Australia and New Zealand. Taken altogether, there was an increase in average continental height from below 1000 to above 2500 feet, all occurring in ages not long preceding the Quaternary glaciation and not paralleled by any phenomenon of such scale in any Cenozoic or even Mesozoic times, *i.e.* not until as far back as the last preceding glaciation of the Permo-Carboniferous.

The net amount of late Tertiary uplift is estimated to have lowered surface temperatures by at least 3° C. on an average everywhere, and much more at the high altitudes; this cooling can be regarded as intensified by secondary effects, such as that mountain-formed clouds reflect solar radiation, and mountain-dried air impedes the transfer of heat from equator towards the poles, since it cools the sea by evaporation.

(*b*) Brooks (1917, 1918, 1928) [14] has evaluated the thermal consequences of change in the ratio of land to sea area, and has demonstrated that quite a moderate switch from maritime to continental environment of some areas suffices to bridge the gap between glacial and non-glacial climate. The first of Brooks' methods begins from the work of Forbes (1861) and of Spitaler, in tracing the curvature of isothermal lines around land and ocean areas for different parts of the year, showing how "continentality," or percentage of land distribution in the vicinity, does carry a depression in temperature. Equations were even formulated by Spitaler and by Brooks, expressing with considerable precision how land in low latitudes can be correlated with heightened temperatures, and in high latitudes with lowered temperatures. In a later alternative method, Kerner and Brooks himself analyse the whole earth's surface in constituent sectors, instead of linear isotherms, and treat the results statistically, showing the effects of the opened and closed oceanic channels and the presence of adjacent land to leeward and to windward. For example, detailed climatic consequences can be traced for the emergence of an island large enough to develop anticyclones of its own.

The January temperatures of Europe are found to be much more dependent on sea–land proportions than on solar radiation, the isotherms often running nearly North–South instead of East–West as they certainly would on any purely solar irradiation basis, even in latitude ranges over which the solar factor (the topic of all our astronomical discussion) is steeply altering. In July the land–sea contrast is less effective and is exceeded by the differences in solar irradiation, the isotherms running more East–West than North–South. Detailed analysis of world-wide collections of sample areas yields the following generalisation upon fact:

Winter: effect of land to west lowers temperature except just south of equator.

Winter: effect of land to east negligible except in extreme north.

Summer: effect of land east or west raises temperature but less than in the winter westward effect.

Temperature even at sea is modified by presence of land along other portions of the same latitude.

Brooks' equation for temperature T at any point is

$$T = Z + aL_W + bL_E + cI$$

Z is a zonal temperature for a given land–sea proportion in any latitude ϕ. L_W and L_E are percentages of land in the semicircle W or E of any spot. I is percentage of ice in the total circle.

a, *b*, *c* are numerical coefficients obtained by statistical analysis of the actual observed distributions in the world.

$$Z_{\text{WINTER}} = -70(0{\cdot}95 - \cos\phi)\log L - \frac{\tan^4 \phi L}{100}$$

$$Z_{\text{SUMMER}} = 30(1{\cdot}05 - \cos\phi)\log L + \frac{\tan^2 \phi L}{100}$$

Having established these approximate numerical laws from actual records of temperature and the L "factors of continentality" world-wide over all latitudes, Brooks employed them to estimate the temperature depression during *known* land elevation and extension of the land–sea ratio at definite points in the early Quaternary Age when glaciation was beginning. Even without allowing for reinforcement of cooling by ice after the glaciation had developed, or for fall in temperature by the excess heights of mountains, a July cooling sufficient to permit ice accumulations to survive the summer is readily achieved.

	Jan.	July	
Scandinavia . . .	36	18	(20)
East Anglia . . .	18	13	(20)
Alps	13	9	(11)
Japan	9	3	(7)

The first two columns here are the depressions of temperature (F) calculated by Brooks as due to change in land–sea ratio, and the last column is estimated from geological and biological evidence as actually having occurred.

These figures certainly cover the temperature range demanded (p. 625) for distinguishing glacial from non-glacial, but, since Brooks' equation includes a term denoting the effect of the ice itself, some portion of the cooling is consequential and only some portion is antecedent. It is probable that increase in land area in high latitudes is not always sufficient to initiate the glacial phase if a summer rise accompanies the winter fall at first; but, where glaciation can begin on the high elevations, "continentality" seems to be the factor allowing local high-level ice to develop into an entire regional ice-sheet. For instance, if the Quaternary glaciation in Norwegian mountains began when elevation took large areas above the snow-line, it only spread through Europe because general uplift had recently created a wide extension of land areas. Once the resulting ice-sheet reached a certain size, it became independent of the kind of land covered. There were, of course, many subsidiary

consequences of opening or closing sea channels when uplift altered the land-bridges, among which the famous deflection of the Gulf Stream (once considered responsible for the whole affair) is now given a minor part.

All these geological and geographical changes and their meteorological consequences, being based on observed association of actual facts, seem to deserve the category which we labelled "A"; this was true also of the orbit and axis phenomena, but not of some suggested pole migrations.

7. Solar Changes

The astronomical theories attempting to account for the Ice Age in sections 2, 4, 5, were confined to estimating how change in distributing the illumination over the earth might alter the climate, even if the radiation leaving the sun were perfectly constant, and even if the total radiation reaching the earth as a whole over the year were not altered by any change in the atmospheric blanket. In section 8 we shall relax the latter restriction, and meanwhile we relax the former by considering the possibility of alterations in the sun itself. The arguments have no support from any independent knowledge, and occasionally have indirect opposition, and are therefore of the categories which we termed B and C, so their positive value can be suggestive only.

The early suggestions of connecting the Ice Age with some solar variation came from Hitchcock (1891), Dubois (1895), Arctowski (1908), and Huntington (1914). The two most serious arguments have been (*a*) by Huntington and Vissher (1922) [15] and (*b*) by Sir George Simpson (1934, 1940) [16].

(*a*) This hypothesis was that turbulence of the earth's atmosphere may be intensified around the time of the maximum prevalence of sun-spots, and that, with accentuated maxima, belts of cyclonic storms migrating towards the poles would induce heavy precipitation in high latitudes, or perhaps in equatorial latitudes in the Permo-Carboniferous. The objection to any such theories is that we know nothing to suggest that the sun-spot maxima reached higher peaks in the past; indeed, the persistence of their 11-year periodicity (traceable in Sequoia rings to 1000 B.C., in clay laminations to 10,000 B.C., and even in Australian "varves" of Carboniferous age) would probably not have been maintained if their unknown mechanism were of vastly greater efficiency in the past. Actually the total rate of solar-radiation varies only a very few per cent. over the sun-spot cycle, though the ultra-violet constituents probably vary hundreds per cent.; the latter affect the

ionosphere, or earth's outermost atmospheric layers, very profoundly, but that is just because they are totally absorbed therein and fail to reach the lower air in which all weather originates. Even if we were to intensify hypothetically the ultra-violet fluctuations, we still lack evidence that the rarefied ionosphere has any powerful reaction upon the denser weather-producing regions.

(*b*) Simpson's work is important in that it traces very thoroughly what meteorological consequences *would* follow if the solar temperature were to vary, and must be kept in reserve in case any evidence of past solar fluctuation is ever found. Simpson's analysis has the peculiarity that an *increase* in solar radiation, if it occurred, causes glaciation *via* a stage of increased precipitation, which initiates a cycle of events leading to final cooling with warmer interludes.

In considering whether theories of change in the sun can deserve the status " B," or are of no more value than the merely suggestive and unlikely " C," it must be remembered that most of such hypotheses arose when no more was known of solar evolution than the crudest Giant-Dwarf pictures of a generation ago. Since 1939 Bethe and many collaborators and followers have made it clear that the source of maintenance of solar heat is the transformation of hydrogen to helium ; cooling of the sun can only be the extremely slow process of hydrogen exhaustion, quite irrelevant to oscillating phenomena such as the Quaternary glaciation—unless there had occurred a catastrophic collapse such as is probably implied in a Nova outbreak, which event no terrestrial life would have survived. Oscillation of energy output between successive atomic " fuels " in the helium conversion is quite likely during the early history of a star, but scarcely at the solar stage. Gamow has traced a plausible small increase in solar energy, but, even if proved, it is not reversible in periods as brief as the mere million Quaternary years. In fact, to rely upon intrinsic solar change is to relegate the whole tale to category C in the present state of knowledge, not merely an unknown but a very unlikely foundation.

There is one notable exception to the completely unspecified mechanism of any solar change relevant to terrestrial climate ; in 1939 Hoyle and Lyttleton [17] showed that, if the sun passes through interstellar nebulosity of kinds widely prevalent in " empty " space, the resulting accretion of gas would impose certain small temperature fluctuations. These could be positive or negative under different circumstances, and therefore be a conceivable cause of Carboniferous warm forest or of Quaternary ice. This is perhaps the only extra-terrestrial agency ever based upon a real physical

process; but as the interstellar clouds have a distribution entirely unplotted as yet, the unknown factor is only removed one stage further.

8. Abnormal Atmospheric Absorbents

We have considered in 2, 4, 5, the definite changes in distribution of a constant radiation over different parts of the earth, and in 7 the suggestions as to what might follow any inconstancy in the solar source. An obvious sequel is to ask whether without either kind of change at all our climate might still fluctuate wildly by interposition of some novel atmospheric blanket.

It must be realised that all such hypotheses can be made to yield opposite conclusions: an atmospheric blanket could (*a*) cool the earth by cutting off daylight, but (*b*) could also warm the earth by cutting off the latter's own re-radiation to space at night, or (*c*) neutralise changes in radiation by reflecting or re-radiating back in either direction. Again, transparency and opacity might well be in different parts of the spectrum, just as a glass roof to the horticulturist's greenhouse is permeable to entering sunlight but opaque to the infra-red rays from the warmed interior and so acts as a heat-trap.

The classical investigations start from Arrhenius (1896) and Chamberlin (1899), who considered the effects of any possible changes in the carbon dioxide content of the earth's atmosphere as an absorber of solar radiation, and Humphreys (1913, 1920) [18], Abbot and Fowle (1913), and Arctowski (1915), who considered the effects of widespread volcanic dust. This dust might upset the thermal equilibrium of the atmosphere as a radiation filter, and might thus have originated climatic change in the past.

The most circumstantial suggestion was that the CO_2 in our atmosphere must have increased greatly after times of dense vegetation owing to organic decay, or diminished by exchange in plant life. CO_2 absorbs strongly in the infra-red, giving it the "glass-house" property of preventing the earth from cooling by re-radiation; but in fact H_2O in the *lowest* atmosphere absorbs so strongly in somewhat similar spectral ranges that only in the dry upper layers could fluctuation of CO_2 produce significant alteration in the balance of radiation. The ozone content of the upper atmosphere does fluctuate considerably during solar ultra-violet bursts associated with sun-spot areas; but ozone is a purely sun-produced substance, and to demand long-time fluctuations brings exactly the same objection as to all theories demanding the totally unknown factor of intrinsic solar change. The H_2O content, and the cloud and

precipitation following the ascent of damp air during instability of steep temperature gradients is of course a very potent source of *local* earth cooling and warming, but there seems no ground for supposing a total increase over Quaternary or other time.

The very attractive theory that an atmosphere of dust may have prevailed over certain geological ages, and profoundly affected climate, originated when it became evident that large but single volcanic explosions (Krakatoa, 1883; Katmai, 1912) did spread a world-wide cloud of finely divided solid particles which took several years to subside. The Katmai cloud is alleged to have reduced solar radiation by 20 per cent., so a lowered temperature sufficient to initiate glacial conditions is conceivable if such a dust cloud were maintained for much longer time. Humphreys showed from the optical effects (the Krakatoa sunsets were memorable all over the world) that the relevant dust particles were about 0·00185 mm. in diameter, which would scatter and reflect rather than absorb the main solar radiation, but would scarcely obstruct the terrestrial re-radiation to space.

So long as no particular time in the geological sequence was stated to contain the consequences, these theories are of the category B with nothing to support or disprove them. But on comparison with the geological record there is even a descent towards category C with definitely adverse evidence. For it is as sequel to the Ordovician, Devonian, and early Tertiary that glacial periods due to volcanic dust ought to have occurred; in those ages a large proportion of the land surface of the world was violently volcanic, and materials vastly exceeding our local Krakatoas were actually present. But those times, most suitable to the theory, show no ice age, the late pre-Cambrian, Cambrian, and Permo-Carboniferous being peculiar and isolated sole predecessors of the Quaternary glaciation.

There have been other atmospheric theories; *e.g.* Defant (1924) discussed the decrease of local pressure gradients following the great volcanic explosions of 1883, 1886, 1888, 1902, which tended to replace south-west winds by north-east in Atlantic, Pacific, and European areas. This is a gas effect, not one of dust at all. A high gas *density* in Mesozoic times has even been postulated by Harlé on the intriguing grounds that the huge flying reptiles were too heavy for locomotion without it. But there is no suggestion of any reversal or oscillation in the earth's loss of atmosphere.

In fact, the only region where large changes do actually occur rapidly is the ionosphere, too high and too rarefied to affect climate, which arises in the moist and dense lower atmosphere. It is

extremely unlikely that the solar bursts which do agitate violently the ionosphere ever penetrate to the climatic levels, but it will be important to look for any evidence of secondary exchange effects between upper and lower atmosphere. After that, we shall still require, out of the unknown, evidence that solar fluctuations of the relevant duration and suddenness and intensity have occurred, to disturb the ionosphere more than at present.

9. Conclusion

We have found that the only agencies of glaciation worthy of inclusion in the category A, of causes known to be real occurrences, are the phenomena of earth axis and orbit, precisely timed and quantitatively estimated, and the phenomena of land-elevation and continentality, not precisely timed and only in Brooks' careful work quantitative. The many other theories degenerate into categories B (unknowable) or C (vetoed by known facts), and are at best suggestive speculation and misleading if imported to reinforce "A" theories.

It seems quite possible that the undoubted changes in earth's axis and orbit might be an agency of phasing, whose coincidence permitted the late Tertiary uplift to initiate a glaciation not allowed since the Permo-Carboniferous. Since the Milankovic oscillation seems too rapid, a major problem of the future may be to ask whether long-delayed after-effects, rather than contemporary changes in irradiated area, could exhibit a closer fit to the Pleistocene chronology. If elevation of land, reinforced on this rare occasion by coincidence with after-effects of an optimum in the spin and orbit conditions, really caused this unique ice age, the physics of its origin could be removed one step further back to the agency causing that great Tertiary uplift. The tetrahedral theory of intermittent shrinkage of the earth over geological time may become relevant here, but will require careful and critical application on exact isostatic methods before the chain of circumstances becomes at all clear.

REFERENCES

1. Flint: *Glacial Geology* (New York, John Wiley, 1947).
2. Wegener: *Origin of Continents and Oceans* (London, 1924).
3. Jeffreys: *The Earth*, p. 261 (Cambridge, 1924 and 1929).
4. Croll: *Climate and Time*, 1875.
5. Milankovic: *Théorie mathém. des phénom. thermiques produits par la radiation solaire* (Paris, 1920). See also *Handbuch der Klimatologie*, Berlin, 1930, and *Handbuch der Geophysik*, 1938.

6. PENCK: *Das Klima der Eiszeit* (Vienna, 1936).
7. SIMPSON: "World Climate during the Quaternary Period," *Q.J. Roy. Met. Soc.*, **60**, 425, 1934, and elsewhere.
8. SPITALER: *Das Klima des Eiszeitalters* (Prague, 1921).
9. SOERGEL: *Die Vereisungskurve* (Berlin, 1937).
10. ZEUNER: *The Pleistocene Period* (London, 1945). "Climate of the Countries adjoining the Ice Sheet," *Proc. Geol. Assoc.*, **48**, 379, 1937.
11. KÖPPEN and WEGENER: *Die Klimate der geologischen Vorzeit* (Berlin, 1924).
12. BLANCHARD: *L'Hypothèse du Déplacement des Pôles* (Le Mans, 1942).
13. FLINT: (*loc. cit.*).
14. BROOKS: *Climate through the Ages* (Benn, 1928 and 1949), and *Q.J. Roy. Met. Soc.*, **43**, 169, 1917, and **44**, 253, 1918.
15. HUNTINGTON and VISSHER: *Climatic Changes* (Yale, 1922).
16. SIMPSON: (*loc. cit.*).
17. HOYLE and LYTTLETON: "Effect of Interstellar Matter on Climatic Variation," *Proc. Camb. Phil. Soc.*, **35**, 405, 1939.
18. HUMPHREYS: "Volcanic Dust in the Production of Climatic Change," *Mount. Weather Obs. Bulletin*, 1913.

SPORE DISPERSAL IN RELATION TO PLANT DISEASE AND HEALTH *

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I. Human Pathology and Plant Pathology

Human pathology and plant pathology are very different. In man bacteria are the chief pathogens, but in plants fungi are the principle agents causing disease.

In bacteria the air-borne spore has not been developed. It is true that the bacterial cells responsible for respiratory diseases are projected into the air with the atomised spray discharged from the nose and mouth during sneezing, coughing and even talking. It is true that indoors air-borne bacterial infection may be a serious matter. But bacterial cells are so sensitive to ultra-violet light that they are rapidly killed out of doors.

Fungi, however, in striking contrast to bacteria, have specialised in the production of spores that are mostly air-borne. Indeed, much of the beauty of structure which is a feature of so many fungi is associated with the production and liberation of these spores. Again, although the spores of some fungi do not continue viable for long periods when suspended in the air, nevertheless the spores of many species remain alive and capable of germination for a surprisingly long time, even when subjected to the extreme conditions of illumination and desiccation which they may encounter during transport in the air.

There is an important difference between the attack of a mammal by a bacterial pathogen and the attack of a plant by a disease-producing fungus. The bacterium must gain entry to its host by being inhaled or swallowed or perhaps through a wound, but for the majority of fungi which attack plants the spore need merely be deposited on the leaf of the host. The fungus can then, if conditions for germination are suitable, find its own way in, either by

* Being a Chadwick Lecture delivered in June 1949 at Chelsea Physic Garden.

direct penetration of the cuticle by the germ-tube of the spore or by the germ-tube seeking out a stoma and thus effecting entry. *The plant has a large vulnerable surface ready to receive the air-borne spores of fungal pathogens.*

II. Efficiency of Dispersal in *Phytophthora infestans* and *Synchytrium endobioticum*

The epidemic spread of a plant disease depends on the pathogenic fungus having an efficient type of spore dispersal. This may, perhaps, be illustrated most clearly by considering two well-known diseases of potato: "blight" due to *Phytophthora infestans* and "wart disease" caused by *Synchytrium endobioticum.* Both were introduced from the American continent.

"Blight" appeared in the early forties of last century and spread with dramatic speed when suitable August weather conditions—a succession of warm muggy days—obtained. At that time Plant Pathology had not emerged from the Dark Ages, but, even if the knowledge we now have had been available then, little could have been done to stop the spread of the fungus. Its method of spore dispersal was too efficient. Crops of conidiophores are produced on the diseased foliage and the spores are easily liberated and carried by gentle breezes to infect nearby plants. Thus the epidemic spread of "blight" is associated largely with the air-borne dispersal of the spores, although also with the speed of completion of the life-cycle and the ease with which infection of the foliage occurs under damp, warm conditions.

"Wart Disease" made its appearance in this country early in the present century, but luckily by that time Plant Pathology had grown into a vigorous science and so the disease was brought under fairly complete control. It was possible largely to prevent the spread of the causal fungus, because it does not possess an efficient means of dispersal. It does not produce air-borne spores. The fungus attacks the underground parts, mainly the tubers. From the fungus in the diseased tubers minute motile zoospores are formed which can be dispersed only in the soil water. In addition, relatively large black thick-walled resting-spores are formed which are finally liberated into the soil when diseased tubers decay. The spread of the disease from one district to another is brought about largely by the transport of slightly diseased tubers containing resting-spores. Thus by preventing the movement of infected seed tubers and by insisting on the use of immune varieties in contaminated areas the disease has been successfully checked.

III. Dispersal by Means other than Air-borne Spores

Although typically the fungi are dispersed by air-borne spores, there are a number of other types of dispersal of importance in certain diseases. These will be briefly considered.

A number of pathogenic fungi are spread by insects. A familiar example, though of little economic importance, is the anther smut of campions and pinks caused by *Ustilago violacea*. In the White Campion (*Lychnis alba*) there are separate staminate and pistillate plants. When a "female" plant is infected by the smut the mycelium penetrates throughout, but causes little disturbance except in the flower where the staminal rudiments are stimulated to develop. Stamens are produced whose anthers dehisce to liberate violet smut spores in place of the much larger yellow pollen grains. The sticky smut spores are dispersed to healthy flowers just as the pollen grains are distributed in the normal course of pollination. Smutted flowers of White Campion are extremely common in this country.

Another example economically more important is the Dutch Elm disease caused by the joint activities of the fungus *Ceratostomella ulmi* and the elm bark-beetle *Scolytus*. The fungus fructifies in the brood-chambers of the beetle just below the bark of the trunk and larger limbs of the elm. When the young beetles emerge they are contaminated both internally and externally with the sticky spores of the fungus. These insects then proceed to feed on the young twigs of healthy elms, so introducing the parasitic fungus.

Again a number of pathogenic fungi form slimy spores unfitted for air dispersal and are normally spread by rain splash. This occurs in the anthracnose of Dwarf and Runner Beans caused by *Colletotrichum lindemuthianum*.

Further, many plant diseases caused by fungi are seed-borne. This is especially true of the smut diseases of cereals. In loose smut of oats caused by *Ustilago avenæ* brand spores from infected heads adhere to healthy grains during threshing. Unless this contaminated grain is treated with a fungicide before sowing, the brand spores germinate at the same time as the oat grains and infected plants result. In *Ustilago tritici*, the loose smut of wheat, the story is somewhat different. The smutted heads appear early when the healthy plants are in flower. The brand spores, wind-borne to the stigmas of healthy flowers, germinate at once, producing germ tubes which cause slight infection of the developing seeds without impairing their viability. Thus the fungus overwinters as a dormant mycelium in the infected grain. Here a fungicide applied to the

surface is useless, since the parasite is within the grain, and the only method of control is treatment with hot water, taking advantage of the fact that the fungus is more susceptible to heat than is the wheat embryo itself.

Again, most of the serious diseases of flax are seed-borne, and during the last war a great saving was effected by careful inspection and disinfection of flax seed, which greatly reduced the incidence of disease.

However, in spite of the many exceptions, fungal diseases of plants are typically air-borne, and it is this type of infection with which we are mainly concerned here.

IV. Study of Spores in the Air

Before the beginning of the eighteenth century fungi were considered as mere by-products of corruption, without the power of reproduction, arising spontaneously from decaying organic matter. It was the great Tuscan botanist Micheli who, in the early days of the eighteenth century, established the "seed theory" of fungi on solid ground. He demonstrated "seeds"—or as we should call them nowadays "spores"—in a great range of fungi. However, the idea of spontaneous generation was slow to die, and not until the brilliant researches of Pasteur in the latter half of last century was it firmly established that spontaneous generation is certainly not an everyday occurrence. The work of Pasteur turned the minds of many workers towards the problem of air-borne spores, and since his time many observations on the spore content of the air have been made.

In the past two decades spores in the air have been considered largely from the point of view of plant pathology on the one hand and inhalent allergy on the other. It is necessary briefly to consider, in broad outline, the methods employed in this work—methods which have recently been fully reviewed by a Committee of the National Research Council of America.

At the present time there are two methods widely used in studying the micro-organisms of the air—the aerial plankton. The first is the gravity-slide method, in which a horizontal sticky slide is exposed for a given time and the deposit examined directly under the microscope. Here the fundamental difficulty is one of identification. Rarely are specific determinations possible and the genus, family or even class, may be impossible to ascertain when the spores, like most fungal spores, are small, unicellular, hyaline and spherical or oval. However, in some genera the fungal spores are large and of an unmistakable form, such as those of *Helminthosporium*

and *Alternaria* and the uredospores of rusts. In this connection it is of interest to note an early investigation by Cunningham in 1873 who made—to use his own words—" a systematic and prolonged series of observations carried on within the two large jails in Calcutta with a view of determining, if possible, whether there were any connection traceable between the prevalence of any specific bodies in the atmosphere and the occurrence of particular forms of disease." From that point of view his work was essentially a failure, but from the point of view of the study of spores in the air his work was outstanding, and he gives careful figures of the spores deposited on his greased slides which were exposed vertically in an aeroscope so that the wind always impinged directly on the sticky surface. A number of the spore types figured by Cunningham can be identified, but the majority cannot.

It should be noted that the allergist concerned with pollen grains in the air has not this great difficulty of identification, since pollen grains tend to have characteristic surface markings which make identification of the genus at least a fairly simple matter.

The second method, widely used in studying the micro-organisms of the air, involves the use of a petri dish containing sterile nutrient agar, which, after brief exposure to the air, is incubated for several days to allow trapped spores to germinate and grow into visible colonies. The principal objection to this method is that only a few of the trapped spores give rise, in the space of a few days, to colonies that can be identified. Obligate parasites, especially the rusts, downy mildews and powdery mildews do not figure in counts obtained by this type of method, and many slow-growing saprophytic fungi, especially the toadstools and their allies, do not develop quickly enough to be identified before they are swamped by more vigorous moulds. Again the results obtained depend on the composition of the culture medium used, since one type of medium favours the growth of certain species more than others.

V. Dispersal around a Centre

During the past twelve years plant pathologists have given considerable attention to the question of the dispersal of air-borne spores around a centre of liberation in an attempt to define the size of the danger zone around infected individuals or groups of plants. It is clearly of importance to know, if possible, how close to a centre of infection it is safe to plant a susceptible crop.

A major contribution to this subject was made by the Russian worker Stepanov. In one experiment he liberated slowly through gauze at a certain point approximately 1,000,000,000 spores of

the smut *Tilletia caries* at a height of 1 m. above the ground with the wind varying from 0 to 4 m. per sec. The spores were trapped at different distances from the source in the general leeward direction. The results of his experiment are given in Fig. 1.

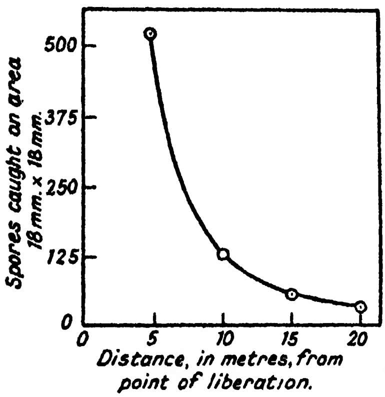

FIG. 1.—Dispersal of *Tilletia* brand-spores.
(*Data from* STEPANOV.)

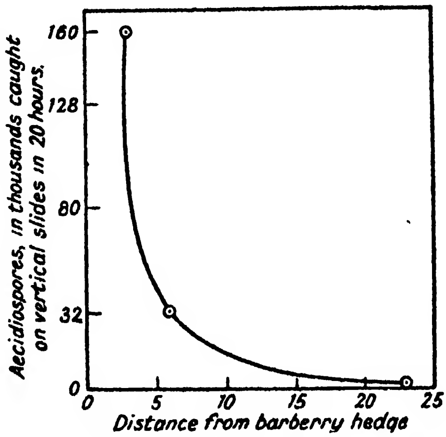

FIG. 2.—Dispersal of æcidiospores of *Puccinia graminis*.
(*Data from* LAMBERT.)

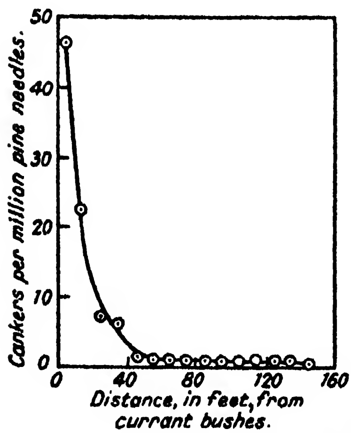

FIG. 3.—*Cronartium ribicola*. Cankers formed on pine needles near heavily infected currant bushes.
(*Data from* BUCHANAN *and* KIMMEY.)

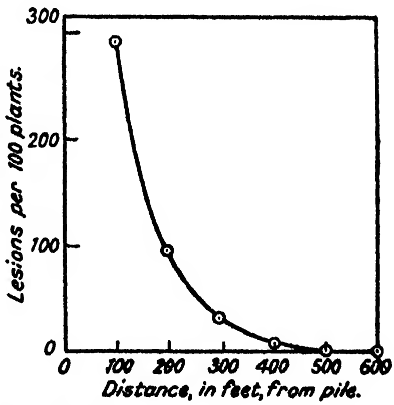

FIG. 4.—*Phytophthora infestans*. Blight in potato field near infected potato refuse pile.
(*Data from* BONDE *and* SCHULTZ.)

The striking feature of Stepanov's work was the demonstration of the very steep gradient in spore concentration on receding from the source of supply. This seems to be a characteristic feature of wind dispersal. Data given by Lambert for æcidiospores caught on vertical greased slides at increasing distances from a barberry

hedge heavily infected with *Puccinia graminis* tell the same story (Fig. 2). In general, it is clear that even if spores at the centre are liberated by the thousand million, the number falling per square metre a mile away may be almost nil.

Again the same dispersal pattern is evident if we consider the degree of infection of plants at increasing distances from a source of air-borne inoculum. Thus the currant rust *Cronartium ribicola* has pine as its other host. Buchanan and Kimmey found that on pines around heavily infected currant bushes the percentage of infected needles fell off so rapidly with distance that beyond 60 yards from the bushes the pines were infected to a negligible degree (Fig. 3). Potato blight may afford a similar picture. In parts of the U.S. the first infection of potato fields by *Phytophthora infestans* is due to conidia developing on shoots derived from diseased tubers on old potato refuse piles. A study of the primary infection in a potato field adjoining such a pile showed clearly that the number of primary blight lesions per 100 plants decreased rapidly with distance (Fig. 4). Many similar examples could be quoted.

The theoretical basis of spore dispersal in the air has recently been considered in detail by a number of workers. The rate of fall of fungal spores in still air varies from 0·05 cm./sec. for the minute spores of *Lycoperdon pyriforme* (4·2 μ diam.) to 2·8 cm./sec. for the large elongated conidia of *Helminthosporium sativum* (24 $\mu \times 68 \mu$), but for most fungal spores the rate of fall is of the order of 1 cm./sec. It has been calculated that if a spore of *Ustilago zeæ* fell by its own weight after attaining a height of one mile in the air and if the wind blew at 20 m.p.h. it would be carried to a distance of about 2500 miles in the nine days of its descent to the earth. A number of similar calculations have been made, but they seem to be of little or no value. It is quite clear that individual spores can be carried for hundreds or even thousands of miles by strong winds, but it is doubtful if the rate of fall in still air has much to do with the distance of transport, and what is of primary importance to the student of plant pathology is not the occasional transport of individual spores, but rather the normal distance of effective dispersal.

Since the wind is so capricious in its behaviour it may well be doubted if any generalisations can be made about dispersal, but Gregory points out that "the problem is statistical because, whilst the destination of a single spore in a wind eddy is inscrutable, the average distribution of vast numbers of spores over a uniform area and over a period of time offers hope of rational treatment."

Gregory has applied the eddy diffusion concepts of modern meteorology to the study of spore dispersal and has developed an elaborate mathematical formula to describe dispersal around a centre. In this formula the constants are physical ones and not arbitrary. Further, the results of experiments on dispersal show reasonable agreement with the theoretical values. Without going into the mathematical argument we might notice that a picture of wind dispersal can be obtained by watching the smoke-trail drifting away from a factory chimney. It tends to be conical, with the apex at the chimney's mouth, and the cloud is visibly turbulent. At the periphery, eddies are continually mixing the smoke cloud with smoke-free air, thereby increasing the width of the cloud and diluting it. On any cross-sectional plane the average concentration of smoke particles is inversely proportional to the square of the distance from the source.

The principal result emerging from a study of wind dispersal from isolated centres is that, on the whole, dispersal is essentially local, the concentration of spores at any considerable distance from a source being very low indeed. Thus the plant pathologist finds that the practical safety zones around centres of infection are remarkably small. However, there are many careful observations that point to occasional long-distance infection and in this connection the cereal rusts call for special attention.

VI. Long-distance Dispersal

During the past quarter of a century the epidemiology of Black Stem Rust of wheat caused by *Puccinia graminis* var. *tritici* has been extensively studied, chiefly by Stakman and his co-workers in U.S.A. and by Craigie and his assistants in Canada. This disease of wheat is probably the greatest single menace to the food supply of the world. Although *P. graminis tritici* is such an important disease on the American continent, it is practically of no importance in Britain, since the alternate host—the common barberry—is so rare, and further the repeating stage on wheat—the uredospore stage—cannot survive the winter temperatures in this country.

On the continent of North America there is a more or less continuous belt of wheat cultivation spreading northwards from Mexico through the Mississippi valley to Western Canada. In Western Canada the first infection of the wheat in early summer may be by æcidiospores from local barberry bushes, by uredospores which have survived the winter or by air-borne uredospores coming from the far-away warmer south where the fungus develops earlier.

Barberries are rare in West Canada, the coldness of the winter normally prevents the survival of uredospores over the winter, and it seems, therefore, that the early primary infection of the wheat is largely due to uredospores blown from the wheat fields far to the south.

In connection with the epidemiology of black stem rust in Canada, Craigie has made a long-period study of the uredospores of *P. graminis* in the air at a number of places in Western Canada. The technique used in each test was to expose a vaselined slide for two days in a spore-trap with a wind-vane attachment, so that the vertical sticky surface of the slide was continually presented to the wind, and to count the number of spores deposited on 1 square inch of sticky surface. It was found, for instance, that viable spores occurred in the air of Manitoba for a week or two in each year *before* any infections could be seen in the field. Sometimes the vaselined slides showed sudden, but temporary increases in uredospore concentration, constituting the so-called "spore showers". Such a shower was usually followed, after a necessary incubation period of one or two weeks, by the development of rust on the wheat in the neighbourhood.

It is of interest to consider an unusually early and rather spectacular example of a spore shower which occurred in 1929. The records for the period 12 June to 28 June, 1929, are given in the following table (after Craigie).

Date.	Number of Stem Rust Uredospores caught each Two-day Period on 1 square inch of Sticky Glass Surface at—		
	Morden.	Winnipeg.	Brandon
June 12 . . .	0	0	0
„ 14 . . .	0	0	0
„ 16 . . .	24	133	0
„ 18 . . .	174	193	68
„ 20 . . .	0	0	0
„ 22 . . .	0	0	0
„ 24 . . .	0	0	0
„ 26 . . .	0	0	0
„ 28 . . .	15	1	0

The actual spore shower occurred between 16 June and 18 June. From 12 to 19 June a south wind prevailed. At this time the local wheat was free from rust. The evidence suggested that the uredospores came from a region of the Mississippi valley at least 500 miles away.

The long-distance dispersal of rust spores in America raises the

question of whether it is different in kind from the local dispersal around a centre, which has already been discussed. It is difficult to answer this question with confidence, but it should be noted that the uredospores certainly do not come from a "point source," but perhaps from an area of rusted wheat several thousands of square miles in extent.

VII. Vertical Dispersal

In considering gradients of spore concentration on receding from a source attention has been focused on horizontal gradients, but, as we would expect, there are also vertical gradients. These have been investigated by collecting spores on sticky surfaces exposed from aeroplanes operating at different heights. The pioneer work on the spores of the upper air was undertaken by Stakman and his colleagues in 1923. He found that the greater the altitude the fewer the spores, and this has been borne out by later workers.

Two concrete examples may be given. The fungus *Gymnosporangium juniperi-virginianæ* attacks cedars forming galls from which, following rain, basidiospores are discharged in enormous number. From an aeroplane flying at about 85 m.p.h. over a heavily infected area of red cedar, petri dishes containing agar were exposed for a standard time. These dishes were later examined and the trapped spores were counted. The results are recorded in the following table (data from MacLachlan):

Height.	Spores caught per Petri Dish.	Average.
100 feet	12, 29, 20, 17	19·5
500 "	8, 12	10·0
1000 "	2, 2	2·0
1500 "	0, 0	0
2000 "	0, 0	0

Again Craigie exposed greased slides in Manitoba over wheat-fields heavily infected by stem rust. Each slide was exposed from

an aeroplane for ten minutes and later the number of uredospores caught on 1 square inch was counted.

Some of his results are shown in this table:

Date.	Number of Uredospores per square inch intercepted by Microscope Slides at Different Heights: 10-minute Exposure.			
	1000 feet.	5000 feet.	10,000 feet.	14,000 feet.
August 5, 1930 . .	24,200	7560	108	10
„ 14 „ . .	24,000	170	36	30

These vertical gradients of spore concentration are of considerable interest, since they help to explain the efficacy of certain physical barriers to the spread of air-borne disease. Thus a mountain chain may protect an area of wheat from uredospores originating in a neighbouring country, since the vast majority of the spores, being in the lower levels of the air, are precipitated on the mountain-sides.

The long-distance dispersal of the uredospores of Black Rust in America naturally raises the question of the possible intercontinental spread of plant diseases by air-borne spores. For example—are viable spores air-borne from America to Europe and *vice versa* ? This is still an open question, although the available evidence suggests that the Atlantic acts as a fairly complete barrier to the spread of air-borne plant diseases. Observations on the spore-content of the air, based on collections made at the masthead of a ship, suggest that the concentration of spores in the air falls off very rapidly away from land, and over mid-ocean the air is free or almost free from fungal spores, although it contains many bacteria of marine types flung into the air no doubt in spindrift from the crests of the waves.

Although it remains a possibility that viable spores of plant pathogens might be blown from America to this country, there can be no doubt that Man has been the chief agent in the intercontinental spread of plant diseases. Many pathogenic fungi have been introduced into Britain with infected plant material and a constant vigilance must continually be exercised to prevent further introductions.

VIII. Spore Dispersal and Inhalent Allergy

The dispersal of air-borne spores belonging to pathogens of crop plants is indirectly of great importance for the well-being of Man,

but air-borne spores have a more direct connection with human health.

It is in the field of inhalent allergy that spore dispersal becomes of interest to medicine.

The air-borne microspores or pollen grains of flowering plants have long been known as the cause of hay-fever. In North America this disease is particularly prevalent and its control has received considerable attention. There are two basic principles of an allergic reaction such as hay fever. First, that one person may be susceptible to one or more types of pollen but not to others, individuals differing in the pollen types to which they react. Secondly, the severity of the attack depends roughly on the amount of irritant pollen inhaled.

It is very easy to find out the type or types of pollen to which a person is allergic and, once that is known, a temporary immunity can be built up by the injection in gradually increasing amount of extracts of the pollen involved.

In America for each city and for each important resort the picture of the pollen content of the air throughout the year has been established. It seems that each year this picture is repeated at any given locality with only very minor variations. The necessary data are obtained by exposing greased slides daily on top of a large building and recording the number of grains of each species caught on a standard area in twenty-four hours. As already mentioned, due to their beautiful surface sculpture, the grains of the different species are fairly readily recognised. Wodehouse gives a diagram of pollen prevalence for a town in New York State. Elm pollen which may cause severe hay fever in some people becomes abundant in early April, followed in May by oak, another hay fever plant. Those who suffer from the disease in the summer are mainly people allergic to the pollen of various grasses, whereas late-summer hay fever is due in the main to pollen of the American ragworts (*Ambrosia* spp.).

It was not until 1930 that fungal spores were clearly proved to produce allergic reactions in the same way as pollen grains do. They are inhaled, and a chain of reactions is set up resulting in nasal catarrh and possibly asthma as well.

In America the study of air-borne fungal spores by allergists has been organised on a nation-wide scale. It seems that quite a high proportion of asthmatics may be susceptible to mould spores, particularly those of *Alternaria*. Species of the mould *Alternaria* are very common on decaying vegetable matter, especially on the straw of wheat and other cereals. The spores, produced in enormous

numbers, are dry and easily liberated into the air. Further, they are of a characteristic form which makes easy their identification when caught on an exposed sticky slide.

Recently the study of the prevalence of *Alternaria* spores in the air has been undertaken by Hyde and Williams in the Cardiff district. They have published detailed observations extending over two seasons and reach the conclusion that "the atmospheric concentration of *Alternaria* spores is sufficiently high to create a presumption that *Alternaria*-spore allergy exists among the human population, and that catarrhal or asthmatic attacks occurring either during the late summer, especially near wheatfields, or at any other time near the scene of wheat threshing may fairly be suspected of having some such origin."

Thus what we may call the phytoplankton of the air has a significance for Man from more than one point of view, and workers the world over are now concerned in its study. But we may be permitted to question the wisdom of the Americans in heralding the birth of a new science—Aerobiology. We may admire their enthusiasm but still let us hope that it will be a long time before a "Journal of Aerobiology" appears in the World List of Scientific Periodicals.

REFERENCES

Bonde, R., and Schultz, E. S., *Bull. Me Agric. Exp. Sta.*, **416**, 1943.
Buchanan, T. S., and Kimmey, J. W., *J. Agric. Res.*, **56**, 9, 1938.
Craigie, J. H., *Sci. Agric.*, **25**, 285, 1945.
Cttee on Apparatus in Aerobiology, *Phytopath.*, **31**, 1941.
Cunningham, D. D., *Microscopic Examination of Air*, Calcutta, 1873.
Gregory, P. H., *Trans. Brit. Myc. Soc.*, **28**, 26, 1945.
Hyde, H. A., and Williams, D. A., *Trans. Brit. Myc. Soc.*, **29**, 78, 1946.
Lambert, E. B., *Phytopath.*, **19**, 1, 1929.
MacLachan, J. D., *J. Arnold Arb.*, **16**, 411, 1935.
Moulton, F. R. (editor), *Aerobiology*, Washington, 1942.
Stackman, E. C., *et al.*, *J. Agric. Res.*, **24**, 599, 1923.
Stackman, E. C., and Christensen, C. M., *Bot. Rev.*, **12**, 205, 1946.
Stepanov, K. M., *Bull. Pl. Prot. Leningr.*, *Ser. 2* (*Phytopath.*), *No. 8*, 1935.
Wodehouse, R. P., *Pollen Grains*, New York, 1935.

PHYSICO-CHEMICAL ASPECTS OF THE CARBONISATION OF COAL

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Carbonisation has been defined as the "gradual progress of a carbon compound towards carbon under the influence of temperature" [1]. Almost any carbon compound can thus be the raw material for a carbonisation process, but in practice attention is usually confined to carbohydrate materials, *e.g.* cellulose, especially the more complex forms present in plant tissue, wood and, in particular, in the partly decomposed product, coal. Coal itself is an "omnibus word" [2] used to denote a wide range of substances (*cf.* an earlier article in Science Progress [3]) and we shall be concerned here to deal rather generally with the phenomenon of the carbonisation of coal.

Analytically, the three important elements in coal (and in all other substances carbonised) are carbon, hydrogen and oxygen. During carbonisation most of the oxygen and hydrogen, together with some of the carbon, is removed as volatile matter, leaving a solid residue, the coke, which contains a high proportion of carbon (80–90 per cent. on a dry, ash-free basis). Although this appears to be rather simple analytically, the structural complexity of coal makes its disruption very involved and the elucidation of the precise nature of all the stages of carbonisation is thus a matter of great difficulty.

Essentially three kinds of change can be recognised, which take place more or less simultaneously: (1) molecular reactions in which volatile matter is liberated; (2) chemical re-arrangements in the remaining solid, brought about by the heat treatment necessary to drive off the volatiles; (3) physical changes in the assembly of the larger units (crystallites, colloidal particles, fluid material, etc.), which determine the physical properties of the final residue, the coke. Geometrically, these changes can affect apparent density, pore structure and the extent of the internal surface. The nature of the linkages between the units particularly affects the hardness of the

coke. The nature of these changes and the effects of alteration in the carbonisation treatment are usually deduced from studies of the properties of the coke and of the volatile matter which is driven off. The most successful results have been obtained by considering these together, but many workers have concentrated wholly or mainly on one product. Thus as a result of a large amount of work on the nature of the volatile products of carbonisation it has been established that different types of product are formed in the various ranges of temperature through which the coal is heated. These are summarised in the following table (temperatures which have been quoted for the appearance of specific products are given in brackets):

Temperature.	Process.	Main Products.
Below 200° .	Removal of occluded gases	H_2O, CO_2, some CH_4, etc.
Above 200° .	Distillation of waxes and resins	Waxes and resins
300°–600°. .	Main decomposition of coal substance	
(225°–350°)	. . . .	Saturated and unsaturated oils
(above 250°)	. . . .	H_2S, mercaptans, thio-ethers
(300°–320°)	. . . .	Water and unsaturated neutral oxygen compounds
(325°–375°)	. . . .	Resins and unsaturated compounds
(300°–500°)	. . . .	Nitrogenous compounds, including NH_3 and N_2
(Above 400°)	. . . .	Primary gas, paraffins and olefines, primary tar; appreciable evolution of H_2
(600°) . .	. . . .	Further elimination of C_2H_6 and sudden increase in evolution of CO and H_2
600°–1000° .	Further removal of O and H from carbon skeleton	CO, H_2
Very high temperatures		H_2 only

(*Cf.* also a table in a comprehensive review of "The Constitution of Coal," by Horton, Randall and Aubrey [4].)

These qualitative observations can be correlated with the operation of fairly well-defined processes. The resins are said to arise from the "liquation" of resins and waxes "adventitiously" present in the coal [4]. The evolution in large quantities of paraffins and olefines (with perhaps some oils and water) between 300° and 500° represents the decomposition of the main coal substance. The decomposition is two-thirds complete at 500° and it will be seen later that 600°–700° represents a temperature above which primary decomposition does not occur. For any further "carbon-isation," a second, high-temperature stage of rather different chemical nature is required.

Primary tar contains, in addition to waxes and resins, some hydro-aromatic compounds, but few benzene homologues, although it does contain phenols and aromatic bases (aniline, pyridine, etc.). Wheeler [5] suggests that the phenols are formed from cellulosic constituents of coal which do not give rise to a great deal of liquid distillate. The resins give rise to paraffins, olefines and naphthenes, but no phenols. Tar which does not differ appreciably from primary tar is produced in low-temperature carbonisation, in which the temperature does not exceed 600–700°. Ordinary coal-tar as produced in gas-works practice is a product of high-temperature carbonisation, in which some of the constituents of the primary tar are decomposed at temperatures above 600°. Aromatic hydrocarbons can thus be formed, either by dehydrogenation of hydroaromatic compounds or by condensation of olefines liberated from higher unsaturated compounds. In some high-temperature retorts the decomposition of primary tar at the walls is severe and results in the formation of "retort carbon." Lessing and Banks [6] have shown that the yield of retort carbon decreases, with corresponding increase in the coke yield, if a number of inorganic salts are added to the coal. These substances are thought to catalyse the polymerisation of volatile matter, to form solid products, before it can escape from the coke as primary tar. Further details of the substances found in low-temperature tar and of the secondary products formed in high-temperature carbonisation are given by Lowry [7, 8, 9] and Spencer [10]. (It must be emphasised that this description refers to a typical bituminous coal. Although other coals follow the same general scheme, the amounts and nature of the products obtained at each stage vary a great deal according to the rank of the coal.)

Cobb and co-workers have made a quantitative study of the formation of primary tar [1, 11]. By heating various coals *in vacuo* and allowing the charge to remain at each of a series of temperatures until gaseous evolution ceased, they have investigated the nature and amount of the products of each hundred-degrees' range. This technique reveals what are thought to be the main products of the primary decomposition, since they are removed at the temperature of their formation before any secondary reactions can take place at higher temperatures. The gaseous and solid products have been investigated, but no analyses have been made of the liquid distillates. For a typical bituminous coal the proportions in the three states are (totals up to 1100°): solid 61·5 per cent., gaseous 21·4 per cent., liquid (by difference) 17·1 per cent. They find that of the gaseous products, paraffins account for nearly all the hydrocarbon fraction and are almost completely removed before a temperature of

600–700° is reached. Evolution of carbon monoxide and hydrogen begins at 500° and becomes very pronounced between 700° and 900°, falling off at higher temperatures.

Anthracite shows similar trends, but the volume of all gaseous products except hydrogen is consistently less than that for a bituminous coal; the solid residue is correspondingly greater.

One observation has been made on the evolution of liquid products by these workers. They have shown that no liquid products are formed at temperatures higher than 600°. Together with the change in the nature of the gaseous products at this temperature, the result suggests that the process of carbonisation consists of two different major stages which are prominent below and above 600° respectively. These can be pictured provisionally as:

(*a*) a preliminary removal of volatile matter followed by thermal decomposition of the essential coal matter. The coal substance consists of complex compounds of high molecular weight which are broken down into a number of products of varying complexity. These then distil away from the coal, leaving a solid residue which is essentially a carbon skeleton to which small amounts of hydrogen and oxygen are chemically bound:

(*b*) further removal of hydrogen and oxygen. In the coke remaining after the first stage, however, the hydrogen and oxygen are distributed only over the surface of the carbon skeleton; consequently this stage is not a major disruption of the coal substance. They are so firmly bound, however, that violent heat treatment is needed to remove them and when removed they give rise to small molecules only. Hence there are no liquid products of this stage, but there is considerable gaseous evolution (particularly of hydrogen and carbon monoxide).

This broad division is supported by the results of investigations on the cokes which remain after each stage of carbonisation. Up to 600° the true specific gravity of the coke material varies according to the nature of the substance carbonised. During this stage, however, it increases progressively, and for all substances reaches the same value of about 1·4 at 600°. It continues to rise at higher temperatures, but is no longer dependent on the nature of the raw material. It therefore appears that some definite stage is reached at 600° which represents the reduction of all starting materials to something approaching a common structure.

Using similar carbonisation technique and combining chemical investigation with X-ray examination of the cokes, H. L. Riley and co-workers have described the micro-crystalline changes which occur during each stage of carbonisation. Their work began with the

relatively simple case of the carbonisation of cellulose, the simplest single substance at all closely related to coal. The X-ray photographs of the cokes made from cellulose show that the progress of carbonisation is accompanied by the formation of aromatic ring systems. These become graphitised and the hexagon layer planes so formed are oriented into crystallites. The number of layer planes in the crystallites remains constant after formation, at an average of 3·6, and as no great change in size of the crystallite takes place in this dimension (after the crystallites have once been formed) " it seems that this may represent the actual rather than an average value " [12]. On the other hand, the layer planes extend laterally very rapidly during carbonisation up to about 700°. From 700° to 1200° growth is small, but between 1200° and 1500° there is a further but smaller sharp rise and then up to 2000° very little further growth [12, 13]. The first stage of carbonisation of cellulose thus appears to be the aromatisation of the original carbohydrate material, followed by a partial breakdown of the resulting matter to form a graphitic skeleton of carbon. This continues to grow by incorporating attached matter into the hexagon system as it is aromatised.

Similar results are shown by glycine and lignin, except that for the latter there is no sharp rise at 500°. This is in accord with the initial structure of lignin, which is known to be aromatic. It is thought that the smaller growth at 1200–1500° may be due to the combination of carbon residues left mobile by the expulsion of oxygen, hydrogen, nitrogen and sulphur atoms chemically bonded into the structure, perhaps in the form of heterocyclic rings.

From the dimensions of the crystallites and the carbon, hydrogen and oxygen contents of the cokes, Riley deduces possible arrangements of the remaining residues, which he assumes to be bonded to the carbon atoms at the edges of the hexagon planes. These are complex for low-temperature cokes, but as carbonisation proceeds some become incorporated into the layer system and some are removed as molecules of varying complexity, until only the simplest radicals remain attached to a hexagon system which is essentially complete. (In Reference 14 will be found diagrams illustrating this process.) At 500°, therefore, the main volatile organic product is methane; above 700–800° the side-chains have been almost completely removed and only hydrogen and oxygen are left.

The same general processes take place when coal is carbonised, but with more complicated detail, corresponding to the more complex nature of the coal substance. For two bituminous coals it has been shown that lateral growth of crystallites is continuous and even from 400° to 1800°, and resembles that of lignin rather than that

of cellulose [12]. This is to be expected since (*a*) coalification produces similar changes to those taking place during the first stages of carbonisation of raw vegetable matter and hence the structure is already partly aromatic, (*b*) more than one kind of decomposition can occur and hence the temperature range over which matter is available for addition to the hexagon layers is greater. Anthracite is similar, but the initial crystallites are somewhat larger and growth starts somewhat later (700°). (The molecular structure of coals and anthracite has been discussed by Riley [3, 15] and by Bangham [16].

For a specimen of bituminous coal, the "c" dimension (height of the crystallites) does not remain constant during carbonisation. Starting at 4·8 layer planes, it increases to 5·5 at 500°, falls to 4·7 at 1000°, then rises rapidly and may reach about 11 at 2000° (*cf.* cellulose 3·6 rising only at very high temperatures to 4·6 at 2000°). The initial rise and fall here are only small, but it is thought that they are significant since other coals show similar but more pronounced changes. Welsh anthracites do not show the initial rise, falling from 6·4 layers originally to 4·1 at 1000° and then rising to the original value. Scottish anthracites, on the other hand, maintain a constant value of about 5·4 up to 1200°.

X-ray work on coal suggests that it consists mainly of a structure of flat aromatic lamellæ (layer spacing 3·5 A.) forming a turbostratic system, together with less regular bitumen lamellæ which are sufficiently flat to be turbostratic also [17]. The term "turbostratic" (signifying randomly oriented) has been used to describe systems, such as carbon black, which are neither crystalline nor amorphous. Thus carbon black shows two-dimensional repetitions without the repetition in the third dimension characteristic of graphite. It can therefore be said to consist of turbostratic groups, each group consisting of a number of graphite layers stacked together roughly parallel and equidistant, but with a random orientation about the layer normal.

It thus appears that for a bituminous coal, layering of bitumen lamellæ on to the carbon skeleton causes an increase in the "c" dimension up to 500°; then up to 900° the escape of gases disorders the structure and may even disrupt some of the layer planes, but that above 900° thermal agitation causes further stacking of the crystallites [12]. In anthracite the first stage is omitted as it has already taken place during coalification. The results for the Scottish anthracites support the theory that the second stage has already taken place and that they are really pyro-anthracites which have been formed at a temperature of about 900° probably as a result of igneous intrusion.

The main difference between coal and simple pure compounds is that coal contains bitumen which allows the turbostratic system to become mobile in the first stages of carbonisation. The mobility is not great enough to allow rapid movement of the large lamellæ. Thus carbonisation should be sensitive to rate of heating, since a fast rate may not allow time at any given stage for complete equilibrium to be established. Riley has in fact found that slow carbonisation of a bituminous coal moves the curve of " c " dimension against carbonisation temperature to lower temperatures, showing that the structure responds more completely to both effects under conditions which allow more time for movement of the lamellæ. The difference is not the same for all coals and might be expected to vary greatly both with quantity and quality of " bitumen " content. Kipling has found for a blend of two coals that a very fast rate of heating causes a lowering in carbonisation yield, together with an increase in the apparent density and mechanical strength of the coke. Charcoals activated from the coke show the same differences in mechanical properties and also a lowered adsorption capacity for gases. These results can be interpreted in terms of a more random orientation of the lamellæ (thus avoiding the tendency to give a soft graphite-like structure) and in terms of the number of projecting groups left on the carbon skeleton. Again, there is evidence that not all coals show these effects to the same extent [18]. The relation between rank and the behaviour of coals in the plastic stage of carbonisation has been discussed by Brewer [19].

It has further been observed that oxygen promotes and hydrogen destroys the rigidity of the turbostratic system. This may be due to steric hindrance associated with the large oxygen atoms or, alternatively, to cross-linking by means of hydrogen bonds.

In chemical terms, Cobb has suggested that methane is formed at 500° by some such mechanism as :

$$R_1.CH_3 + R_2.H = R_1.R_2 + CH_4.$$

This corresponds to the removal of side-chains as depicted by Riley. Similar mechanisms are suggested for the production of hydrogen at these and higher temperatures (A), and of hydrogen and carbon monoxide in roughly equimolecular proportions at and above 700° (B, C). (B) and (C) are suggested as more likely than the alternative possibility that oxygen is eliminated directly as oxides of carbon, followed by establishment of the water-gas equilibrium, using the hydrogen formed in reaction (A). (In this connection, a recent important paper on the water-gas equilibrium is relevant [20].) In

either case, reaction (A) remains dominant after most of the oxygen has been removed (*i.e.* at and above 900°):

$$R_1.H + R_2.H = R_1.R_2 + H_2 \quad . \quad . \quad . \quad (A)$$
$$R_1.OH + R_2.H = R_1.R_2 + H_2O \quad . \quad . \quad . \quad (B)$$
$$H_2O + C = H_2 + CO \quad . \quad . \quad . \quad . \quad (C)$$

It is probable that in many cases there is no simple mutual valency saturation of R_1 by R_2 as suggested above. Riley's calculations for cellulose cokes [13] show that the degree of unsaturation of the carbon atoms in the borders of the hexagon layer planes increases from 700° upwards, and it is possible therefore that R_1 and R_2 are parts of the same layer plane in which, after loss of hydrogen and oxygen and perhaps carbon, there is a re-arrangement of bonding within the plane, rather than a combination of R_1 and R_2 in the sense suggested by Cobb.

Thus the picture of the chemical changes occurring in the carbonisation of coal is:

(i) removal of water, carbon dioxide and other occluded gases;
(ii) removal of waxes, resins, etc., by simple distillation;
(iii) simultaneous breakdown of the main coal matter, with production of gaseous and liquid distillates and condensation of the remaining solid matter into layers of hexagonal rings which become progressively more aromatic in character;
(iv) loss of radicals bound to the edges of the layer planes giving: (*a*) liquid distillates at lower temperatures; (*b*) methane and other gases at higher temperatures when only the smaller residues are left;
(v) loss of the residual hydrogen and oxygen from the borders of the planes as water gas, leaving the hexagons progressively more unsaturated and more graphitic. The ratio H_2/CO increases as the temperature rises and is highest for high-rank coals.

We next consider the physical changes which accompany these chemical changes and their effect on the physical properties of the coke. The essential coal substance appears to be in the form of micellæ or colloidal particles in which there is a nucleus of carbon atoms forming a rudimentary graphitic structure. To this is attached chemically a disordered array of other organic residues, and the whole is mixed physically with resins, waxes, other volatile organic compounds and some mineral matter. During carbonisation, the softening of the waxes and the production of the higher melting and boiling compounds by the removal of residues from the nuclear

structure produces a plastic condition which is of great importance in determining the final physical structure of the coke. This plastic mass * is made spongy by the escape of volatiles and, as the temperature rises and more matter distils away, it becomes more viscous and gradually hardens to a rigid coke. After this stage, any further gaseous evolution may modify the rigid framework of the coke, but does not alter its fundamental structure.

The plastic stage (*c.* 350°–500°) † is important in two respects. The final pore structure is probably considerably influenced by the rate of gaseous evolution during the final stage of hardening. It is also possible that some of the smallest pores may become blocked during the hardening of the plastic mass by further carbonisation of volatile material which does not escape during the final stages of plasticity. A latent pore structure of this kind would be important in post-carbonisation treatment, *e.g.* activation of the coke to produce active charcoal [18, 24].

The size, orientation and degree of interlocking of the crystallites must also depend on their freedom of movement during the plastic stage and on the amount of non-attached matter (*e.g.* " bitumen ") which can either become part of the crystallites or can serve to bind them together. These factors must be of considerable importance in determining the mechanical properties of the coke.

Cobb [1] gives tables of real specific gravity of cokes produced at a series of carbonisation temperatures, from which the following data are taken:

Temp. °C.	S.G.		Micro-porosity.	
	a	*b*	*a*	*b*
15 . . .	1·44	1·29	3·03	1·00
400 . . .	1·45	1·34	4·8	5·8
600 . . .	1·49	1·50	5·1	17·9
800 . . .	1·76	1·77	9·8	20·8
900 . . .	1·83	1·85	9·9	18·6
1100 . . .	1·70	1·87	1·0	14·9

Micro-porosity = $100(d_{H_2O} - d_{Hg})/d_{H_2O}$
a = Welsh ortho-anthracite.
b = Welsh meta-bituminous coal.

* Cannon [21, 22] correlates heat of wetting with internal area of cokes and hence shows that there is a decrease of internal area from 200° right up to 1000°. There is a marked drop just before the main evolution of volatiles commences, suggesting that fluids are flowing over the micelles and filling or covering micro-pores. Cannon's results, however, scarcely seem to agree with the pore volumes of Cobb and there may be some other factor besides internal area which determines heat of wetting. See also Bangham [16].

† A description of the processes occurring in retorts during the plastic stage is given by Reynolds and Birge [23].

These values include no pores which are not accessible to water and therefore give a fairly accurate value of the specific gravity of the solid matter of which the carbon skeleton is made. Thus up to 600° the specific gravity of the coke varies according to that of the raw material, but, as volatiles are removed, it tends to a common value characteristic of the carbon skeleton at that stage. This increase appears to be due to the closer packing of the crystallites and layer planes, partly arising from removal of side-chains from the layer planes, and partly due to thermal agitation which gives the crystallites enough freedom of motion to achieve some degree of orientation.

The total volume occupied by the coke increases slightly between 450° and 500°—this may be due to initial expulsion of volatiles—but contraction begins at 550° or below. At this temperature pores have been formed with thick walls of soft material and relatively low specific gravity. The total volume continues to decrease, but the porosity increases until a maximum is reached at about 850°. As the material continues to lose weight, the pore walls become thinner, but of greater density and strength. From 850° upwards the loss of weight is small and therefore the decrease in pore volume must be accompanied by an increase in the thickness of the walls.

The above picture, due to Cobb, can be correlated with Riley's results. The pores are probably channels between the crystallites. Thus the initial thinning of the walls would correspond in part to removal of adhering radicals from the edges of the crystallites. If this process continues, however, it leaves the layer planes of the crystallites unsaturated. Consequently bonding forces may arise and produce tighter binding between crystallites, leading to a general reduction in size and of pore volume, and also to greater mechanical strength of the coke. Riley has shown that there are essentially two stages of increase of mechanical strength [14, 25]. Below 600° the increase depends on the external pressure applied to the coke during carbonisation—the greater the pressure, the greater the increase. This is especially marked with weakly caking coals, which suggests that increase in pressure increases the number of micelles which come into contact and therefore increases the degree of inter-particle bonding. For coking coals, the effect of pressure is smaller, since there is a greater chance for micelles to come together during the plastic stage. The importance of this stage is also shown by the fact that cokes prepared by fractional carbonisation (so that all available volatiles are removed at each stage) are only slightly coherent powders; further by the observation that

addition of inert powders (alumina, silica, etc.) to bituminous coals can reduce the amount of free bitumen available for bonding together the rigid crystallites to the extent that a coherent coke cannot be formed [26]. Above 700°, however, the increase in mechanical strength is independent of external pressure. It therefore appears that the responsible change is chemical rather than physical, the effect of pressure below 700° being to bring into contact surfaces between which chemical reaction is to take place, whereas above 700° the rigidity of the coke prevents an external pressure from having any effect.

The transition temperature of 700° between these two processes is one at which a number of other properties of cokes show marked changes [27], *e.g.* (i) a sudden increase in temperature of ignition; (ii) a sudden decrease in reactivity to sulphuric acid and the degree to which dispersion in sulphuric acid takes place; (iii) adsorption of carbon dioxide reaches a maximum and at higher temperatures falls off rapidly; (iv) a rapid increase in electrical conductivity. The yield of mellitic acid obtained by oxidation with nitric acid followed by alkaline permanganate reaches a maximum at 700° and shows that graphitisation is almost complete at this temperature. This is borne out by the increase in electrical conductivity at the same temperature, which is a clear sign of increase of chemical unsaturation with corresponding increase in the number of free-moving electrons, and especially increase in the ease with which electrons can pass from one crystallite to another. This must be the result of graphitisation or some essentially similar process, which is helped by the removal of hydrogen bonded between the hexagon layer planes—the use of such a bond reducing the number of resonance forms which the system can assume [28]. These hydrogen atoms probably prevent access of oxidising agents to the planes and their removal may be responsible for the increase in the rate of oxidation which is characteristic of graphitisation.

The adsorption of carbon dioxide at 20° is much greater for cellulose cokes than for cokes made from coal at the same temperature (800°). It is suggested that the cellulose cokes may contain more oxygen than those from coal and that a more extensive micro-capillary system can therefore be developed from cellulose by reactions (B) and (C) above. (This is hardly in accord with the analyses given by Riley for cokes prepared at 600° and above, but comparisons are dangerous unless the same coal is used for both experiments.) Cobb has further evidence to support this theory, namely that if coal is exposed to oxygen at 200° for four weeks before carbonisation, the coke shows greater adsorption of carbon

dioxide. These observations are paralleled by the reactivity of the cokes to carbon dioxide at 900°.

Riley, supposing that hydrogen bonded between hexagon planes might hinder wet oxidation of cokes, found that the hydrogen content of the cokes could be altered by altering the rate of carbonisation: fast to 900°, 0·58 per cent. H; slow to 900°, 0·89 per cent. H. He suggested that the hydrogen cannot be bonded between the planes until the planes are in position, a process which takes some time, as it necessitates that a large number of atoms should come together. Therefore fast carbonisation should reduce the amount of hydrogen so bonded, most of the hydrogen being driven off as volatile matter before the planes were formed. It has also been shown by Warren [29, 30] that fast carbonisation results in an increase in evolution of hydrogen, perhaps at the expense of methane and ethane. Rate of carbonisation does not affect the evolution of unsaturated hydrocarbons, although all gases tend to be liberated at a higher temperature when carbonisation is fast. Fast carbonisation also decreases the coke yield and decreases the hardness logarithmically with the rate of carbonisation. These results are qualitatively in accord with those of Kipling quoted above [18], except that Kipling finds a marked increase in hardness of coke on increasing the rate of carbonisation. (It must be noted, however, that different coals were used by the two workers and that the tests of mechanical strength or hardness differed somewhat.) There is no change in porosity, but the density is decreased by fast carbonisation. Warren suggests that the slower rate of heating allows time for a change to a more stable form to take place, for which there is not sufficient time in fast carbonisation. Such a change may be the polymerisation of small units into large molecular species (*e.g.* "bitumen" molecules and large fragments attached to layer planes) or (*cf.* Riley) orientation of components of a turbostratic system. If there is sufficient time, most of the material which can polymerise does so, but in fast carbonisation some of this material is distilled away and hence the coke yield is reduced. These differences affect mainly the plastic stage. The significance of such effects have recently been assessed by considering the extent to which they are maintained throughout post-carbonisation treatment [18].

We thus see that the nature of the changes which were briefly classified in the first section of this paper have been studied in detail in relation to their physical consequences. The picture is far from complete, but recent work has shown up many possible directions for the future work which will lead to a fuller understanding of the "gradual progress" of carbonisation.

REFERENCES

1. MILNER, SPIVEY and COBB, *J. chem. Soc.*, 1943, 578.
2. PIRIE, *British Coal*, London, 1944.
3. H. L. RILEY, SCI. PROG., 1947, **35**, 590.
4. HORTON, RANDALL and AUBREY, *Fuel*, 1944, **23**, 65.
5. WHEELER, *J. Inst. Fuel*, 1938, **12**, 8.
6. LESSING and BANKS, *Fuel*, 1946, **25**, 93, reprinted with corrections from *J. chem. Soc.*, 1924, **125**, 2344.
7. LOWRY, *Ind. Eng. Chem.*, 1934, **26**, 133.
8. RHODES and WOOLRIDGE, in *Chemistry of Coal Utilisation*, (ed. LOWRY), 1945, II, p. 1357.
9. HOWARD, in *Chemistry of Coal Utilisation* (ed. LOWRY), 1945, I, p. 761.
10. SPENCER, *Petroleum*, 1944, **7**, 34, 135.
11. BOLTON, CULLINGWORTH, GHOSH and COBB, *J. chem. Soc.*, 1942, 252.
12. BLAYDEN, GIBSON and RILEY, *B.C.U.R.A. Proceedings of Conference on the Ultra-Fine Structure of Coals and Cokes*, 1944.
13. BLAYDEN, RILEY and TAYLOR, *J. chem. Soc.*, 1939, 67.
14. RILEY, *Chem. and Ind.*, 1939, **17**, 391.
15. ——, *Quarterly Reviews*, 1947, **1**, 59.
16. BANGHAM, *Ann. Rep. chem. Soc.*, 1943, **40**, 29.
17. BISCOE and WARREN (B. E.), *J. appl. Phys.*, 1942, **13**, 364.
18. KIPLING, in the press.
19. BREWER, *Ind. Eng. Chem.*, 1944, **36**, 1164.
20. LONG and SYKES, *Proc. Roy. Soc.*, 1948, **193A**, 377.
21. CANNON, *J. Inst. Fuel*, 1942, **16**, 51.
22. ——, GRIFFITH and HIRST, *B.C.U.R.A., Proceedings of Conference on the Ultra-Fine Structure of Coals and Cokes*, 1944.
23. REYNOLDS and BIRGE, *Ind. Eng. Chem.*, 1940, **32**, 363.
24. Fuel Research Board, *Technical Paper* **47**—Production of Active Carbon from Bituminous Coal.
25. BLAYDEN, NOBLE and RILEY, *J. Iron and Steel Inst.*, 1939, **136: II**, 47P.
26. BLAYDEN, RILEY and SHAW, *Fuel*, 1946, **25**, 13.
27. RILEY, *Trans. Faraday Soc.*, 1938, **34**, 1011.
28. BALFOUR, RILEY and ROBINSON, *J. chem. Soc.*, 1936, 457.
29. WARREN (W. B.), *Ind. Eng. Chem.*, 1935, **27**, 72.
30. ——, *Ind. Eng. Chem.*, 1938, **30**, 137.

PHASES IN PHOSPHORUS-MAKING

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ALTHOUGH the history of phosphorus-making requires three chapters for a complete version, it is surprising to find only the first two of these given in the texts. There was a first period: that first liberation after an eternity in bondage of white or yellow phosphorus by Hennig Brand of Hamburg, a period when the "secret" became known to Kunckel and Krafft, who improved the mode of preparation and brought considerable attention to the element. Then the second chapter when Robert Boyle became interested in phosphorus, and, more important, when his assistant or "operator," Ambrose Godfrey Hanckwitz, made sufficient phosphorus on a laboratory scale to be able to export it. This second phase, being centred as it was in London, has rightly been acknowledged both by chemical historians and in studies of the development of matches. But what of the third, that supremely important period in which not only white poisonous phosphorus was manufactured by Arthur Albright, the Birmingham Quaker, but non-poisonous red phosphorus also became produced on a considerable scale to supply both Europe and America? The history of phosphorus not merely changed completely when Scheele and Gahn first suggested bone-ash as raw material, but also turns on that pivot of 1849 when Schrötter of Vienna, discoverer of amorphous phosphorus, was lecturing to the British Association in Birmingham and was a guest of Albright. Schrötter sold patent rights in that year to Albright for a form of phosphorus which enabled industry to harness the element without the considerable risks of fire and of jaw-bone disease.

The first period of phosphorus-making contrasts with the other two in having confusing versions of the parts played by Kunckel and Krafft and of the contact between Krafft and Robert Boyle. Although Paracelsus with his "icicles which are the elements of fire" may have prepared phosphorus from urine, Brand of Hamburg, quack and alchemist, must be credited with the first definite preparation which set going the remarkable craze for phosphorus-

making or attempts at making it. Whatever Brand sought in his particular alchemical experiment in which he distilled over and condensed his *kaltes Feuer*, or *mein Feuer* as he called it, when egged on by Frau Margaretha, we do not know. But the result was a sensation, with Phosphorus Mirabilis the wonder of its age, a product which literally came to light in Hamburg 280 years ago and which burned down the walls of Hamburg within recent years. Brand, while the secret was his, became the man of the moment, patronised by dukes who were anxious to hide him in the Harz mountains to keep the recipe hidden too. Yet considerable attention was to be given to the preparation of phosphorus in Brand's time by two contemporaries. Johann Kunckel (1630–1702) was an alchemist's son who turned to glass-making, assaying, pharmacy, and to some chemistry-teaching in the Wittenberg school, while Johann Daniel Krafft was his friend at Dresden. Whether these two extracted the secret from Brand before furthering the history of phosphorus, or whether they made an independent discovery of it, depends for the answer on which of the rival versions is accepted. One common theme is that Kunckel heard of Brand's phosphorus-making after showing one of his friends a sample of "Bologna stone" or phosphorescent barium sulphide (which becomes phosphorescent calcium nitrate in more authentic references). This friend took Kunckel to see Brand's phosphorus; Kunckel wrote to Krafft about it; and Krafft later sought to buy the secret from Brand just as Kunckel arrived to see his duplicity. According to Leibniz, acquaintance of Brand and librarian and historian to the Duke Friedrich, both Kunckel and Krafft acquired the secret from Brand himself. Yet Wilhelm Homberg, that discoverer of boric acid who was provided with "the most superb laboratory Chemistry had ever seen," maintained that Kunckel re-discovered phosphorus-making after the original mode of preparation had been lost.

After several, occasionally confusing, points had been added to the Brand-Kunckel-Krafft phase of phosphorus-making, the English period of predominance in phosphorus comes as a relief, despite alternative ideas as to how Boyle came into the picture. Kunckel had certainly contributed his share by modifying Brand's process and introducing a method of casting phosphorus into sticks, all this apart from advertising it for medical use in his "Treatise on Phosphorus Mirabilis, and its wonderful Shining Pills." Kunckel's publicising phosphorus was outdone by that of Krafft, however; for it was Krafft who demonstrated its properties in his visits to America and to England. In 1677 Krafft not only showed specimens of phosphorus at the court of Charles II, but demonstrated

before Boyle and members of the Royal Society. Boyle was delighted to see "how vividly they shined; how the twinkling sparks, without doing any harm to the Turkey Carpet they lay on, continued to shine for a good while . . ." And although Krafft claimed to have furnished Boyle with his preparative process, we have Boyle's word that he made phosphorus without other hint than Krafft confessing "to me at parting, that at least the principal matter of his phosphorus was somewhat that belonged to the body of man." Certain it is that from this point the English period was to the fore. Boyle deposited his recipe with the Royal Society; Boyle's assistant, Ambrose Godfrey Hanckwitz, "chemist of Southampton Street, Covent Garden," announced "for the information of the curious, he is the only one in London who makes inflammable phosphorus . . . All unadulterated, wholesale 50 shillings an ounce." It was this same Godfrey who became an unacknowledged father of the N.F.S. by demonstrating fire-fighting with his wood lath and plaster house, constructed, set alight, and then extinguished with his "machine."

The third phase in phosphorus-making, one which took a laboratory curiosity and gave us phosphorus for large-scale industrial use, came with Albright's zest for manufacturing it. Gahn and Scheele had hinted at the possibilities of phosphorus being produced from bone-ash, this as long ago as 1770. But it was not until Albright in the 1840's took it up that any headway was made. Since Albright had joined forces with his brother-in-law in the manufacture of potassium chlorate for matches, he naturally thought of producing phosphorus for the same purpose, particularly in face of the prohibitive price of four guineas a pound. The year 1844 saw him make a start with white phosphorus prepared from South American bone-ash. Vitriol, as a second essential which Roebuck of Birmingham had ensured with his leaden-chamber process, was forthcoming not only from Albright's own lead chambers, but also from the growing Chance chemical works adjacent to the Albright venture at Oldbury. Albright succeeded simply because no obstacle proved too great for this indefatigable man. We hear of him scouring Europe for bones, of his buying them from a Danubian canned beef factory, and of his calcining them on the spot when they proved too odorous a cargo. Then in 1849 came his most important contribution to phosphorus-making with red phosphorus as his aim. This Birmingham Quaker had been so distressed with the prevalence of "phossy jaw," with often fatal results among girl workers in match factories, that when Schrötter described red amorphous phosphorus Albright realised the advan-

tages of this form. Moreover, the casting of phosphorus into glassy sticks in his own factory had always been a risky procedure, the early method being for workmen to suck molten phosphorus into glass tubes.

By 1851 we can find evidence of the struggle to produce sufficient phosphorus to supply a considerable demand. "We have lost one order for a ton of phosphorus from not being able to get it ready," he wrote in that year. But soon further supplies were on the way, with American buyers pleased with the quality and asking for still more. That same year saw a large sample of amorphous phosphorus shown at the Great Exhibition, with the Lundström brothers of Sweden taking some back with them, preparing red phosphorus matches with it, and then forgetting all about these until the Paris Exhibition of 1855 caused them to retrieve the matches from an attic and find them still strikeable. The Lundströms ordered a considerable amount from Albright, and had to convince him that it was for match manufacture when the Quaker believed that "amorphous phosphorus in such quantities as stated in your letter can, to the best of my judgment, only be used for the purposes of war." After 1857, when Albright joined in a partnership with Wilson, his factory together with that of Coignet et Fils of Lyons supplied Europe with phosphorus for some decades. It was a triumph for a British firm. But it was also a large-scale demonstration that 1849 began a new and more vigorous chapter in the story of the phosphorus-makers.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By Michael W. Ovenden, B.Sc., F.R.A.S., University Observatories, Cambridge.

Recent Developments in Photoelectric Photometry—1.—Astronomical observations can in principle be reduced to a few fundamental types of measurement, and of these one of the most important, and one which is utilised in almost all astrophysical investigations, is the measurement of apparent brightness. Knowledge of stellar temperatures, radii, masses, evolution, distribution and internal constitution has all depended at some stage of analysis upon an accurate stellar photometry.

2.—The earliest method of photometry was, naturally, the visual method. From its earliest systematic use by Sir William Herschel in 1796, it has developed specialised techniques and subsidiary apparatus to increase its accuracy; its use can be said to have culminated in the production of the *Revised Harvard Photometry* where visual magnitudes of over 45,000 stars are listed. But it is natural that visual methods have severe limitations, and, while still in extensive use for the routine observation of brighter variables, it has been replaced in more complex studies by photographic photometry. An estimate of relative accuracy of the two methods can be obtained from the fact that an experienced visual observer working at a brightness level of about two magnitudes above the theoretical limit of his instrument can attain an accuracy of 1/20 magnitude, while under similar conditions photography can reach 1/100 magnitude.

While using photography for standard investigations, astronomers have experimented with the use of any new photometric device that may be developed, and the application of the photoelectric cell is an obvious choice. The pioneers of the successful use of the photoelectric cell in stellar photometry were Stebbins and Kunz, who at Illinois in 1914 used an alkali metal hydride emissive cell. However, the path of development was not without its thorns for photoelectric photometry. The difficulty is to measure accurately the minute photocurrents involved; without amplification, the current given by the most efficient photosensitive surface available,

with Sirius observed with the 100-inch telescope, is only $2{\cdot}5 \times 10^{-9}$ amp., equivalent to a deflection of 2 cm. after half a minute of charging with a highly-sensitive moving-coil galvanometer, while most work would be with currents in the range 10^{-15} to 10^{-18} amp. With amplification, deflections can be increased, but only at the expense of noise fluctuations produced by the amplifying circuit making significant contributions to the final deflection. Thus, while photoelectric photometry has been in use for many years in special investigations (such as the photometry of extended objects, where its integrating effect over an area is a useful attribute), it is only recently that it has reached a stage of development where it can be said to have so strongly challenged photography that there is reason to believe that it will shortly supersede the earlier method.

3.—Three types of photoelectric cell are available, the photoconductive, photoemissive and photovoltaic cells. The photoconductive selenium cell was early applied to astronomy, but, proving inferior to the photoemissive cells, it was discarded in their favour; recent progress with thallium sulphide and lead sulphide cells suggests that a new attempt at applying photoconductivity might be of value. Photovoltaic cells have not found an application, development being entirely with photoemissive surfaces.

With all but the brightest stars direct measurement of the primary current is not possible. Two methods of procedure have in the past been adopted. In the "rate of charge" method, the emitted electrons are collected for a sufficiently long, known interval of time on a sensitive electrometer and the total charge measured. The "constant deflection" method consists in amplifying the current, and measuring the potential drop across a high resistance. The constant deflection method, because of its convenience in practice, has been preferred to the theoretically slightly more accurate rate of charge method.

The circuit of a constant deflection photometer is equivalent, with inherent instrumental capacity, to an RC integrator whose time constant T is usually of the order of 1 second. A time of observation of $t = 2\pi T$ is convenient.

4.—To examine further the optimum conditions for the working of a constant deflection photometer, it is necessary to take into account the "shot noise current." The number of electrons emitted from the cathode per second being within the range 10^4 to 10, statistical fluctuations in the rate of emission cannot be neglected. These random fluctuations constitute the shot noise current, a fundamental limitation to the accuracy of measurement. If

I = initial current in amps., q the charge on the electron, T the time constant of the equivalent circuit, and I_s the r.m.s. variation of I in amps., then $I_s = \sqrt{qI/T}$ [1]. In the ideal photometer, where the shot noise is very much greater than any other source of random noise, the maximum useful amplification consistent with the condition that the deflection due to shot noise be negligible can be calculated; Kron has estimated that for a time constant of 2 seconds, and a galvanometer sensitivity of 4×10^{-11} amp./mm., this optimum amplification is of the order of 5×10^{6}.

The early astronomical photoelectric photometers used either or both of two methods of amplification, *viz.* secondary-electron gas amplification within the photocell, and/or simple valve amplification. With such methods it has not been found possible to approach to any satisfactory degree the condition that the noise level of the photometer should be set solely by the shot noise component. The overall signal-to-noise ratio in a simple integrating-circuit photometer has been given by Johnson [2] substantially in the form

$$(S/N)_{RC} = \sqrt{2\pi RC}.IM\{2\pi k\theta/R + \pi q I_g + (\pi q I + AI^2)M^2\}^{-\frac{1}{2}} \quad . . (a)$$

where M is the amplification factor designed to make photocell and amplifier noise equal, k is Boltzmann's constant, θ the absolute temperature of the resistance, I_g the current on the grid of the electrometer valve, and A is the seeing constant representing the noise equivalent to atmospheric fluctuations. It is seen that the successive terms in the bracket representing contributions to noise level are due respectively to resistor, valve, and primary and atmospheric shot noise. From this equation it is seen that the adjustable components are I_g and R, and that I_g should be as small as possible while R should be large. I_g is usually set by the valves available, and a limit is set to increase of R by the necessity of keeping T reasonably small. Johnson concludes that the present imperfections of gas plus valve amplification circuits are due to resistor noise. The solution to the problem would appear to be the use of very high multiplication within the photocell itself. Apart from the inherent lack of linearity, commercial gas-filled cells are not capable of amplifications greater than ~ 10, and until 1939 this was the main limitation on the photoelectric photometry of low intensity light sources.

With the advent of the photomultiplier tube in the late 1930's, and its wartime development, the situation became hopeful. The photomultiplier tube depends upon the formation of secondary electrons by bombardment of suitable materials by the photoelectrons; the tube is so designed that several such secondary emissions are placed in cascade, and an electric potential applied

between successive stages to direct and accelerate the electrons. In recent years, the design and manufacture of photomultiplier tubes has made great progress (see, for example, the recent summary by Rodda [3]), and now commercial tubes can be obtained giving amplifications $\sim 10^6$. This is seen to be approaching the optimum amplification given above. A multiplier tube, providing its own amplification, is thus a suitable tool for astronomical photometry.

5.—Astronomy was not slow to make use of the photomultiplier tube, and in the hands of Kron the constant deflection method would appear to be approaching its fundamental limits already discussed. He has recently published [1] a full description of a typical photometer using a multiplier tube, and with these tubes now available commercially (in both America and, latterly, Britain) a Kron photometer may well become standard equipment at all leading observatories. As an estimate of the accuracy and range of such an instrument, Kron states that at Mount Hamilton, under average seeing conditions, in a test comparison of two seventh-magnitude stars with a 12-inch refractor, a precision of $\sim 0{\cdot}00096^m$ could be obtained from 5 readings in the course of an hour; this is an accuracy a whole order of magnitude better than photographic. For fainter stars, the accuracy will be less, due to difficulties in allowing for sky brightness, while for brighter stars, where nearby comparison stars of suitable brightness are not available, allowance for atmospheric extinction must be made, and the accuracy will then depend upon the constancy of the extinction coefficient.

6.—Ignoring fluctuations in atmospheric seeing (in practice, of great importance) the main limitation to indefinite accuracy is the shot noise current. The advent of the photomultiplier tube brings with it the possibility of overcoming to some extent even this fundamental limitation, for it enables the photoelectrons to be counted individually rather than as a mean current. Due to its high internal amplification, each primary electron produces a bunch of $\sim 10^6$ electrons. With the additional property of having a very short time of amplification ($\sim 10^{-8}$ second), the multiplier tube produces a series of pulses which, with a pulse amplifier (whose noise is not in a form in which it contributes to the final reading), can be made to record automatically on a counting unit.

The number of electrons per second equivalent to currents of 10^{-15} to 10^{-18} amp. are 10^4 and 10 respectively; it is thus to be expected that electron-counting will be a powerful photometric technique. It has, in fact, been shown by Johnson [4] and others that it is fundamentally more accurate than the constant-deflection method, the inferiority of the latter method being due to imper-

fections of an RC circuit as an integrator, attaching more weight to electrons emitted at the end of the observing period than to those emitted earlier. The ideal photometer, in which only initial fluctuations of photocurrent contribute to the noise level, is represented by equation (*a*) with $R = \infty$, $I_g = 0$, $A = 0$, and $M = 1$, whence we obtain for such an instrument, with an observing time $t = 2\pi RC$,

$$(S/N)_{RC} = (\pi q/It)^{-\frac{1}{2}} \quad . \quad . \quad . \quad . \quad (b)$$

With the counting of n individual events, if Δz be the r.m.s. deviation from the mean of z, the measured response, then $\Delta z/z = n^{-\frac{1}{2}}$; thus for the ideal electron-counting photometer,

$$(S/N)_{e.c.} = (q/It)^{-\frac{1}{2}} \quad . \quad . \quad . \quad . \quad (c)$$

From (*b*) and (*c*),

$$\frac{(S/N)_{e.c.}}{(S/N)_{RC}} = \frac{\varepsilon_{RC}}{\varepsilon_{e.c.}} = \sqrt{\pi}$$

where ε is the probable error. Small as this improvement is, if the constant deflection photometer can reach its fundamental limit, even a small increase may be of significance.*

In any case, electron-counting appears to offer many practical advantages. The technique has been applied recently both in America and Britain. Levitt and Blitzstein [5] have published a brief account, without circuit details, of the instrument now in operation at the Cooke Observatory, Philadelphia. G. G. Yates [6] has described an independent design in use on the 15-inch Huggins refractor of the Cambridge Observatories. To overcome changes of sensitivity due to atmospheric fluctuations moving the image of a star over the photosensitive surface, a secondary lens is used to form on the photocell an image of the object-glass (Fabry system).

The lower magnitude limit of a given instrument is set by the condition that the sky count must be of the same order of magnitude, if not much smaller, than the star count. The sky count consists of two parts: a variable component due to actual sky illumination, and a "dark current" present without any illumination due to thermionic emission of the various surfaces of the multiplier tube. If all pulses from the tube were of the same size, the count should be independent of applied voltage. Actually, it is necessary to include in the circuit a pulse amplitude discriminator to eliminate pulses due to electrons emitted from the secondary surfaces, which suffer fewer stages of multiplication. Statistical fluctuations in the pulse

* "Whether the vagaries of the earth's atmosphere rather than the sensitiveness of the equipment will set the limit of precision in the future is a question" (N. L. Pierce, 1947).

amplitudes about an average value mean that some smaller pulses will not be counted, and that changes in voltage applied to the tube will cause changes in the number of pulses rejected. The dark current is, of course, a function of temperature. Yates has shown that, by cooling the tube from 10° C. to − 80° C., the dark current is not only reduced by a factor of 3, but is also rendered much less sensitive to voltage changes.

He has also demonstrated that the variation of count with variation of applied voltage is much smaller than the variation of multiplication. As it is the latter quantity that is usually utilised, the use of electron-counting, apart from a greater theoretical accuracy, also improves the stability of the photometer.

The electron-counting photometer is still in its infancy, and although no figures have yet been published of its performance, it is doubtful if it can be said to have reached the accuracy quoted for Kron's photometer, which is in a mature stage of development. There would appear to be no reason why it should not in due course reach the same degree of efficiency, when its fundamentally greater accuracy may be of importance.

7.—One of the most far-reaching applications of these new photometric devices will be the accurate delineation of the light-curves of eclipsing binaries. Eclipsing binaries hold an astrophysical importance out of proportion to their numbers, as their chance peculiar geometrical relationships to the earth enable unique observations of certain physical properties of the component stars to be made. In particular, the light curves yield information of ellipticity of the components, and any limb-darkening effect of the eclipsed star; direct information on these properties can be obtained in no other way. But their study presents special problems for photometry. The rate of change of brightness during an incipient eclipse may be great. The elements of the system are very sensitive to the light-curve near minima, and so a photometer which is both rapid, yet reproducible over a large range of intensity, is required. The suitability of photoelectric photometry is clear, and, when its use becomes general, it may well transpire that present theory is inadequate to deal with the enhanced accuracy of observation, and interesting developments in this theoretical field will surely follow.

Solar Spectroscopy at Great Heights.—1.—With the publication by the United States Naval Research Laboratory [7] and the Johns Hopkins University [8] of the analysis of the first ultra-violet solar spectra taken at high altitudes, a survey of some of the difficulties encountered in the application of wartime developments of rocket propulsion would seem appropriate.

Since 1946 captured German V-2 rockets have been used for upper atmospheric research. Of the various projects under way, one is the measurement of solar radiation and solar spectroscopy. The principal purpose of using a V-2 rocket is, of course, the reaching of high altitudes. The limit of this type of missile is somewhat in excess of 160 km. Here the atmospheric pressure is $\sim 10^{-9}$ atmospheres; atmospheric absorption above this height should be negligible for wavelengths greater than 1000 A. In practice, it has not hitherto been found possible to take satisfactory spectra at a height of more than 75 km., or at wavelengths below 2100 A.

Difficulty is experienced in exposure time available. The longest flight with a V-2 so far made has been of only 9 minutes duration, of which time only 4 minutes are spent above 50 km. On the other hand, limitations of space prevent the use of a large, and hence rapid, spectrograph, and resolution is similarly limited. The rocket is vertical during the initial acceleration period of about 1 minute after launching; subsequently, the orientation may vary considerably and rapidly, and a means has to be provided for the elimination of the effect of rolling. It has not yet proved possible to design a telemetering system for the solar spectroscopy measurements, and the recording film must therefore be housed in a way which enables it to be recovered unharmed from the wreckage, no easy task when the speed of return to earth is about 4000 feet per second. (Craters 80 feet deep have been formed.)

2.—The design described by Durand, Oberly and Tousey [7] has the spectrograph mounted in one fin of the rocket. Two optical windows are provided to allow for a roll of more than 180°, and the incident sunlight falls on one or two lithium fluoride beads. These beads serve both to increase the acceptance angle of the spectrograph, and to form an image of the sun about $\frac{1}{100}$ mm. in diameter which acts as the primary slit. Light from this image falls upon a 1-inch original concave grating of 15,000 lines to the inch. Mounted Rowland-fashion, the 35-mm. ultra-violet-sensitised film is constrained to lie on the Rowland circle for wavelengths of 3400 A. to 1000 A., between the two automatically-winding cassettes, the take-up spool being made of armour-steel. Exposures of 0·1, 0·6 and 3·0 seconds are taken successively. Arrangement for a long 100-second exposure at the top of the trajectory was provided, but proved unsatisfactory. To facilitate the recovery of the cassette, it is ejected by an air-burst from the main missile after complete exposure. (In one case, even with these precautions, 6 days elapsed between firing the rocket and recovering the film.)

The spectrograph used by the Johns Hopkins University [8]

differed from this design in important respects. A servo-mechanism maintained orientation in one axis, perpendicular motion serving only to broaden the spectrum. The lithium fluoride beads were replaced by a quartz diffusing screen and primary slit; while this system does not suffer from the defocussing at high inclination inherent with the beads, it is of necessity much slower.

3.—Various successful spectra were obtained between October 1946 and October 1947. Of these, the most satisfactory was that of March 7, 1947, by the Naval Research Laboratory, when by chance the direction of roll of the rocket was such as to broaden the spectrum perpendicular to the plane of dispersion, resulting in a fortuitous resolution comparable with good laboratory spectra. Most information of the ultra-violet solar spectrum has been obtained from this exposure, taken at a height of 75 km.

The general trend of the absorption limit at various heights was as expected. Even at 35 km. some radiation could be observed at 2100 A. The absorption due to residual ozone between 2300 A. and 2600 A. is seen, together with the predicted " window " between the absorption of ozone and O_2 between 2300 A. and 2100 A.

Provisional identifications of some 200 lines are listed [7] in the range 3000 A. to 2280 A.; of special interest are multiplets of *FeI* and *SiI* about 2500 A., and *FeII* and *MnII* near 2600 A. (13 lines of the *FeII* multiplet being resolved), the *MgII* doublet at 2803 A. and 2796 A. is present, and shows central *emission*. Such emission was predicted by Menzel, and is known in the total sun for only two other lines, the analogous H. and K. doublet of *CaII*, although the emission here is much weaker than for the *MgII* lines. This phenomenon is of particular interest, as such radiation may be enhanced in solar flares, and is of a suitable wavelength to explain such low-level ionospheric effects as the sudden phase anomaly on long radio wavelengths.

The structure between 2200 A. and 2280 A. is very complex, and the authors conclude that it is not possible to explain it in terms of atomic lines. They suggest a band interpretation as more satisfactory, and, while no attempt at analysis of possible bands is made, it is noted that a strong band of NO exists centred at 2264 A. A layer of only 0·5 mm. at N.T.P. could produce a detectable absorption, and if the interpretation is correct, an average concentration of $\frac{1}{100}$ per cent. above 50 km. could be measured.

The general level of intensity of the continuous spectrum, as compared with a carbon-arc comparison made prior to the flight, has been plotted in terms of the intensity of a black-body at 6000° K, normalised at 4800 A. While the overlapping of many lines and

bands make accurate densitometry difficult, it would seem that the general intensity falls progressively from 70 per cent. at 3000 A. to 10 per cent. at 2600 A.

While significant advances of ultra-violet solar spectroscopy have yet to be obtained from rocket research, the success of these preliminary flights warrants the belief that such advances will not be long delayed. Fortunately, further progress does not depend upon the limited supply of captured German rockets. American ballistics science has not been slow to utilise the technical advances made during the war, and already several missiles of superior design have proved their worth above 200 km. New developments in this field are eagerly awaited.

REFERENCES

Photoelectric Photometry

1. Kron, G. E., *Harvard Coll. Obs. Circular No. 451*, 10–29, 1949.
2. Johnson, H. L., *Astrophys. J.*, **107**, 34–7, 1948.
3. Rodda, S., *J. Sci. Instrum.*, **26**, 65–70, March 1949.
4. Johnson, H. L., *Pub. Astr. Soc. Pacific*, **61**, 85–7, April 1949.
5. Levitt and Blitzstein, *Panel on Orbits of Eclipsing Binaries, Bulletin No. 6*, 13–15, October 1947.
6. Yates, G. G., *Mon. Not. R. Astr. Soc.*, **108**, 476–90, 1949.

Rocket Solar Spectroscopy

7. Durand, E., Oberly, J. J., and Tousey, R., *Astrophys. J.*, **109**, 1–16, January 1949.
8. Hopfield, J. J., and Clearman, H. E., *Phys. Rev.*, **73**, 877, 1948.

PHYSICS. By F. A. Vick, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

Some Applications of Physics in Medicine—1.—Some of the applications of physics in medicine are very well known. Medical applications of X-rays, for example, followed very soon after their discovery. More recently, advances in atomic and nuclear physics have led to proposed and actual applications in medicine which have been well publicised in the technical and lay press. These include the use of stable and radioactive tracer elements, irradiation with neutrons, high-speed electrons and protons, and the indirect use of very high-speed electrons to produce penetrating X-rays. It is not only in these more obvious and spectacular fields, however, that the applications of physics are important, but also in many others; so much so that the medical student finds, to his initial sorrow but later interest, that he has to learn an increasing amount of physics,

not always labelled as such but often appearing in disguise, as for example in physiology.

Much of the work is carried out by small teams of which a physicist is only one member, and most of it is published in specialised books and periodicals which the conventional physicist rarely sees. It is the purpose of this and later articles to draw attention to a few recent researches in the field, not only because they are interesting, but also because they may suggest applications in other fields.

2.—The techniques involved in using X-rays are, naturally, improving all the time as a result of widespread detailed work on the design of X-ray tubes, locating the beam with greater precision, measuring dose, determining the physical effects of X-rays on biological tissue, controlling exposure and improving the quality of visual or recorded shadowgraphs used in diagnosis. One interesting problem arises from this last subject in the following way. Much of a medical radiologist's time is spent in a darkened room peering into a fluorescent screen at a picture which is hardly ever as bright or as detailed as he would like. It is not possible to increase the X-ray intensity because the tolerance level of the patient may already have been reached. In many cases this situation leads to the taking of many radiographs in which the X-ray energy is integrated for an appreciable time to give an adequate exposure for a photograph which may be studied under better light conditions and at leisure. But in many cases the radiologists have not the leisure, the photographic process adds to expense, and the doctor would like to study the patient during all phases of movement of the organs concerned. Some method of making a fluorescent image brighter without increasing the intensity of X-rays incident on the patient would be immensely worth while (see for example, W. E. Chamberlain, *Radiology*, **38**, 383, 1942). Two papers dealing with this problem in a preliminary way have now appeared. The first is by J. W. Coltman (*Radiology*, **51**, 359, Sept. 1948) who begins by giving a few figures. An X-ray photograph (radiogram) is normally viewed at a brightness of 30 millilamberts, at which the visual acuity is such that two lines separated by 0·025 mm. can be resolved. X-ray fluorescent screens are often viewed at an average brightness of 0·001 millilamberts, when a separation of rather less than 1 mm. can just be resolved. After penetrating a very bulky patient the X-rays may produce a screen brightness of only 0·00005 millilambert, the resolution limit is perhaps 5 mm., and the temptation to push up the X-ray intensity beyond the tolerance limit is consequently very great. The figures quoted for the resolution limit are for 100 per cent. contrast, whereas the actual contrasts are normally

very small, so the position is really much worse, and even these visual acuities are obtained only after at least twenty minutes of dark adaptation. The modern fluorescent screen converts perhaps 30 per cent. of the absorbed X-ray energy into light, but only about 15 per cent. of the incident X-ray energy is absorbed, and some light is lost in the screen, so that the overall efficiency of the screen is about 3 per cent. It is possible that this efficiency will be increased in the future, but even with a perfect screen the gain would only be a factor of 30, which is less than desired. Some method of amplification of the brightness of a screen is thus required. There is a fundamental limit to any such amplification, however, since the weak X-ray beam arriving at the screen contains comparatively few quanta, and they produce scintillations which may ultimately "spoil the picture." Experiments have shown, fortunately, that the eye can tolerate a certain amount of scintillation.

The type of "amplifier" discussed is the "image converter," similar in principle to the "infra-red telescope" developed in this country, U.S.A. and Germany during the war (T. H. Pratt, *J. Sci. Instr.*, **24**, 312, 1947). In its simplest form, already tested, the X-rays fall on a fluorescent screen in contact with the plain glass window at one end of a cylindrical evacuated glass tube. The light generated goes through the glass and excites electrons from a photo-sensitive surface on the inside of the window. These emitted photoelectrons are accelerated by an appropriate electrostatic field and impinge on another fluorescent screen at the other end of the tube, where they form a visible image identical with the original one (ideally). Each X-ray quantum, $\lambda = 0{\cdot}2$ A.U., will generate about 25,000 quanta of wavelength in the region $\lambda = 5000$ A.U. Assuming that all the X-rays are absorbed in the phosphor, about 30 per cent. of the X-ray energy will be converted to light, giving about 7500 light quanta for each X-ray quantum. Not all these reach the photosensitive surface, and the latter will have a quantum efficiency of up to 10 per cent. Hence about 450 electrons will be emitted for each X-ray quantum. These must be focussed to a point on the second phosphor, and this is achieved by an axial magnetic field of appropriate magnitude. After acceleration down the tube, these electrons will gain sufficient energy to liberate more quanta at the second phosphor, thus giving a gain in brightness. It is important that the light here produced does not shine back on to the photosensitive surface, since this would lead to instability ("positive feed-back"). A thin layer of aluminium is evaporated on to the back of the second phosphor to prevent this effect. If the aluminium is thin enough, the electrons will punctuate it but

not light, and also the layer acts as the accelerating anode. A second difficulty, not mentioned by Coltman, may arise if appreciable intensities of X-rays reach the second screen. The second screen may be chosen to fluoresce at a colour (yellow-green) at which the eye is most sensitive, but the first screen may fluoresce in the deep blue for greater efficiency in the photo-electric process. The first tube made was calculated to give a brightness gain of about twenty, which is not enough, but the process could be repeated in a two-stage tube to give an overall gain of 400.

A second design was planned in which, by electron lenses, the image on the second screen is reduced in size by a factor of five compared with that on the second screen, giving a brightness gain of 25. The second image is then enlarged optically by an appropriate eyepiece (with small loss of brightness and contrast). It is envisaged that the increase of brightness will be sufficient to resume experiments with stereoscopic fluorescent pictures, which had hitherto been disappointing owing to the low brightnesses then possible. (*See* J. W. M. Dumond, *Radiology*, **19**, 366, 1932.)

The second paper, by G. A. Morton, J. E. Ruedy and G. L. Krieger (*R.C.A. Rev.*, **9**, 419, Sept. 1948) is concerned with general principles rather than specific designs, and takes into account the optical devices associated with the image converter. As an alternative to the reduced image size image converter described in the previous paragraph, a reduced image of a fluorescent screen as normally used may be produced on the photosensitive surface by a wide aperture lens, giving an additional brightness amplification. As is pointed out in the paper, a Schmidt objective with aperture 10 metres or more in diameter could be used. Hence the gain in brightness achieved is not just that due to the image converter, but also the result of the greater aperture of the objective lens which can then be used to advantage. The preliminary investigations discussed in the two papers indicate that a very important aid to the medical radiologist is likely to result from further development work.

3.—Leaving for a later occasion further work with X-rays, we turn now to a very different subject by way of example—the detection of traces of poisonous gases in the atmosphere. Some such gases give adequate warning by their smell (*e.g.* H_2S), but with others, *e.g.* CO, it is very desirable to have some method of warning when the concentration has reached a dangerous limit. Fortunately, the mechanism of poisoning involving the prevention of the vital oxygen transfer to hæmoglobin in the lungs is very analogous to the poisoning of a platinum catalyst used for speeding the reaction of

oxygen with another gas, *e.g.* in the combustion of hydrogen. The use of a platinum catalyst in a poison-gas alarm is described by J. Boche (*Philips Techn. Rev.*, **8**, 321, 1946). In acting as a catalyst, the two gases involved are adsorbed together on the platinum surface, causing a lowering of the activation energy for the oxidation process, which proceeds rapidly at a comparatively low temperature. If gas molecules like HCN, H_2S, CO are already adsorbed, the catalyst is rendered ineffective and is said to be "poisoned." In the apparatus described, a mixture of methyl alcohol vapour and air is passed over a platinum wire covered with a layer of aluminium oxide coated in turn with a layer of finely divided platinum (platinum black). The wire is in one arm of a Wheatstone bridge. The heat of oxidation released by the reactions at the platinum black surface heats the wire, which attains an equilibrium temperature of 120–150° C., when the bridge is adjusted to balance. The presence of a poison affecting the catalyst immediately lowers the temperature of the wire, unbalances a bridge and actuates a relay which sounds an alarm. The apparatus is reset by first heating the platinum wire to about 400° C., which causes the adsorbed poisons to evaporate. The detector responds to some vapours, *e.g.* petrol, which is not poisonous, and does not necessarily indicate the presence of all poisonous gases and vapours, but it should be valuable in situations in which the presence of CO (for example) is a possible hazard.

4.—As an example of the use of a radioactive tracer element, we may take the problem of determining the time taken for blood to flow between two points in a leg vein in normal patients and those suffering from venous thrombosis (S. B. Osborn and H. P. Wright, *Brit. J. Radiol.*, **22**, 110, Feb. 1949). $Na^{23}Cl$ was irradiated by neutrons to prepare $Na^{24}Cl$. Na^{24} is a convenient isotope, since it is a γ-ray emitter (β-rays would not penetrate the overlying tissues), has a half life of about 14·8 hours, and is not likely, in NaCl to cause any disturbing physiological reactions. In principle, saline solution containing the tracer is injected into the vein at one point and its arrival at the second is detected by a Geiger-Müller counter (G.-M. counter) placed there, but in practice it is not possible to do this directly, since both the background and enhanced counting rates are subject to fluctuations which mask the change until some time after it occurs. The method adopted depends on plotting against time the total number of discharges in the G.-M. counter from some time zero. The slope of the curve obtained gives the counting rate at each instant, and the graph obtained consists of two straight portions, one for the initial background rate, and one for the blood containing the tracer. The intersection of the two

straight lines indicates the time at which the radioactive sodium first reaches the G.-M. counter. In the apparatus used, three electrically-driven pens record on a rotating drum. Two give small constant deflections when operated, one being used as a time marker and the other to indicate the moment of injection of the saline solution into the vein. The third is attached to an assembly which can move down a vertical geared rack. Each impulse from the G.-M. counter, suitably amplified, allows this pen to descend one notch (about 0·6 mm.). The total distance descended by this pen is a measure of the number of impulses during the time of the experiment, and draws the required graph in a series of small steps. All three pens are, of course, in the same vertical line. The G.-M. tube is shielded as far as possible from γ-rays arriving from directions other than vertically below.

5.—It is intended to return to this subject at a later date, with particular reference to applications of "classical" physics in medicine.

GENERAL AND PHYSICAL CHEMISTRY.

By J. W. Smith, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic.

The Metaphosphoric Acids and Metaphosphates.—Recent studies have provided ample proof, if any were required, that the inorganic chemistry of the commoner elements is by no means the exhausted subject which is sometimes pictured. The application of physical methods to the study of compounds which have been known for many years has served to correct many misconceptions arising from the earlier qualitative studies, and to re-awake an interest in compounds the structures of which have been long in doubt.

An outstanding problem for some years has been the composition and structure of the metaphosphates, which have long been known to exist as polymers of varying degrees of complexity, produced, for example, by heating phosphoric acid or adding a small amount of water to phosphorus pentoxide, followed by the neutralization of the metaphosphoric acid produced with alkali, or by submitting acid orthophosphates to various thermal treatments. A very large number of these metaphosphates have been described, but the position has been very confused, as many of the reports on the subject have been completely contradictory. During the past few years, however, much has been done towards clarifying the position, particularly through the X-ray, thermal, and microscopic studies

of Boullé (*Compt. rend.*, 1935, **200**, 658, 832) and of Partridge, Hicks, and Smith (*J. Amer. Chem. Soc.*, 1941, **63**, 454) and quite recently through the conductimetric studies on aqueous solutions by Davies and Monk (*J. Chem. Soc.*, 1949, 413).

The degree of complexity in the sodium metaphosphates has generally been deduced by the application of two methods, the calculation of the molecular weight from freezing-point depression measurements, and the application of the so-called "Ostwald-Walden-Bredig rule" to conductivity data. As has been pointed out by Davies and Monk, however, the former method is not entirely convincing, as even in cases where activity corrections have been applied there is a great deal of uncertainty regarding the extent of complex formation and ion association in the multivalent electrolytes. The "rule" which is applied in the second method states that the equivalent conductivity of a salt at 25° C. is less at a concentration of 1/32 than at 1/1024 g.-equivs. per litre by an amount $10n$, where n is the valency product of the ions. Although this rule holds fairly well in some cases, *e.g.* for magnesium salts, it is not very satisfactory in the present instance as it fails for the orthophosphates and has never been adequately tested for ions of valency in excess of 2.

Much of the earlier work on this subject, and even some of the later work, has therefore to be discounted, and in this summary an attempt will be made to present the picture of the subject which seems to be the most probable as a result of the present evidence.

One of the earliest forms of sodium metaphosphate to be obtained was the so-called "Maddrell's salt," a sparingly soluble solid, stated to exist in two crystalline forms, which was first obtained by Graham (1833). It is most readily produced by heating sodium dihydrogen phosphate (NaH_2PO_4) at 300–400° C. for several hours. If, on the other hand, the product is kept at 500–600° C. for some time a white soluble compound, "Knorre's salt," is formed. This same material was prepared by Knorre by fusing together disodium hydrogen phosphate (3 parts) and ammonium nitrate (1 part) and heating them at about 330° C. for six hours.

Knorre's salt melts at 625° C., and if the melt is allowed to cool slowly the same compound crystallises again, but if it is cooled rapidly the product is "Graham's salt," a glass readily soluble in water. Knorre's salt and Graham's salt resemble one another in coagulating albumen and in yielding no precipitate with calcium salts, but the latter gives precipitates with silver and lead salts, whereas Knorre's salt does not. Graham's salt, long regarded as a

hexametaphosphate $Na_6P_6O_{18}$, is the material utilised for softening water.

The inter-conversion of these compounds has been reinvestigated by Partridge, Hicks, and Smith by thermal and X-ray methods. They find that the form of Maddrell's salt obtained at lower temperatures is converted into the second form at 400–425° C., and that this passes into Knorre's salt at 475–500° C. Also, when Graham's salt is heated for some time at 300° C. or above it recrystallises, yielding Knorre's salt, but, especially at the lower temperatures, some Maddrell's salt is also formed.

Knorre's salt may be purified from other metaphosphates by precipitating these from dilute aqueous solutions as the lead salts. When pure it is not deliquescent. Cryoscopic measurements, particularly those of Nylèn (1936), suggested that this compound was a trimetaphosphate $Na_3P_3O_9$. This formulation has been confirmed by Davies and Monk as a result of their conductivity measurements. The plot of the equivalent conductivity (Λ) against the square root of the equivalent concentration (c) is of the normal form to be expected for a true salt, and linear extrapolation of the values for the most dilute concentrations studied indicates that the equivalent conductivity at infinite dilution (Λ_0) is 133·70, leading to a value of 83·59 for the mobility of the metaphosphate ion. The course of this $\Lambda - \sqrt{c}$ curve lies very near to the theoretical limiting slope calculated from Onsager's equation for a uni-tervalent salt. The slight deviations from this law are attributable to ion association, and on certain assumptions they deduce that the thermodynamic dissociation constant of the NaP_3O_9'' ion is 0·068, corresponding on Bjerrum's electrostatic theory to a closest distance of approach of the Na^+ and P_3O_9''' ions of 8·41 Å.

These workers have also obtained solutions of trimetaphosphoric acid by exchanging hydrogen for the sodium by passing a solution of the sodium salt through a column of cation-exchange resin in the hydrogen form. They find the acid to be relatively stable in water, its conductivity showing no change over considerable periods. It is a very strong acid, the dissociation constant of the HP_3O_9'' ion being about 0·009, comparable with the second dissociation constant of sulphuric acid.

The tetrametaphosphates were first studied by Warschauer (1903), who prepared the copper salt by stirring powdered copper oxide into a slight excess of orthophosphoric acid and allowing the mixture to stand until a clear blue mass was obtained. This was heated on a water bath and later on a sand bath at a temperature not exceeding 450° C. until the excess of acid began to be evolved

as a white vapour. On powdering the resulting mass and extracting with water the insoluble copper tetrametaphosphate $Cu_2P_4O_{12}$ remained. If this compound is stirred with sodium sulphide solution, copper sulphide is formed and sodium tetrametaphosphate remains in the solution. The sodium salt is very soluble and gives no precipitate with either silver nitrate or hexamminocobaltic chloride ($Co(NH_3)_6Cl_3$, a reagent which gives a reddish-yellow precipitate with pyrophosphates but not with any of the metaphosphates), but it coagulates albumen. Warschauer's conductivity measurements led to the tetrameric formula $Na_4P_4O_{12}$, and by cryoscopic measurements in fused sodium sulphate decahydrate Bonneman (1937) obtained a molecular weight of 418, in accord with this formulation.

The tetrameric form of this compound has also been confirmed by Davies and Monk, who have shown that the slope of the $\Lambda - \sqrt{c}$ curve is nearest to the theoretical slope for a uni-quadrivalent salt. From the results they deduce that the dissociation constant of the NaP_4O_{12}''' ion is 0·009, corresponding on Bjerrum's theory to a mean ionic diameter of the Na^+ and P_4O_{12}'''' ions of 6·40 Å., and comparable with the value of 0·005 for the $KFe(CN)_6'''$ ion.

The solutions of sodium tetrametaphosphate, on treatment with cation exchange resin, yielded tetrametaphosphoric acid, the solutions of which showed no change of resistance with time, indicating the stability of this acid in dilute solution. The acid is strong, its fourth dissociation constant being about 0·0018.

A homogeneous form of Graham's salt, the so-called hexametaphosphate, is best produced by fusing any of the other forms and maintaining the melt at 650–700° C. for at least an hour before chilling very rapidly, preferably in thin sheets. The product is hygroscopic and extremely soluble in water. The formulation of it as a sixfold polymer has always rested on the doubtful evidence of conductivity measurements (Tammann, 1892) and the alleged preparation of various salts of the type $Ag_5NaP_6O_{18}$ and $Na_2Ca_2P_6O_{18}$, the action of the commercial product in softening water without the precipitation of the calcium salt being attributed to the formation of the latter compound. The amorphous glassy nature of Graham's salt, however, combined with the fact that it has a large and rather indefinite solubility in water, yielding solutions of very high viscosity, suggests that it is a high polymer of colloidal character. Although early molecular weight determinations by the cyroscopic method (Jawein and Thillot, 1889), carried out in about 10 per cent. aqueous solutions, suggested a molecular weight of about 400 $Na_6P_6O_{18}$ = 612), this result was almost meaningless. On the

other hand, Lamm (*Arkiv Kemi Min. Geol*, 1944, **17A**, No. 25) by the ultra-centrifuge method has found a molecular weight of about 13,000, equivalent to nearly 130 $NaPO_3$ units. He states that the solutions show a remarkable uniformity of particle size, thereby differing from solutions of the usual organic polymers which reveal rather a wide range of particle sizes in the ultra-centrifuge. He also obtained some evidence that the particle size decreases with increasing time of heating of the melt from which it is chilled.

Using carefully purified material, Davies and Monk have found that the change of equivalent conductivity with concentration in this case is quite unlike that encountered with a normal salt, the slope of the $\Lambda - \sqrt{c}$ curve for the most dilute solutions being much greater than would be expected for a uni-sexavalent salt. On the other hand, the curve obtained was very similar to that found for known colloidal electrolytes. They infer that the colloidal character of this compound does not arise through the loose association of $(PO_3)_6$ ions or other relatively small units to form a micelle, as if such were the case a critical concentration should exist at which reversible dissociation into simple units would occur. Such is shown, for instance, by octadecylamine hydrochloride and cetyl pyridinium bromide. With solutions of Graham's salt, however, there is no evidence of such a transition even at equivalent concentrations as low as 5×10^{-6}. The conductivity of the solutions increases with time, showing that this form of the salt is much less stable than the tri- and tetrametaphosphates. It is not surprising, therefore, that the free acid produced by the cation exchange process proved to be too unstable to give accurate conductivity data, although approximate measurements indicated that the acid has a conductivity–concentration curve similar to that for the sodium salt.

At various times claims have been put forward for the existence of monomeric and dimeric forms of sodium metaphosphate. Thus Pascal (1923) obtained a product which he believed to be the monometaphosphate by treating $(C_2H_5PO_3)_n$, formed by boiling a mixture of ether and phosphorus pentoxide, with sodium ethoxide in ether. His evidence for the monomeric structure rested on his freezing-point depression measurements, but Nylèn's measurements on this product indicated a degree of polymerisation of between two and three. Similarly no support has been obtained for the monomeric character of the product obtained by Beans and Kiehl (1927) by heating microcosmic salt, NaH_2PO_4, or $Na_2H_2P_2O_7$.

Pascal and Rechid (1933), by heating glacial phosphoric acid until crystals of an insoluble polymeride just began to form, and then dissolving the product in water at 0° C., obtained a solution

which when neutralised with the calculated amount of sodium hydroxide and treated with an excess of alcohol precipitated an oil which crystallised rapidly to give a product which they regarded as a hydrate of sodium dimetaphosphate. Travers and Chu (1934) claimed to have made the same compound in purer form by heating a mixture of disodium hydrogen phosphate (5 parts) with ammonium nitrate (1 part) and ammonium chloride (1 part) in a vacuum for eight hours at 250° C., the product being neutralised and purified by recrystallisation.

However, conductivity measurements by Davies and Monk have shown that all these products are actually more or less impure forms of sodium trimetaphosphate, and that there is no evidence whatever for the existence of either a mono- or dimetaphosphate. They conclude, therefore, that the monomer is unstable, but tends to give polymers of which the unit group is $-\mathrm{O}-\underset{\mathrm{O}}{\overset{\mathrm{O}}{\mathrm{P}}}-'$. These polymers may be closed rings of three or four units in the tri- and tetrametaphosphates, or a long chain of colloidal dimensions in Graham's salt.

Little is yet known of Maddrell's salt, the low solubility (less than 0·1 g. per litre at 15° C.) having restricted its study. Boullé stated initially that it had the same X-ray spectrum as Knorre's salt and hence inferred that it was an insoluble trimetaphosphate, but his later evidence was at variance with this and suggested that he was not dealing with a pure product.

In addition to Maddrell's salt, other insoluble metaphosphates known as Kurrol salts have also been described. They are said to be formed by heating sodium dihydrogen phosphate at various temperatures below fusion (Pascal, 1924) or by the prolonged fusion and slow crystallisation of Graham's salt. Their formation seems to be capricious, as some investigators have failed to obtain any such product under these conditions. Pascal describes them as crystalline and melting at 809° to give a liquid quite distinct from that obtained by fusion of Graham's salt. They are insoluble in water, but dissolve in solutions of pyrophosphates or of Graham's salt, giving very viscous solutions, a circumstance which suggests that they are high molecular weight polymers which pass into colloidal solution. As a result of viscosity measurements Pascal inferred that at least three different Kurrol salts exist, but there has been very little systematic recent investigation of the sodium salts. A potassium analogue of this compound is much better characterised, however. When potassium dihydrogen phosphate is heated at 170° it yields a soluble potassium trimetaphosphate, there being no

potassium analogue of Maddrell's salt. On heating to temperatures above 320°, however, a Kurrol salt, melting at 838° C. is formed. Lamm and Malmgren (*Z. anorgan. Chem.*, 1940, **245**, 103) have found that when this product is powdered and treated with two equivalents of concentrated sodium chloride solution the powder swells gradually and finally yields a viscous elastic mass. The potassium in the polymer is partly replaced by sodium and most of the substance becomes soluble. If the chloride is removed by kneading with alcohol, a residue remains which in 1 per cent. aqueous solution has a viscosity five times that of water. Dilute hydrochloric acid, sodium hydroxide, or sodium thiocyanate reduce the viscosity of these solutions to normal values, whilst higher concentrations of these compounds cause precipitation. Sedimentation studies on freshly prepared solutions indicate that the molecular weight of the particles is about 20,000, but in dilute sodium thiocyanate solution the mean molecular weight appears to be between 100,000 and 140,000.

These recent studies indicate that there are many avenues yet to be explored in this field, particularly in respect of these high molecular weight polymerised forms. It seems to be most probable that these compounds have structures similar to those of silicates, and that the properties are critically dependent on the conditions of preparation.

Many of the outstanding problems are of considerable practical importance. For instance, the effect of Graham's salt in softening water and in removing boiler scale, which had been attributed to the formation of a $Ca_2P_6O_{18}''$ anion, is now by no means clear. The fact that at very low concentrations (1 to 5 parts per million) it acts as a deterrent to the crystallisation of calcium carbonate has long been puzzling, as this quantity is very far from sufficient to combine with all the calcium it appears to hold in solution. This action, on which the so-called "threshold treatment" of water is based, can be most readily explained by the adsorption of the colloid on submicroscopic nuclei of the carbonate, thereby preventing their growth and hence their precipitation, and doubtless the normal softening action of Graham's salt is in some way to be attributed to its colloidal character.

BIOCHEMISTRY.

By Professor W. O. Kermack, D.Sc., LL.D., F.R.S., The University, Aberdeen.

Hæmoglobins and Other Iron-Porphin Compounds.

When the biochemist makes a broad survey of the structure of natural compounds, he cannot fail to be impressed with the almost

ubiquitous occurrence of a few basic patterns. Examples of such patterns are the steroid ring system and the porphin nucleus. These basic patterns turn up in all kinds of animal and plant tissues but in a variety of forms, and the different forms often serve quite distinct purposes and perform different functions in cell metabolism. In the evolution of living matter it would seem that such structures must have been developed at a very early stage, modifications being introduced at various times to meet new needs and to produce higher efficiency and better adaptation to environment.

The porphin nucleus (I), which has the shape of a flat disc and contains four pyrrole rings connected by four —CH= groups, is best known in the form of its derivatives hæmoglobin and chlorophyll. Hæmoglobin, as is well known, contains in its molecule four hæm groups, hæm (II) being a combination of the commonest porphin, namely protoporphin, with one atom of ferrous iron. (In this article we have followed the practice adopted in a valuable recent review by Granick and Gilder (*Adv. Enzym.*, 1947, **7**, 305), who use the words porphin, protoporphin, etc., instead of the more common porphyrin, protoporphyrin. The change seems justified as porphin is in general use to denote the fundamental tetrapyrrole ring system (I) lacking the methyl, vinyl and propionic acid side-chains of " protoporhyrin ".) In the molecule of hæmoglobin these four hæm nuclei are attached to a molecule of the protein called globin. With chlorophyll, the basis of which is a somewhat different prophin, phæophorbide, in combination with magnesium, we shall not be concerned here.

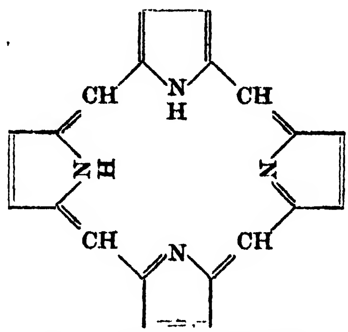
Hæmoglobin, Myoglobin and Other Compounds undergoing Reversible Oxygenation

The outstanding property of hæmoglobin is its ability to form a relatively loose compound with molecular oxygen, while the iron atom still remains in the ferrous condition. Suitable oxidising agents do oxidise hæmoglobin to methæmoglobin, which contains ferric iron, and indeed molecular oxygen does bring about the same oxidation, but only very slowly, in marked contrast to the speed with which oxyhæmoglobin is formed.

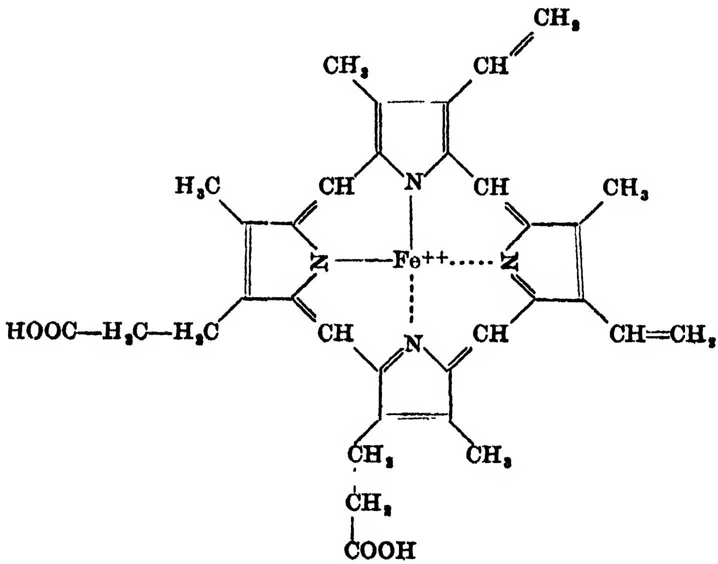
Hæmoglobin is essentially an oxygen carrier, taking up oxygen when the oxygen pressure is high, and releasing it again when the oxygen pressure is low.

It may be noticed in passing that hæmoglobin is not the only oxygen carrier devised by nature in the course of evolution. There are at least three other distinct types. The best known is perhaps hæmocyanin, found in many crustaceans and molluscs, a protein

containing copper as the element concerned in the oxygen-carrying function. Sipunculids, unsegmented marine worms, contain a reddish-purple iron-containing protein called hæmerythrin, which functions as an oxygen carrier. Hæmerythrin, though it contains



(I) PORPHIN NUCLEUS



(II) HÆM

$[Co(NH_3)_5.O_2.(NH_3)_5Co](NO_3)_4$
(III)

iron, lacks porphin of any kind. The green pigment chlorocruorin found in certain annelids, more closely resembles hæmoglobin in that it consists of an iron-porphin compound united to a protein; here, however, the porphin is not the protoporphin present in hæmoglobin, but another porphin called "chlorocruoroporphyrin."

It may be mentioned that in recent years a number of inorganic compounds have been found to share with these natural pigments the property of reversible oxygenation. The latest of these to be discovered is a co-ordination complex of cobalt with ammonia which, under suitable conditions, can be oxygenated to yield a compound which has probably the constitution (III) (Michaelis, *Arch. Biochem.*, 1948, **17**, 201). In this the cobalt remains divalent and the compound gives off oxygen readily when the oxygen partial pressure is sufficiently low. It is perhaps surprising that no cobalt-containing oxygen carrier has so far been detected in living tissues.

Hæmoglobin in the narrower sense of the term is peculiar to the vertebrates. Each species has its characteristic hæmoglobin, but all of them have the same prosthetic group, namely hæm. The hæmoglobins of various species differ in respect of their protein moieties, but all the various vertebrate hæmoglobins share a strong family resemblance. The molecular weight is always in the neighbourhood of 68,000 and one molecule carries four hæm groups. In addition however to these vertebrate hæmoglobins, there are a number of other hæm-containing oxygen carriers, which may also be conveniently classified as hæmoglobins, though they differ in some respects from the true hæmoglobins. Various invertebrate hæmoglobins come into this category; the molecular weight of these compounds varies from 17,000 to several millions. The invertebrate hæmoglobins of molecular weight 17,000 (sometimes called erythrocruorins) contain only one hæm per molecule, instead of the four present in true hæmoglobin.

Vertebrate hæmoglobins and the invertebrate hæmoglobins of relatively low molecular weight are normally enclosed within cells in the blood or hæmolymph of the animal; some invertebrate hæmoglobins, however, occur free in the hæmolymph in colloidal solution, and such extra-cellular hæmoglobins have very high molecular weights, 400,000 or over.

In recent years considerable advances have been made in the study of a series of closely related compounds, myoglobins. These are found within muscle cells, and are more difficult to study both because they occur in small quantities in the animal body, and also because they are more troublesome to isolate and purify.

Myoglobin from horse muscle was first crystallised by Theorell (*Biochem. Z.*, 1932, **252**, 1; *ibid.*, 1934, **268**, 46, 55, 64 and 73), who also made a careful study of the properties of the purified pigment, but the myoglobin of other animal species proved more difficult to isolate in the crystalline form.

Roche, Derrien and Vieil (*Trav. Membres Soc. Chim. Biol.*, 1942,

24, 1016) were able to crystallise ox myoglobin, but could only obtain the myoglobin of the dog, pig and sheep muscle in an amorphous form. Theorell and de Duve (*Arch. Biochem.*, 1947, **12**, 113) have now succeeded in crystallising human myoglobin from heart muscle and also from the urine of patients excreting this protein, and find that human heart muscle myoglobin and myoglobin from urine show only slight and probably insignificant differences in their properties. Human myoglobin crystallises in the same fan-shaped clusters of needles as horse myoglobin, but its carboxy compound appears to show some spectral peculiarities as compared with the corresponding derivative of horse myoglobin. The myoglobins of different animals are species specific and apparently differ from each other in somewhat the same way as the various hæmoglobins. They have approximately the same iron content as hæmoglobin, namely, 0·34 per cent., but the molecule has a molecular weight of about 17,000 and contains only one hæm group.

SOME RECENTLY DESCRIBED HÆMOGLOBINS

1. *Root Nodule Hæmoglobin.*—The red pigment present in the root nodules of leguminous plants was first described by Mothes and Pietz in 1937 (*Naturwiss.*, 1937, **25**, 201), but it was Kubo (*Acta. Phytochim.*, 1939, **11**, 195) who first identified it as a hæm derivative. His conclusions were confirmed by Burris and Haas (*J. Biol. Chem.*, 1944, **155**, 227) and also by Keilin and Wang (*Nature*, 1945, **155**, 227). The latter obtained the compound in a purified form and showed that it could be reversibly oxygenated and was to be regarded as a true hæmoglobin. They determined its relative affinities for oxygen and carbon monoxide. The partition constant $K = (HbCO) \times (pO_2)/(HbO_2) \times (pCO)$, which measures this, was found to be 37 at 15° as compared with figures from 125 to 550 for various vertebrate hæmoglobins. They also measured the span, that is, the distance in Ångström units between the α-absorption bands of the oxy- and the carboxy-hæmoglobin. This they found unusually large, being 100, whereas for vertebrate hæmoglobins it is 43–56.

The function of the hæmoglobin in the root nodules does not seem to have been definitely ascertained. It appears to be present in all leguminous root nodules so far investigated. On the other hand, cultures of the bacteria concerned, namely *Rhizobium*, do not appear capable of synthesising the pigment in the absence of the plant nodule cells. Similarly, plant cells free from bacteria produce no hæmoglobin. It is only the symbiotic union of bacteria and plant cell which appears capable of synthesising the compound

and it seems agreed that the hæmoglobin probably plays some important part in the process of nitrogen fixation.

2. *Gastrophilus Hæmoglobin.*—The larva of the horse bot fly, *Gastrophilus intestinalis*, contains a pigment which proves to be a hæmoglobin and has been recently investigated by Keilin and Wang (*Biochem. J.*, 1946, **40**, 855). The female of the bot fly fixes its eggs to the hair of the horse. In five or six days the larvæ are completely formed, but they do not emerge until licked by the horse's tongue. The larva penetrates under the epithelial layer of the tongue and ultimately, after intermediate stages, locates itself in the cardiac end of the stomach attached to the mucosa by means of its mouth hooks, where it remains in a relatively inactive form for several months. In its tracheal cells there is developed the hæmoglobin which serves an important function in respiration. For long periods little oxygen is available to the parasite in the horse's stomach. From time to time, however, especially during feeding, bubbles of air will be swallowed. The presence of hæmoglobin allows of the absorption by the larva of a much larger amount of oxygen from such bubbles than would be possible if ordinary solution of the oxygen had to be relied upon. The oxygen taken up in the oxyhæmoglobin forms a store which the organism can use during the long periods when no air reaches it.

Keilin and Wang have purified and crystallised the *Gastrophilus* hæmoglobin, which behaves in the Tiselius apparatus as a homogeneous protein and has an iso-electric point at *p*H 6·2. It contains 3·82 per cent. hæm which corresponds to an equivalent weight of 17,300. Preliminary measurements of the osmotic pressure of the methæmoglobin prepared from this hæmoglobin yield a mean value of 34,000 ± 3000 for the molecular weight (Adair, Ogston and Johnston, *Biochem. J.*, 1946, **40**, 867). The molecule of *Gastrophilus* hæmoglobin therefore contains two hæm nuclei and is only half the size of a molecule of vertebrate hæmoglobin, but double that of myoglobin. Its oxygen dissociation curve very closely approximates a rectangular hyperbola. *Gastrophilus* hæmoglobin has a high affinity for oxygen; in a strong solution at 39°, half saturation with oxygen is reached at an oxygen pressure of 4·9 mm. Hg. and in a dilute solution at less than 0·02 mm. Hg. It has a low affinity for carbon monoxide, the partition constant K being about 0·67, which is only about 1/800th of that of horse hæmoglobin, and is of the same order of magnitude as the partition constants of certain hæm-containing enzymes which vary from 0·1 to 1. The span is about 95 Å. which is very near that of the root nodule hæmoglobin (100 Å.), but much larger than the span of

other hæmoglobins and myoglobins (31–56 Å.). Neither *Gastrophilus* nor root nodule hæmoglobin supports the suggestion, verified for mammalian hæmoglobins (Anson, Barcroft, Mirski and Oinuma, *Proc. Roy. Soc.*, 1924, **B97**, 61), that log K is proportional to the span. Table I, taken from the paper by Keilin and Wang (*Biochem. J.*, 1946, **40**, 863) gives the value of K for various hæmoglobins and also shows the value of the span in Ångström units and the ratio of the two.

TABLE I

RELATIONSHIP BETWEEN THE PARTITION CONSTANT $K = \frac{(HbCO)pO_2}{(HbO_2)pCO}$ OF DIFFERENT HÆMOGLOBINS AND THEIR "SPAN," THE DISTANCE IN ÅNGSTRÖMS BETWEEN THE α-BANDS OF HbO_2 AND HbCO

Origin of Hæmoglobin.	$K = \frac{(HbCO)pO_2}{(HbO_2)pCO}$.	Span (Å.).	$y = \frac{\log K}{\text{span}}$.	Observers.
Blood of vertebrates . .	125–550	43–56	0·043–0·050	Anson *et al.* (1924).
Muscles of vertebrates . .	28–51	31–36	0·045–0·050	Roche (1932, 1933), Theorell (1934*a*, *b*).
Root nodules of leguminous plants	37	100	0·016	Keilin and Wang (1945).
Gastrophilus larvæ; tracheal cells	0·67	95	− 0·17	Keilin and Wang (*loc. cit.*).

Gastrophilus and horse hæmoglobin have the same prosthetic group but differ in their globin components. The *Gastrophilus* larva therefore synthesises its own hæmoglobin, and does not merely abstract preformed hæmoglobin from the blood of its host.

The three hæmoglobins, those of *Gastrophilus* larvæ, root nodules and myoglobin, resemble each other in (*a*) the high affinity for oxygen, (*b*) low affinity for carbon monoxide, (*c*) the low partition constant K, (*d*) the comparative ease of oxidation to methæmoglobin and (*e*) the localisation within fixed non-circulating cells of the organism. They are somewhat intermediate between the two groups of hæm protein compounds, the true vertebrate hæmoglobins on the one hand and cellular catalysts, such as the cytochromes, on the other.

3. *Hæmoglobins of Ascaris, Strongylus and Nippostrongylus.*—Intestinal worms live under somewhat special conditions and so it is not surprising that the properties of their hæmoglobins should be rather unusual. *Ascaris lumbricoides* contains two hæmoglobins, one in the body wall and the other in the perienteric fluid. Both exhibit the characteristic property of hæmoglobin, that of reversible oxygenation, but the rate at which deoxygenation takes place is extraordinarily slow. According to Davenport (*Proc. Roy. Soc.*, 1949, **B136**, 255), who has made a careful study of these pigments,

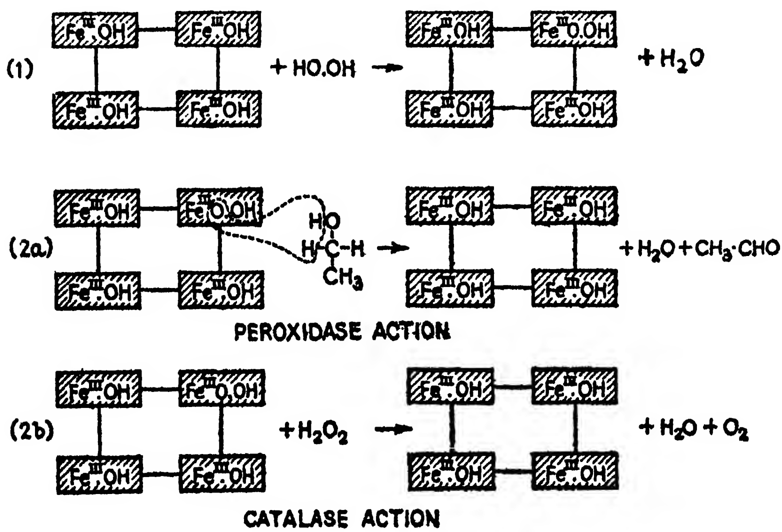
the time for attainment of 50 per cent. deoxygenation is 150 seconds for the perienteric pigment in presence of sodium hydrosulphite ($Na_2S_2O_4$), at 20·5°, *p*H 6, whilst for the body wall hæmoglobin at *p*H 6, 30°, it is 80 seconds; the corresponding figure for sheep hæmoglobin is 0·008 seconds. When *Ascaris* is kept under anaerobic conditions the body wall hæmoglobin becomes deoxygenated, no change can be detected in the perienteric fluid. Similar slow deoxygenation is shown by the hæmoglobin of a species of *Strongylus*, which inhabits the large intestine of the horse (Davenport, *Proc. Roy. Soc.*, 1949, **B136**, 271). The function of such extremely stable oxyhæmoglobins in the organism is obscure; Davenport suggests they may possibly be fortuitous combinations of hæm with proteins of a modified globin type and that they may play no vital role. The hæmoglobin of *Nippostrongylus muris*, a parasite of the brown laboratory rat, is in a different category. This pigment has a strong affinity for oxygen; it is half saturated when in equilibrium with oxygen at less than 0·1 mm. Hg. Its oxygen affinity is similar to that of the hæmoglobin present in the root nodules of leguminous plants. In spite of the high oxygen affinity of *Nippostrongylus* hæmoglobin, the pigment within the parasite is completely and rapidly deoxygenated during incubation under conditions of oxygen deficiency. The pigment is evidently specially fitted to function as an oxygen store or carrier under the peculiar conditions in which the parasite lives.

Some Other Hæm-Protein Compounds

As is well known, certain enzymes and cellular tissue catalysts have been found to be hæm-containing proteins. Both catalase, which brings about the decomposition of hydrogen peroxide into water and oxygen, and peroxidase which promotes the oxidation of suitable substrates by hydrogen peroxide, are of this type. The molecule of peroxidase contains one hæm group; that of catalase contains four. Theorell and Paul (*Arkiv Kemi Mineral. Geol.*, 1944, **A18**, No. 12) have shown that peroxidase may be split into hæm and an apo-protein and these recombine to give the active peroxidase. It has not, however, been possible so far to dissociate catalase in a similar way. In both catalase and peroxidase the iron atom is normally in the ferric state, in contrast to the ferrous state of the iron in hæmoglobin. It is of interest to observe that the inorganic iron ion possesses both catalase and peroxidase activities to a small extent. These are much increased by combination with the porphin nucleus and vastly increased when the resulting hæm is combined with the proper apo-protein. Thus, in the case of

catalase activity, iron porphin is 10^3 times as active, and catalase 10^9 times as active as inorganic iron salts.

Recent work has indicated that peroxidase and catalase are not so clearly distinguishable in their activities as was previously thought. Keilin and Hartree (*Proc. Roy. Soc.*, 1936, **B119**, 141 and *Biochem. J.*, 1945, **39**, 293) have shown that catalase not only catalyses the decomposition of hydrogen peroxide into oxygen and water, but that it also has peroxidase action. This is exhibited when the concentration of hydrogen peroxide in the medium is so low that the ordinary catalase-activity is suppressed. In these circumstances ethyl alcohol is oxidised to acetaldehyde. Light has been shed on the mechanism and kinetics of these reactions in a paper by Chance (*Nature*, 1948, **161**, 914), who shows that, when the concentration of hydrogen peroxide is sufficiently low, one molecule of hydrogen peroxide is taken up by each catalase molecule, that is, by one, but only one of the four hæm groups. If any suitable hydrogen donator is available reaction takes place. If the hydrogen donator is an appropriate alcohol then it is oxidised to the aldehyde, and the hydrogen peroxide combined with the hæm is reduced to water. Here the action of the catalase is that of a peroxidase. If, however, there is excess of hydrogen peroxide, as is normally the case, this functions as hydrogen donator with the formation of oxygen, and the catalase acts in its familiar role. These various steps may be summarised by the following equations :



REPRESENTS A HAEMATIN (OR FERRI-HAEM) GROUP.

Keilin and Hartree (1945, *loc. cit.*) give reasons for believing that under the conditions present in the living cell, where the concentration of peroxide is usually very low, the enzyme may normally act as a peroxidase. In this way it performs the function which is usually attributed to it, namely, that of destroying the toxic hydrogen peroxide produced by various aerobic reactions within the cell. It does this, however, not by the energetically wasteful process of decomposing the toxic substance into oxygen and water, but by using it to promote oxidative reactions useful for the cell's metabolism.

Another group of iron porphin containing compounds of importance in cellular oxidation are the cytochromes, of which the most fully investigated is cytochrome C. This compound, which has been obtained electrophoretically pure, is formed of a protein molecule and an iron porphin derivative which is essentially hæm, of which the vinyl groups have been condensed with two SH groups of the protein. Thus, in effect, the hæm molecule is linked to the protein by two sulphur atoms as well as by the iron atom (through links to two nitrogen atoms of the protein) and also by the two propionic acid groups (see p. 704). Cytochrome C functions in the cell by accepting and giving up electrons, the iron changing reversibly from the ferrous to the ferric state. The ferrous cytochrome is not oxidisable by molecular oxygen; as in the case of hæmoglobin, union of the hæm with the protein inhibits direct oxidation by oxygen molecules. On the other hand, there is no evidence that, like hæmoglobin, it can undergo reversible oxygenation.

Some of the cytochromes found in the cell contain porphins different from protoporphin; thus cytochrome A_3, which according to Keilin and Hartree (*Proc. Roy. Soc.*, 1939, **B127**, 167) may be identical with cytochrome oxidase, contains a porphin resembling spirographin ("chlorocruoroporphyrin"). This is the porphin component of the green chlorocruorine present in the polychæte worm, *Spirographis*. Cytochrome oxidase, as its name implies, allows reduced cytochrome C to be oxidised by molecular oxygen. Presumably it acts by being reduced to the ferrous state by reduced cytochrome C, which so loses an electron and changes from the ferrous to the ferric state. Reduced cytochrome oxidase is directly oxidised by molecular oxygen in marked contrast to cytochrome C, and thus reverts to the ferric state ready to oxidise more cytochrome C. Compounds of the cytochrome group appear to be universally distributed in all cells capable of aerobic metabolism.

Structure of Hæm-Protein Compounds

Various physical methods have been used to obtain information on the state of the iron atom in these hæm-protein compounds and also on the mechanisms of the linkage between hæm and protein. These have recently been reviewed by Theorell (*Adv. Enzym.*, 1947, **7**, 265). Much pioneering work has been done in this field both by Theorell and by Pauling and his co-workers. The chief methods employed have been the study of the absorption bands, the measurement of magnetic susceptibility, examination of titration curves and of the amino-acid composition of the protein components. The magnetic susceptibility indicates the number of unpaired electrons on the iron atom. In the ferrous ion this is four, in the ferric ion five. In hæmoglobin and methæmoglobin it is also four and five respectively. As co-ordination with porphin and protein does not alter the number of unpaired electrons in hæmoglobin and methæmoglobin, it would appear that the iron bonds with the rest of the molecule are essentially ionic, and that true sharing of electrons with the nitrogen atoms does not occur. In oxyhæmoglobin, which is diamagnetic, the number of unpaired electrons is apparently zero, so that not only have the four odd electrons of the iron become paired, but also the two unpaired electrons of the paramagnetic oxygen molecule. The magnetic properties of hæm derivatives is the subject of a recent review by Hartree (*Ann. Rep. Chem. Soc.*, 1946, **43**, 287), which includes a table showing the number of unpaired electrons in a large number of iron-porphin derivatives and may be consulted for further details.

One of the most interesting questions is how the hæm group is attached to the protein in these hæm-protein molecules. The iron atom clearly plays an important part in this combination. This atom can form a maximum of six co-ordinate bonds octahedrally arranged, that is in the six directions along three axes mutually perpendicular to each other. The porphin nucleus is plane and in the iron porphins the iron atom is attached by four bonds in one plane to each of the four nitrogen atoms leaving two possible co-ordination positions at right angles to this plane. As the iron ion takes the place of two protons, in ferrous hæm the net electric charge is zero, and in ferric hæm one unit of positive electricity. In ferric hæmochromogens one of these positions may be occupied by a negative ion, the other by a nitrogen atom of an organic base, ammonia or some such compound. Alternatively, both co-ordinating positions may be occupied by nitrogen, the resulting complex ferric hæmochromogen having one positive charge. In ferrous

hæmochromogens, the two positions are occupied by two nitrogen atoms, *e.g.* the nitrogen atoms of two pyridine molecules. In this way pyridine hæmochromogen is formed. In hæmoglobin we are to regard the iron atom of each of the four hæm molecules as linked to the globin by means of a nitrogen atom of the globin co-ordinating with it. The other co-ordination position is readily filled by an oxygen molecule; even more readily by a molecule of carbon monoxide. Thus we have oxy- and carboxy-hæmoglobin. The atom of the globin which co-ordinates with the iron atom is probably a nitrogen atom contained in an iminazole ring of histidine. This "iminazole theory" fits in well with many observations made by Pauling and his colleagues, particularly on the titration curves of hæmoglobin and its derivatives.

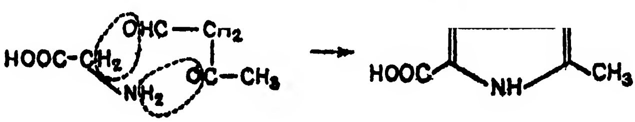
Many iron porphins varying from hæm in the nature and arrangement of the side chains can combine with globin, forming modified hæmoglobins. To do this, however, they must possess one characteristic feature of protoporphin, namely two propionic acid groups as side chains in prescribed positions. This suggests that these two ionised groups, bearing negative charges, play an important function in the binding of the hæm molecule to the globin. Presumably they are attracted by Coulomb forces to some positively charged groups in the globin molecule and serve both to anchor the hæm and orient it in the correct position, thus enabling the iron to form its co-ordinate link with nitrogen. A few details may be added which may help to give more definite form to our picture of the hæmoglobin molecule. The dimensions of the molecule of horse hæmoglobin are $64 \times 36 \times 48$ Å. The four planar hæm molecules are parallel to each other, probably two on the top surface and two on the bottom, or perhaps all four in one plane. The exact structure of the globin protein is still incompletely elucidated, but according to Boyes-Watson and Perutz (*Nature*, 1943, **151**, 714) each globin molecule may consist of four monolayers, each layer involving four folded polypeptide chains (see also Bragg, *Nature*, 1949, **164**, 7). The elucidation of the further details of the structure of the globin component is part of the general problem of protein structure, in which considerable progress is being made, but whose final solution is still far off.

Synthesis of Porphin Nucleus

In recent years considerable light has been thrown on the precursors of porphin nuclei in the cell by work with labelled nitrogen, carbon and hydrogen. That acetic acid takes part in porphin synthesis in the animal organism was indicated by the work of

Bloch and Rittenberg (*J. Biol. Chem.*, 1945, **159**, 45), who showed that rats fed with deuteroacetic acid incorporated deuterium into the hæm of their hæmoglobin. Shemin and Rittenberg (*J. Biol. Chem.*, 1945, **159**, 567 and 1946, **166**, 621 and 627), working with glycine of which the nitrogen was labelled with N^{15}, showed that this amino acid functions as a source of nitrogen in the synthesis of hæmin. Other amino acids, such as glutamic acid, proline and leucine, do not act directly in this way but enter into hæmin synthesis only indirectly by contributing to the general nitrogen stock within the body. It has also been reported (Shemin, London and Rittenberg, *J. Biol. Chem.*, 1948, **173**, 797, 799) that hæm synthesis involving the nitrogen of glycine occurs *in vitro* in the presence of the nucleated erythrocytes of avian (duck) blood, and with blood from patients with sickle cell anæmia. Experiments with normal and anæmic rats fed with glycine, labelled in the α-position with C^{14}, show that this α-carbon atom also enters into the hæmin molecule (Altman, Casarett, Master, Noonan and Salomon, *J. Biol. Chem.*, 1948, **176**, 319, and *Federation Proc.*, 1948, **7**, 2).

Possibly the synthesis of the pyrrole rings of porphin takes place through a reaction similar to that described by Fischer and Fink (*Z. physiol. Chem.*, 1944, **280**, 123), who found that, at room temperature and pH8, formyl acetone and glycine react to give a pyrrole derivative presumably according to the equation



It would seem that a pyrrole derivative is formed in the body from glycine and a β-keto-aldehyde, and then undergoes decarboxylation, so that the carboxy carbon of the glycine is not found in the final porphin molecule.

BOTANY. By F. W. Jane, Ph.D., D.Sc., University College, London.

The Structure of the Plant Cell Wall with Special Reference to Woody Tissues.—Among the extensive literature on various aspects of cell wall structure in plants are two useful summaries, an elementary treatment by Tiemann (*Wood Technology*, London, 1944 and 1947) and the more detailed paper by S. H. Clarke (*Nature*, **142**, 899, 1938). Clarke made no attempt to summarise completely the relevant literature, but sought to give as full a picture as possible of the structure of the cell wall, as revealed by

modern investigations. Completeness is not, in fact, possible, for investigations have relied largely on visual methods, which are limited to particles larger than about 2500 A., and to X-ray technique which is applicable to much smaller particles: the gap between is filled largely by conjecture and inference.

The picture visualised is of long chain molecules of cellulose, composed of large numbers of glucose residues, lying in regular zones or bundles in which they are more or less, but not completely parallel to one another: adjacent bundles or micelles are linked by molecules extending between them. The spaces between these micelles are occupied, in woody cell walls, by lignin and other substances, which, since they do not affect the X-ray diffraction pattern, are presumed to be non-crystalline. These intermicellar materials increase the rigidity of the wall and its resistance to compression, but appear to have little, if any, influence on its tensile strength.

Concerning the microscopic structure of the wall, Clarke uses the nomenclature of Kerr and Bailey (*J. Arnold Arboretum*, **15**, 327–49, 1934), dividing the wall into three parts. Outermost is the very thin middle lamella, composed in the main, if not entirely, of polyuronides; this layer is isotropic. There follows the primary wall, which is formed by the cambial initial and which is anisotropic. It consists of cellulose, hemicelluloses and pectic materials, and is formed during the period of extension growth. It is believed to have relatively slender cellulose aggregates with a somewhat irregular arrangement; as a consequence, the intermicellar spaces are large, a feature which is in harmony with the fact that this layer is more heavily lignified than the secondary wall. The innermost of the three layers is formed by the secondary wall, which is deposited after extension growth has ceased. It consists of cellulose or mixtures of cellulose, hemicelluloses, pentosans, lignin and other substances. At least two zones may normally be distinguished, an outer and a much wider middle one; a third or inner zone is often present, differing in composition, sometimes markedly, from the middle zone; this is referred to by some authors as the tertiary wall. These three regions of the secondary wall are often optically distinct, and it is held that the difference is due to the orientation of the cellulose molecules and their aggregates, those of the inner and outer zones forming a flat spiral, while in the middle zone they are more or less parallel to the longitudinal axis of the cell.

In some cells there is no secondary wall, but elsewhere it may be well developed and exhibit a complicated zonation. In fibres it has been shown that zonation may be due to alteration of inclina-

tion of the micelles in successive layers, this affecting the optical anisotropy of the cell wall. Again, zoning may depend on variations in the degree of lignification. This zonation of the secondary wall must be distinguished from the very thin lamellæ which may be demonstrated in some walls by special methods. Such lamellæ, in the cotton hair, may be less than 400 A. thick, and consist of two zones of different optical density: they are held to be caused by diurnal fluctuations of light and temperature during the period of growth (Anderson and Kerr, *Ind. Eng. Chem.*, **30**, 48–54, 1938): perhaps lamellæ in wood fibres are due to the same causes.

The gap between the visible structure of the cell wall and that revealed by other methods remains to be considered. It is possible to break up the cellulose wall into thin, thread-like fibres of indefinite length and perhaps between 1000 A. and 5000 A. in diameter, while these fibrils may be further divided into dermatosomes or fusiform bodies. There is a good deal of conflicting evidence as to the nature of these bodies, which is not surprising since they have been obtained by harsh chemical and mechanical treatments of the walls and from a variety of plant cells. Bailey and Kerr (*J. Arnold Arboretum*, **16**, 272–300, 1935) go so far as to claim that there is no reliable evidence to indicate that the matrix of the secondary wall is composed of discrete bodies of visible size, and that if there are discontinuities in the structural pattern they are to be found rather in the realm of the micelles or molecular chains. At this end of the picture X-ray evidence leads to the conclusion that the micelles are aggregated into microfibrils which, in turn, are grouped into fibrils.

When a transverse section of a cell like a conifer tracheid is examined under crossed nicols the middle layer of the secondary wall is dark, the inner and outer layers bright. This optical appearance was explained by Bailey and others (*loc. cit.*) as due to the orientation of the micelles, as has already been mentioned. In later papers—Bailey and Vestal (*J. Arnold Arboretum*, **18**, 185, 1937), Bailey and Berkeley (*Am. Journ. Bot.*, **29**, 231, 1942)—it is inferred that the micelles of inner and outer layers of the secondary wall are oriented at 45° to the longitudinal axis of the cell and those of the middle layer at 90°. Preston, however (*Proc. Roy. Soc. B.*, **133**, 327, 1946 and **134**, 202, 1947), held that the primary wall layer of conifer tracheids is the only one in which the cellulose chains lie approximately transversely to the longitudinal axis and that, in other layers, they are in a fairly steep spiral, *i.e.* that in the whole of the secondary wall the chain direction was the same: while he regarded the general chain direction in relation to the

longitudinal axis as almost constant, he held that the optical properties were explicable on different types of angular dispersion of the micelles about the spiral axis. Subsequently Preston (*Biochimica et Biophysica Acta*, **2**, 370–83, 1948) has found indications of a narrow outer lamella in the secondary wall in which the spiral is less steeply oriented than elsewhere: it is held that failure to interpret the fine structure of this layer correctly was due to considerable angular dispersion of its micelles. Wardrop and Preston (*Nature*, **160**, 911, 1947) sought to test this question of orientation of the micelles by a critical experiment. They cut sections of spruce tracheids and fibres of *Nothofagus Cunninghamii* at increasing angles to the radial longitudinal plane and observed the birefringence in the several sections, arguing that this was equivalent to studying birefringence in a transverse section of a cell rotated about a line perpendicular to the direction of observation and to one wall surface. The results of their observations led them to conclude that, in the secondary wall, the outer layers have the micelles in the form of a flat spiral, but not lying transversely, while in the middle layers they occur in a much steeper spiral: there is, however, considerable angular dispersion of the micelles within these layers. They regard the optical heterogeneity as due both to the spiral arrangement of the micelles and to their angular dispersion.

Summarising present views on the submicroscopic structure of the wall, Wardrop and Dadswell (*Austr. Journ. Scientific Research B.*, **1**, 3–16, 1948) figure the primary wall of the wood fibres of *Eucalyptus regnans* as having the micelles arranged almost transversely to the longitudinal axis of the fibre, while in the secondary wall the direction of those of the middle layer approximates to that of the longitudinal axis; in the inner and outer layers the micelles are more inclined, although less so than those in the primary wall.

No visual observations of the micelles have, of course, been possible on account of their small dimensions. Recently, however, a study of the cellulose of the wall of the green alga, *Valonia ventricosa* by Preston, Nicolai, Reed and Millard (*Nature*, **162**, 665, Oct. 1948), by means of the electron microscope, has amply confirmed the conclusions previously arrived at by indirect methods. Previously, as these investigators point out, there has been no proof that the newer physical methods have yielded a more precise picture of the submicroscopic structure than had been arrived at by earlier workers as a result of microscopic examination. Proof was lacking that the wall, intact and untreated, did in fact consist of such units. For the preparation of photographs by the electron microscope untreated cell walls were required, of which the fine

structure was known in enough detail to make interpretation possible. The photographs of the wall of *Valonia* show fibres of indefinite length with diameters which are pretty constant around 300 A. These fibres run, with rare exceptions, in one direction in each lamella, except for odd ones which have probably been displaced during preparation. This structure accords with that deduced from X-ray photographs. Where fibres of two lamellae can be seen the orientation of the one set in relation to the other is rather less than a right angle; at times it is considerably less. These fibres perhaps represent the crystallites or micelles of cellulose; so far no evidence has been obtained to indicate whether they are rod-like or of the form of flattened ribbons.

Considerable attention has recently been devoted to the structure of compression wood and tension wood. The former, found mostly on the lower side of branches and leaning trunks of conifers, has been carefully examined by Luxford and Pillow (*U.S. Dept. of Agric., Tech. Bull.* 546, 1937). It is characterised by excessive hardness, high and irregular longitudinal shrinkage in drying, a dull lifeless appearance and a short brashy fracture: it is lower in practically all strength properties than normal wood and has a somewhat higher lignin and a lower cellulose content. Microscopically it may be distinguished by its rounded tracheids, with intervening intercellular spaces, as compared with the closely fitting tracheids of normal wood, and by the presence of oblique checks on the walls of the tracheids. It would appear that the slope of the fibrils in the wall is greater than in normal wood. Phillips (*Empire Forest. Journ.*, **19**, 282–8, 1940), examining compression wood in *Juniperus procera*, concluded that plantation grown trees contain only about a third as much compression wood as forest grown ones: he relates its development mainly to the wind and in forest trees found that it occurs mainly on the side opposite to the prevailing wind. In more sheltered plantation trees there appears to be much less relation between the formation of compression wood and the direction of the wind.

Tension wood is found most commonly on the upper side of leaning boles in broad-leaved trees and is associated, like compression wood, with eccentric growth of the stem. Whether there is any relationship between this type of wood and compression wood, either in the structure or the composition of the woody elements, or in origin, is not yet clear. Chow (*Forestry*, **20**, 62–77, 1946) has made extensive comparisons between normal and tension wood in beech. He finds that chlor-zinc-iodine gives normal wood a yellow colour and tension wood a dark brown or deep violet

shade which accords with the findings of Clarke (*Forestry*, **11**, 85–91, 1937) that there is a lower lignin content in the fibres of tension wood than of normal wood. This reagent produces a greater swelling of the fibre walls of tension wood than in those of normal wood. Chow finds that tension wood has a significantly lower vessel volume and a correspondingly higher fibre and parenchyma volume than normal wood and that its fibres tend to be longer but narrower and with appreciably thicker walls: moreover, tension wood has smaller vessels, due to a smaller radial—but not tangential—diameter; its vessels are generally less numerous than those of normal wood. He also notes that tension wood fibres show short, discontinuous fibrous markings on their cell walls, which are rendered more distinct by examination between crossed nicols: such markings he believes to be incipient tension failures caused by mechanical forces acting on the wood in the tree: such "failures" are more pronounced in strongly developed tension wood. This type of wood is heavier than normal wood and has an appreciably higher shrinkage in drying. While its lignin content is significantly lower than that of normal wood, analyses indicate that the proportion of cellulose of relatively long molecular chain length is higher. Chow concludes that in tension wood the cellulose chains are relatively more highly oriented in the direction of the longitudinal axis of the fibres, a view which is confirmed by X-ray data obtained in Preston's laboratory. It is argued that tension wood should, as a consequence, be stronger in tension than normal wood; the fact that this is not so is explained by assuming that the spiral markings on the fibre walls are, in fact, incipient tension failures brought about by abnormal conditions of growth in the standing tree. It is difficult to account for the abnormally high longitudinal shrinkage, for this would be expected if the cellulose fibrils formed a flat spiral; a tentative suggestion is that it is due to the closure of the spiral markings in the walls, which might be regarded as spiral cracks.

Further studies of tension wood have been made by Wardrop and Dadswell (*loc. cit.*) who have investigated the phenomenon in two species of *Eucalyptus*, *Nothofagus Cunninghamii* and *Acacia melanoxylon*. They find that in tension wood the tertiary layer of the wall, *i.e.* the inner layer of the secondary wall, is strongly developed but relatively unlignified: this results in a characteristic appearance in transverse section which has led to the use by some authors of the term "gelatinous fibres" for the fibres of tension wood. Chemical analyses show that in *Eucalyptus regnans* tension wood fibres and normal wood fibres contain, *relatively*, equal pro-

portions of lignin, although staining indicates that the tertiary layer of the walls in tension wood is unlignified. It appears, moreover, that the ratio of crystalline cellulose to amorphous material in this layer may be higher in tension wood than in normal wood and that tension wood consists of very pure cellulose of a very highly oriented form. Not only is the micellar spiral in the tertiary layer much more steeply inclined than in the inner layer of the secondary wall but the pitch of the spiral in the outer layer is also somewhat steeper than in normal wood. Wardrop and Dadswell regard the spiral markings on the walls of the tension wood fibres—which Chow considers may be cracks—as incipient slip planes and compression failures, perhaps caused by buckling of the micelles by reason of their relatively great length as compared with their diameter. Such buckling might easily occur in the unlignified fibres of tension wood, whereas in a normal fibre, with its lignin packing in the intermicellar spaces, buckling is less likely to take place.

An attempt has been made by several investigators to use the different optical properties of cellulose, and lignin and pectin, for studying diseased wood (*cf.* Cartwright and Findlay, *Decay of Timber and its Prevention*, London, 1946). If the bulk of the cellulose has been removed from the wall by a wood-destroying fungus of the brown rot type, then the wall should appear relatively dark between crossed nicols. While this method does enable the progressive depletion of cellulose from the wall to be traced in different elements of the wood, its drawback is that even at a stage of decay which is obvious to the unaided eye, sufficient may remain for the wall to show double refraction. Since, however, decay is rarely uniform, areas of severe decay are rendered obvious by the use of polarised light. In white rots, which primarily attack the lignin, even walls in an advanced stage of decay will remain doubly refractive. X-ray interference photographs of wood attacked by a brown rot differ from those of sound wood, although differences are not apparent until the wood has reached an advanced stage of decay.

PLANT PHYSIOLOGY.

By Professor Walter Stiles, Sc.D., F.R.S.
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Nitrogen Assimilation.—In connection with a general investigation of the nitrogen metabolism of the deadly nightshade, *Atropa belladonna*, W. O. James, E. A. H. Roberts, H. Beevers and P. C.

de Kock have examined the way in which the catechol oxidase system of this plant effects the oxidation of amino-acids in the presence of phenols ("The Secondary Oxidation of Amino-acids by the Catechol Oxidase of Belladonna," *Biochem. J.*, **43**, 626–36, 1948). To this end a preparation was obtained from the powdered shoots which was freed from amino-acids by successive washings with water but which possessed considerable polyphenolase activity. This powder rapidly oxidised catechol, phloroglucinol, *p*-cresol, adrenaline, *N*-methyl adrenaline and æsculetin; it oxidised hydroquinone and gallic acid less rapidly and æsculin very slowly; it did not oxidise resorcinol. The oxidation of catechol was little influenced by acidity over a range of *p*H from 4·5 to 8·3 but that of adrenaline was markedly affected, the oxygen uptake in 40 minutes, for example, increasing with the same quantity of reactants from 18μl. at *p*H 4·1 to 112 μl. at *p*H 7·8. The secondary oxidation of a number of amino-acids by the enzyme preparation was examined. In the presence of the enzyme preparation alone the amino-acid glycine was not oxidised, but in presence of catechol oxidation of glycine occurred, with uptake of additional oxygen and release of ammonia, the volume of ammonia produced being approximately twice that of the additional oxygen absorbed. The rate of oxygen consumption related to amino-acid oxidation was, however, slow in comparison with the initial rate of catechol oxidation, but continued for hours after the oxidation of catechol was practically complete. This time lag in the development of the oxidation of the amino-acid suggests that the oxidation is not effected by a simple catechol oxidation product such as *o*-benzoquinone.

The relative quantities of oxygen consumed and ammonia released in the oxidation of the amino-acids suggests that the $CH(NH_2)$ group of the amino acid is oxidised to CO thus:

$$R.CH(NH_2).COOH + \tfrac{1}{2}O_2 \rightarrow R.CO.COOH + NH_3$$

so that, in the particular case of glycine, glyoxylic acid should be formed

$$CH_2(NH_2).COOH + \tfrac{1}{2}O_2 \rightarrow H.CO.COOH + NH_3.$$

This substance was actually isolated in sufficient quantity for its identification.

Other amino-acids, including alanine, were oxidised much more slowly than glycine, but the expected oxidation product of alanine, pyruvic acid, was identified in experiments with this amino-acid. Some amino-acids were not oxidised at all.

The secondary oxidation of amino-acid was preceded by the formation of an intense red colour, probably due to a *p*-amino-

o-quinone, which it is thought is the immediate oxidiser of the amino-acid. It is suggested that the oxidase system brings about the formation of this substance and possibly its regeneration after reduction by the amino-acid, but that the actual oxidation of the amino-acid and simultaneous reduction of the red complex is a non-enzymic action.

Similar experiments, in which secondary and tertiary amines were employed in place of amino-acids, have been described by H. Beevers and W. O. James ("The Behaviour of Secondary and Tertiary Amines in the Presence of Catechol and Belladonna Catechol Oxidase," *Biochem. J.*, **43**, 636–9, 1948). It was observed that when secondary amines, such as dimethylamine, proline, sarcosine and nortropine, were added to a preparation of Belladonna catechol oxidase in presence of catechol there was a development of deep royal purple colours. This was, however, not accompanied nor succeeded by any oxygen absorption attributable to the presence of the amine. It would thus appear that secondary amines are capable of forming compounds similar to those produced by amino-acids in presence of catechol and catechol oxidase, but that these compounds do not effect the oxidation of amines. It was, however, found that they would oxidise glycine as vigorously as the red compound formed by primary amino-acid. It may be noted that a compound with a *p*-amino-*o*-quinone structure could be formed with a secondary amine as well as with a primary amine or amino-acid, but would not be possible with a tertiary amine, nor indeed were coloured compounds formed with these.

Earlier investigations on the effect of oxygen on nitrate assimilation by higher plants have led investigators to diverse conclusions. Thus, whereas Eckerson and Burström concluded that oxygen was necessary for nitrate assimilation, Shive and his collaborators considered that oxygen had an inhibitory action on this process. Some fresh work bearing on this subject has now been reported by J. F. Nance ("The Role of Oxygen in Nitrate Assimilation by Wheat Roots," *Amer. J. Bot.*, **35**, 602–6, 1948). Excised roots were grown in culture solutions containing potassium nitrate, sucrose and manganese sulphate, some of the cultures being flushed with nitrogen and sealed off, while the others were aerated. The nitrate content of the solutions and roots was determined after 31 hours, when it was found that, although nearly ten times as much nitrate had been absorbed by the roots in the aerated solutions as by the roots in nitrogen, the percentage of the absorbed nitrogen assimilated by the former was only 32 per cent. as compared with 76 per cent. assimilated by the roots in nitrogen. In further experiments roots

were allowed to absorb nitrate from the culture solution for 4 hours and then transferred to a solution containing sucrose, calcium sulphate and manganese sulphate for 12 hours. When the latter solution was aerated 4·2 per cent. of the nitrate present in the roots was assimilated, but in solutions in contact with nitrogen 42·1 per cent. of the nitrate was assimilated. In absence of calcium sulphate 17·8 per cent. of the nitrate was assimilated by the aerated cultures. It thus appears that although oxygen furthers the absorption of nitrate by wheat roots it inhibits the assimilation of the nitrate after its absorption in excised seedling roots. In agreement with a previous finding of Burström, Nance found that in macerated roots oxygen brings about an increase in the assimilation of previously absorbed nitrate.

Nance suggests that the inhibition of nitrate assimilation by oxygen is related to the inhibition of fermentation by oxygen, and he supposes that the systems involved in anaerobic respiration use nitrate as a terminal oxidant. The assimilation of nitrate under aerobic conditions would then be associated with aerobic fermentation processes which, it has been alleged, are characteristic of actively growing plant tissues such as meristems, including root tips.

The absorption and translocation of nitrogen using the isotope N^{15} contained in ammonium sulphate has been examined by R. MacVicar and R. H. Burris ("Translocation Studies in Tomato using Ammonium Sulfate labeled with N^{15}," *Amer. J. Bot.*, **35**, 567–70, 1948). In their experiments there was almost immediate absorption of the labelled ammonium ion by the roots of young plants, and rapid absorption continued for about 10 hours. After a slowing down in the rate of absorption during the next 10 hours, the amount of labelled nitrogen in the roots increased steadily from the 20th to the 60th hour. This continued increase in the quantity of N^{15} suggests either synthesis of new protein or an interchange of labelled nitrogen for the nitrogen previously present in existing protein. The fact that in the roots of older (fruiting) plants there is little change in the concentration of N^{15} after 6 hours is explained by the fact that these mature plants would not have a rapidly developing root system and so would not incorporate much new nitrogen into their system.

Determinations of N^{15} in leaves and stems showed that the ammonium ion was rapidly transported to the aerial parts of the plant, a detectable amount of N^{15} being found in these organs after two hours. In young plants the amount of N^{15} in stems and leaves increased regularly up to the 60th hour. In older fruiting plants, however, while a considerable amount of N^{15} reached the lower part

of the stem, comparatively little reached the upper part of the stem. The results of this investigation are regarded as indicating that the absorbed ammonium ion is probably converted to organic nitrogenous compounds which are transported to actively metabolising regions, although the transport of ammonium ions and the formation of organic nitrogenous substances in these regions is possible.

As part of a study of the metabolism of members of the Crassulaceæ, G. W. Pucher, C. S. Leavenworth, W. G. Ginter and H. B. Vickery have examined the effect of different kinds of nitrogen supply on the composition of *Bryophyllum calycinum* ("Studies in the Metabolism of Crassulacean Plants; the Effect upon the composition of *Bryophyllum calycinum* on the Form in which Nitrogen is supplied," *Plant Physiol.*, **22**, 205–27, 1947). Like other succulents *Bryophyllum* exhibits a diurnal variation in its content of organic acids, particularly malic acid, this increasing during the night and decreasing during the day. When the plant is supplied with nitrate the increase in organic acid content is much greater than when the nitrogen is supplied as an ammonium salt. Pucher and his co-workers have now made analyses of the leaves and stems of plants of *Bryophyllum calycinum* supplied with solutions containing a constant concentration of nitrogen, but in which the nitrogen was supplied as a mixture of nitrates and ammonium salts, the proportion of the ammonium nitrogen in the different culture solutions being respectively 0, 50, 75 and 100 per cent. With increase in the proportion of ammonium in the solution the absolute quantity in all the leaves of a plant of organic solids, water, total nitrogen, nitrate nitrogen, organic soluble nitrogen, total organic acids, starch, crude fibre and fermentable carbohydrate all diminished. Protein nitrogen, ammonia nitrogen and glutamine reached a maximum with about 50 per cent. of the nitrogen supplied as ammonium salts, while asparagine reached a minimum with the same proportion of supplied ammonium salt. The relationship of the various fractions in the stem to the supply of nitrate and ammonium was similar to that found for the leaves, except that ammonia nitrogen and glutamine increased continuously with increase in the proportion of nitrogen supplied as ammonium.

The smaller amount of organic acid in plants supplied with ammonium nitrogen, as compared with those supplied with nitrate nitrogen, affects malic and isocitric acids equally. The absolute reduction in the amount of citric acid is less, but the proportionate reduction is greater.

The utilisation of various amino-acids by orchid embryos has been examined by E. Spoerl ("Amino Acids as Sources of Nitrogen

for Orchid Embryos," *Amer. J. Bot.*, **35**, 88–95, 1948). The orchid embryo is a useful object of experimentation for work of this kind, since it consists only of an undifferentiated mass of cells with no storage tissue. Nineteen different amino-acids were used as sources of nitrogen in this investigation, and experiments in which nitrogen was supplied as ammonium sulphate and in which no nitrogen at all was provided were carried out as controls. Experiments were made with different concentrations of amino-acid (mostly containing $0{\cdot}006M$ and $0{\cdot}0006M$ nitrogen) and under both light and dark conditions. Using the very young embryos of unripe seeds, it was found that good growth occurred with arginine but with none of the other amino-acids employed, all of which inhibited growth under at least one of the conditions employed. Germination appeared to be inhibited more than subsequent development. With the older embryos of ripe seeds aspartic acid, brought about good growth. All the other amino-acids examined, with the exception of glutamic acid, inhibited growth under at least one of the conditions of experimentation. While glutamic acid did not inhibit growth, embryos supplied with it showed no appreciably better growth than embryos supplied with no nitrogen. With cysteine and cystine growth of the embryos was better in the dark than in light, but with most of the amino-acids used, as well as when no nitrogen was supplied, the reverse effect was observed.

The question of the loss of gaseous nitrogen from green plants has been examined in two papers by F. R. Allison and his co-workers. In the first of these F. R. Allison, K. S. Love, L. A. Pinck and V. L. Gaddy describe experiments with *Chlorella* and *Lemna* grown on artificial culture media containing various inorganic and organic nitrogen compounds ("Gaseous Losses of Nitrogen from Green Plants. I. Studies with *Chlorella* and *Lemna*," *Plant Physiol.*, **23**, 496–504, 1948). With most of the sources of nitrogen employed no significant loss of nitrogen occurred in the *Chlorella* cultures, whether these were grown in the light or dark. A loss of nitrogen was, however, observed with *Chlorella* cultures supplied with ammonium nitrate, although the loss averaged only about 5 per cent. of the nitrogen supplied. The loss occurred in cultures that were dying as a result of marked increase in the acidity of the medium to about *p*H4, and it is concluded that no loss occurred under more normal conditions of acidity.

Few experiments were carried out with *Lemna* owing to the sensitiveness of the plants to the reaction of the medium, which made it difficult to grow them on media containing a number of nitrogen compounds. Excellent growth was, however, obtained in

culture media containing sodium nitrate, potassium nitrate and arginine, but with none of these was there any loss of gaseous nitrogen.

In the second paper F. E. Allison and L. De T. Sterling ("Gaseous Losses of Nitrogen from Green Leaves. II. Studies with Excised Leaves in Nutrient Media," *Plant Physiol.*, **23**, 601–8, 1948) describe experiments in which leaves of *Iris, Belamcanda* and *Narcissus* were floated on nutrient media and exposed to light for 60 to 84 hours. Nitrogen was supplied in the media as ammonium nitrate, potassium nitrate and ammonium chloride. Although the leaves absorbed nitrogen from all the media employed no loss of gaseous nitrogen was observed in any instance.

F. G. Viets, E. I. Whitehead and A. L. Moxon have reported on the changes which occur in the content of the various nitrogen fractions in detached leaves of maize with the sheaths kept in a 0·005N solution of calcium sulphate ("Nitrogen Metabolism of Detached Corn Leaves in Darkness and in Light," *Plant Physiol.*, **22**, 465–76, 1947). Leaves were analysed for sugars and various nitrogen fractions at intervals of three days. In the dark, reducing sugars and sucrose were rapidly utilised, but in the light the content of reducing sugars remained approximately constant, while the amount of sucrose increased. As regards nitrogen, protein hydrolysis proceeded at about the same rate in light and in darkness, residual α-amino acids, aspargine, glutamine and ammonia accumulating in the leaves. There was some accumulation of peptides in the early stages of protein hydrolysis in the light, but these were later hydrolysed. Much more glutamine was synthesised in the dark than in the light and was not related to the content of amino-acids.

The similarity of protein breakdown in light and in darkness is to be noted. The formation of the products of photosynthesis in the light does not prevent the breakdown of protein in detached leaves, and it would appear that synthesis of proteins is controlled by some other part of the plant. Thus it has been suggested that the roots may be concerned in supplying some factor to the leaves necessary for amino-acid and protein formation.

A series of sand culture experiments to determine the effects of three levels of nitrogen and three of phosphorus and their interrelationships on the growth and composition of seedlings of *Cinchona* has been described by A. J. Loustalot and H. F. Winters ("The Effect of Three Factorial Levels of Nitrogen and Phosphorus on the Growth and Composition of *Cinchona ledgeriana*," *Plant Physiol.*, **23**, 343–50, 1948). The nutrient solutions contained nitrogen in the respective concentrations of 3, 18, and 81 parts per million

(p.p.m.) and phosphorus in the concentrations 0, 5 and 25 p.p.m. As each nitrogen level was combined with each phosphorus level, there were thus nine different treatments. The growth of seedlings receiving the lowest amount of nitrogen was very poor, but there was no significant difference between plants receiving the medium and largest amounts of nitrogen. The growth of plants receiving nitrogen at the highest level increased with increase in the phosphorus supply, but phosphorus supplied at the highest level considerably depressed the growth of plants receiving nitrogen at the lowest level and also depressed, although to a lesser degree, those with a medium supply of nitrogen. The roots and stems of plants receiving nitrogen at the highest level contained more total alkaloid and quinine sulphate than those of plants supplied with less nitrogen. Leaves contained a higher nitrogen content the higher the level at which nitrogen was supplied, but as the phosphorus supply increased the nitrogen content of the leaves diminished, so that with high phosphorus supply the nitrogen deficiency of leaves of plants receiving the smallest nitrogen supply was emphasised. It is thus important to maintain a correct balance between nitrogen and phosphorus and possibly other mineral nutrients in order to obtain maximum growth.

The importance of an adequate nitrogen supply for alkaloid-forming plants has also been emphasised by G. M. James (" Effects of Manuring on Growth and Alkaloid Content of Medicinal Plants," *Economic Bot.*, **1**, 230–7, 1947), who states that it is hardly possible to over-manure with any nitrogenous fertiliser on soils containing a proportion of clay and lime. Ammonium sulphate may, however, cause damage and a decrease in yield if supplied in two high a concentration, though even then the alkaloid content may be increased. Mrs. James also mentions the importance of a proper balance between the main nutrient elements to obtain the best yield.

ENTOMOLOGY.

By A. D. Lees, M.A., Ph.D., Agricultural Research Council, Unit of Insect Physiology, Cambridge.

The Eradication and Control of Mosquitoes.—The recently published *Proceedings of the 4th International Congresses of Tropical Medicine and Malaria* (Vols. 1 and 2, Washington, 1948) provide striking testimony of an almost world-wide recession of malaria. Remarkable though the advances in other directions have been—in the field of therapeutics, for example—the large-scale control of malaria has proved to be an entomological problem. With the

new insecticides, particularly DDT, playing a decisive role, malariologists have sought either to reduce the population density of the mosquito carrier below the numerical threshold necessary for malaria transmission (control or "species sanitation"); or, where the geographical situation has permitted, to eliminate the species from the affected area (species or genus eradication). These aims and their application in differing circumstances are discussed by F. L. Soper (*Proceedings*, **1**, 850).

Species eradication has as yet been undertaken only in areas where some natural barrier to reinfestation exists. Its possibilities were first demonstrated by the eviction of the African mosquito, *Anopheles gambiæ*, from Brazil in 1940. The story of this campaign, in which paris green was used as a larvicide, has been told by F. L. Soper and D. B. Wilson in their book "*Anopheles gambiæ in Brazil 1930 to 1940*" (1943, Rockefeller Foundation).

The history of *A. gambiæ* as an insect invader was repeated in 1941 when the species was recorded for the first time at many points along the Nile Valley and within 200 miles of Cairo. The control measures undertaken by the Egyptian Government in consultation with the Rockefeller Foundation were organised on the lines of the Brazilian campaign and were designed to protect Lower Egypt and the Delta from invasion (A. T. Shousha, 1948, *Bull. World Hlth. Org.*, **1**, 309). But before their full effects were apparent Upper Egypt was struck by the disastrous epidemic of 1942–43. The spread of *gambiæ* had been halted, however, and eradication from the 500 miles of infested valley was completed by 1945. In the Sudan similar measures were successful in again confining *gambiæ* to territory above the Second Cataract.

Certain factors are considered to have favoured the successful conclusion of these campaigns. For example, in Africa *A. gambiæ* is both a domestic and a forest insect; in Brazil it failed to reach forested areas where control might have been more difficult. And in Egypt the winter temperatures are too low for the species to proliferate freely. Moreover, the habits which make this species particularly dangerous, namely its preference for breeding in shallow sunlit pools near human habitations, also render it comparatively vulnerable to attack with larvicides. On the other hand, the eradication of indigenous mosquitoes, often much less domestic in their breeding habits and less restricted to man as a host, presents a far more difficult problem. Thus the anti-*gambiæ* measures in the Nile Valley reduced the density of indigenous anophelines without eliminating them; and an attempt to eradicate *A. pharœnsis*, the most dangerous native species, was eventually abandoned.

Nevertheless, in other, more isolated, areas—particularly islands—remarkable progress has been made.

In the Kharga and Dahkla Oases, lying far out in the Libyan Desert, the occurrence of *A. sergenti* created an economic as well as a health problem, as it had been necessary to prohibit the growing of rice, the chief money crop. DDT was first used here as a larvicide in 1946 and appears to have reduced this species to vanishing-point.

The island-wide eradication campaign in Cyprus, also begun in 1946, was undertaken not because of the occurrence of epidemic malaria but rather as a general measure for improving public health. The two most dangerous anophelines are *A. elutus*, a swamp breeder, and *A. superpictus* which breeds in the pools and seepages of mountain streams. A systematic attempt was made to cover all possible breeding-places in the island, using DDT in kerosene sprays. According to the account by M. Aziz (*Proceedings*, **1**, 703) *elutus* had probably been exterminated by 1948, while *superpictus* was reduced to very small numbers. In Sardinia moderately serious endemic malaria is transmitted mainly by *A. labranchiæ*. An anti-anopheline campaign here is using DDT as a larvicide, and is supplementing this with residual house spraying (J. R. Busvine, 1948, *Nature, Lond.*, **161**, 189). The final outcome of this campaign, in which genus eradication is being attempted, is awaited with interest.

In localities which are not isolated geographically the "residual" method has been the principal means of control. As the residual film of insecticide usually retains its toxicity for many months (five to ten is a normal range), the method is inexpensive and therefore applicable to rural communities; and it has the advantage of being particularly effective against just those house-haunting species that include the worst disseminators of malaria. The obvious disadvantage is that the control measures can never be relaxed.

In British Guiana house spraying with DDT has been the only control method employed. Nevertheless, a 200-mile coastal belt has been virtually cleared of *A. darlingi*, the vector both of malaria and filariasis. By 1947 some 90 per cent. of the inhabitants of the Colony were protected by DDT (C. B. Symes and A. B. Hadaway, 1947, *Bull. ent. Res.*, **37**, 399; G. Giglioli, 1948, *Malaria, Filariasis and Yellow Fever in British Guiana*, Med. Dept. B. G.). Moreover, this method brought a great additional benefit, since it proved equally effective against the yellow fever mosquito *Ædes ægypti*. As jungle yellow fever exists in the hinterland, the presence of

Æ. ægypti in the settled coastal areas had previously necessitated the upkeep of a costly fever service. This organisation had been responsible for inspecting all houses for breeding larvæ at weekly intervals.

Similar successes are reported in the *Proceedings* from many parts of the world. In the United States (G. H. Bradley, **2**, 1598); in Venezuela (A Gabaldon, **1**, 913); in India (M. K. Afridi, **2**, 1588); in Portugal (F. J. C. Cambournac and A. E. da Fonseca, **1**, 696); Greece (G. A. Livadas and G. Belios, **1**, 884) and Italy (A. Missiroli, **2**, 1566). Residual sprays were even effective against *A. fluviatilis*, a species which transmits malaria inside houses in Bombay Province but has its chief resting-places outdoors (D. K. Viswanathan, **1**, 873). At the same time some entomologists emphasised that the indiscriminate use of residual insecticides was no substitute for a proper understanding of the biology of mosquitoes. In illustrating this view N. H. Swellengrebel (**2**, 1563) has given the following examples:

The residual method appears to be ideal against the *maculipennis* group which enter houses and rest on the walls before feeding. A single house with sprayed walls will afford the occupants a good measure of protection. DDT control is also successful in Java against the brackish water breeder *A. sundaicus*. But this species usually feeds first and rests on the walls afterwards. To be effective, therefore, control measures must be carried out on a large scale so that many treated surfaces are available. On the other hand, *A. punctulatus* in New Guinea resists control by this method, as it transmits malaria mainly outdoors. When it does enter houses it feeds on the inhabitants and leaves, seemingly without resting on the walls.

The Polarisation of Light as a Factor in the Orientation of the Honey Bee.—Wide interest has been shown in the discoveries of K. v. Frisch on the significance of the dances in the honey bee. Since writing of this work (1947, Science Progress, **140**, 742), v. Frisch has published further results, no less remarkable in character (*Naturwissenschaften*, 1948, **35**, 12, 38; 1949, *Experientia*, **5**, 142).

It will be recalled that when a foraging bee discovers a rich source of nectar she informs other workers in the hive by dancing on the comb. There are two kinds of dance. The first (the round dance) merely conveys the information that the food is within 100 m. of the hive. The second (the waggle dance), which is performed when the food is further afield, imparts information both as to its distance and direction. The track of the waggle dance,

which other bees follow closely, takes the form of a loose figure 8: where the loops of the 8 cross, the track becomes momentarily straight (the "straight run"). Now the sense of distance is conveyed by the number of complete dances per unit of time. The direction of the food, on the other hand, is shown by the angular deviation of the straight run from a line which bears a definite relation to the sun's direction. Thus, if the comb is laid horizontally in the experimental hive, and sunlight admitted, the straight run points directly towards the food. This is presumably a normal light compass reaction. But under natural conditions, that is, in a dark hive with the comb hanging vertically, the horizontal dance cannot be performed. Gravity then becomes a symbol for the light stimulus: if the path of the straight run on the comb is directly upwards, this means that the food lies towards the sun; if the straight run is inclined at 60° to the left of the vertical, the food is 60° to the left of the sun, and so on. Direction-giving may be accurate to 3° under optimal conditions.

The evolutionary significance of the horizontal dance is obscure, for, although the dance is sometimes performed on the horizontal alighting board of the hive, this is of course an entirely artificial arrangement; and direction-giving inside a darkened hive could hardly be expected to achieve much accuracy, as it seems that the position of the sun is not remembered by a bee that has made many turns in entering the hive.

Whatever its true significance, however, the horizontal dance has proved extremely useful for experimental purposes. While observing it, v. Frisch noticed that the straight run gave the correct direction of the food, not only when the sun itself was visible through the glass roof of the observation hive, but also when blue sky only was visible to the dancing bees. The following experiments proved that light was the significant stimulus:

(i) In the dark (observation in red light to which the bees are insensitive) the dance was disoriented and the bees flew out in all directions. The straight run became oriented only when the comb was placed vertically. (ii) When a lamp was introduced into the hive this light source was taken as the "sun" and the straight run became oriented accordingly. (iii) A circular area of blue sky subtending an angle of about 10° was found to be adequate for orientation. If light from the opposite part of the sky was reflected with a mirror down the tube admitting the daylight, the direction of the dance was reversed. (iv) This capacity of the light from blue sky to act as an orienting stimulus also explained the results of a large series of previous experiments in which systematic

errors in the inclination of the straight run on a vertical comb had been recorded. The discrepancy was least at noon but was sometimes as large as 50° in early morning or late evening. It now appeared that the "errors" could be traced to the section of blue sky admitted to the hive during the observation. The bees were compromising between orientation to gravity and direct orientation to visible light.

These experiments suggested to v. Frisch that the polarisation of the light from blue sky might serve as the guide in orientation. It is known that both the proportion of the light polarised and the plane of polarisation bear a regular relation to the sun. The content of polarised light is least nearest the sun, increases to a maximum of 60–70 per cent. at 90°, then decreases again to a point opposite to, and corresponding with, the sun. The plane of polarisation lies at right-angles to a plane joining the sun, the point observed and the observer's eye.

This hypothesis was tested with striking results by placing a polarising screen over the hive. On rotating the screen the direction of the horizontal straight run alters in accordance with the angle of rotation, but when it exceeds 50–60° the dance becomes disoriented. Although some unexplained anomalies occur, connected possibly with the weak polarisation of light near the sun, the phenomenon must be regarded as proven. Obviously this discovery poses further important physiological problems. Not the least among these is the assumption that the compound eye of the bee is particularly suited to optical analysis of this kind.

Sensitivity to polarised light may also be of significance in other insects. As long ago as 1923 F. Santschi observed that homing ants could preserve a correct orientation so long as a small area of blue sky was visible to them. Santschi suggested that the ants were taking their direction from the positions of the stars. The true explanation, v. Frisch remarks, may be hardly less romantic.

The Arthropod Fauna of Soil.—The majority of the arthropod inhabitants of soil are so minute that successive improvements in the technique of extracting them have led to increasingly high estimates of the total population. The method of G. Salt and F. S. J. Hollick (1944, *Ann. appl. Biol.*, **31**, 52) was devised primarily for the rapid estimation of wireworm populations in pasture soil; unexpectedly large populations, ranging up to 10 millions per acre, were recorded.

G. Salt, F. S. J. Hollick, F. Raw and M. V. Brian (1948, *J. anim. Ecol.*, **17**, 139) have since turned their attention to the total

arthropod fauna of pasture soil. The arthropods collected from a Cambridgeshire meadow were representative of a population of 264,000 per sq. m. or 1069 millions per acre; but, allowing for the known loss of very small Acarina, the real population is thought to be at least 1400 millions per acre. This is equivalent to about one animal per cubic centimetre of soil. Taking the "average volume" of the animals collected, they estimate that the arthropod fauna occupies about one part in 20,000 of the gross space in which it lives. This is probably much higher than the corresponding figure for micro-organisms in the sea. Mites and Collembola formed by far the most numerous groups in the collections, the first alone accounting for 62 per cent. of the total population.

The ecological structure of some arthropod communities of soil has recently been examined by T. Weis-Fogh in Denmark (1947–48, *Natura Jutlandica*, **1**, 270 pp.). In the area chosen—raised beach with a predominantly sandy soil—slight differences in height greatly influenced water content, soil texture and type of vegetation. In the drier situations the soil was open and porous, whereas in the lower moister parts the soil pores were smaller and largely infiltrated with mud. Acarina and Collembola were again the dominant members of the "micro-fauna" living within this system of pore spaces.

Two distinct communities are described. The fauna inhabiting the larger pore spaces of the "dry" locality was characterised by small forms, particularly soft-skinned Collembola and root-sucking Trombidiform mites. The moist soil community, on the other hand, mainly comprised larger forms, especially Oribatid mites, living at or near the soil surface. It is suggested that members of the first community, which are usually very sensitive to dry conditions, escaped desiccation during dry weather by moving downwards into the deeper soil pores; and here they were also protected against the larger predators (mainly Staphylinid beetles and Gamasid mites). On the other hand, the Oribatei were prevented by their size from entering the soil pores; hence, although comparatively resistant to desiccation, they were able to survive only in the moist locality. Here they were fully exposed to the attacks of predators, but were in a large measure protected by their tough cuticles.

NOTES

Wapiti Deer in New Zealand

The joint United States–New Zealand expedition to investigate the adaptation of wapiti deer to New Zealand conditions in the fiordland area of the south-west region of the South Island of New Zealand has now completed its work. An interim report, based on information given by Mr. A. L. Poole of the Botany Division of the Department of Scientific and Industrial Research, and deputy leader of the expedition, has been issued, pending the publication of the main conclusions of Dr. O. Murie, leader of the expedition.

Dr. Murie, according to Mr. Poole, said conditions were infinitely worse than he had expected. None of the wapiti ranges in the United States offered such hard conditions, and the fiordlands were the worst area he had encountered outside the Arctic.

The expedition saw many wapiti, both in the forest of the valley floors and on the mountain-tops at 3000 to 4000 feet. The feeding habits of the animals could be followed by their tracks and the disturbed vegetation, and it was possible to study the animals and their feeding habits well in both places. There were few red deer in the heart of the wapiti country, and they were hard to find. It seemed possible that they occurred only on the outskirts of the wapiti country, where they had bred with the wapiti to some extent.

The wapiti herd itself was sparse. Over a total area of 150 square miles (the main breeding area) there were probably not more than a few thousand animals, which, Dr. Murie observed, were mainly young ones.

Mr. Poole considered the wapiti probably had slow breeding habits in this difficult country, and suffered losses because of rough climatic conditions and falling over bluffs. The mountain-tops were precipitous.

Dr. Murie secured about thirty wapiti, and stomach contents from all the animals shot were examined.

The effect of the herd on vegetation was marked, though not

serious. Certain shrubs on the valley floors were affected to a marked degree, and, as most of these plants were epiphytes, the animals managed to survive. The wapiti appeared to concentrate on a particular area and then move on. Some areas were well grazed, and other large tracts were untouched.

The forest consisted mainly of southern beech, and the prevailing wet conditions did not make it very enticing for animals. The wapiti were more or less confined to their present area by encircling herds of red deer, which followed the same policy of eating out an area and moving on.

There was probably a sparse population of stoats in the area, Mr. Poole said. Two or three were trapped and as many seen. One opossum was caught at Caswell Sound, and bird life—wekas, pigeons, bellbirds and others, but no notornis—was plentiful. He doubted whether the wapiti affected bird life. At its present level the herd could do little harm, but if it grew it could do a great deal of harm.

The expedition's search for a breeding-place for sandflies was unsuccessful. The sandfly menace, Mr. Poole said, seemed to be rather over-rated.

Collapsible boats, three of which were brought from America for the expedition, proved useful, because of the number of sheer-sided lakes.

American Geological Institute

On June 1, Dr. David M. Delo of Washington, D.C., began his duties as the first Executive Director of the American Geological Institute. This is a new organisation, established in November 1948, which will represent the profession of geology. It is composed of a union of eleven geological societies whose members total more than 10,000 professional geologists. Headquarters will be located in the National Research Council, 2101 Constitution Ave., Washington, 25, D.C., and the activities of the institute will be carried on in conjunction with those of the Division of Geology and Geography, N.R.C.

The primary objectives of the new institute are the advancement of geology and its application to human welfare by providing a means for the co-operation of organisations active in the fields of pure and applied geology. Membership is open to all non-profit organisations concerned with the earth sciences—geology, geophysics, geochemistry, mineralogy, etc.

The institute is organised as an instrument of the National Research Council, in this way uniting geologists with all other

American scientists who are seeking solutions to problems which can be attacked best through group or united action. Initially its functions will be concerned primarily with the non-research activities of the geological profession, and will supplement the work of the Division of Geology and Geography of the National Research Council, which is chiefly concerned with the co-ordination of research in geology and geography and its inter-relationships with allied sciences.

Officers of the institute are A. I. Levorsen, Dean of Mineral Sciences, Stanford University, president; W. B. Heroy, Beers and Heroy, Dallas, Texas, vice-president; and Earl Ingerson, U.S. Geological Survey, Washington, D.C., secretary-treasurer.

"Biologia Generalis" (W. H. P.)

A new journal has been published by Springer of Vienna called *Biologia Generalis*, edited by Ludwig von Bertalanffy. The first number contains four original articles, of which the first by E. Tschermak-Seysenegg deals with botanical researches on parthenogenesis. The second article by B. Klatt deals with the problem of skull form as an exercise in mathematical biology. Karl Höfler deals with the methods of fluorescence microscopy in relation to cell physiology, and the editor, L. v. Bertalanffy, contributes an article on the organisation of knowledge. The number concludes with an interesting general review by H. Schanderl on our expanding knowledge of yeasts, dealing particularly with their cytology and heredity.

Miscellanea

The honours list published on the occasion of H.M. the King's birthday included the following: *O.M.*: Sir Robert Robinson, president of the Royal Society; Earl Russell. *C.H.*: Mr. Lionel G. Curtis, president of the Royal Institute of International Affairs. *G.B.E.*: Sir Walter Moberly, chairman of the University Grants Committee. *D.B.E.*: Dr. Harriette Chick, for services to the study of nutrition. *Knights*: Prof. David Brunt, professor of meteorology, Imperial College of Science and Technology, London; Dr. R. G. Hatton, lately director of the East Malling Research Station, and consultant director of the Commonwealth Bureau of Horticulture and Plantation Crops; Mr. H. S. Souttar, consulting surgeon, London Hospital; Prof. Godfrey H. Thomson, Bell professor of education, University of Edinburgh; Prof. J. A. Scott Watson, chief scientific and agricultural adviser, Ministry of Agriculture. *C.B.*: Mr. H. Campion, director of the Central Statistical Office, Cabinet Office; Mr. W. G. A. Perring, director of the Royal Air-

craft Establishment, Farnborough. *C.M.G.:* Dr. F. Dixey, geological adviser to the Secretary of State for the Colonies, and director of Colonial Geological Surveys; Mr. H. Faulkner, deputy engineer-in-chief, General Post Office; Dr. C. H. Hampshire, secretary of the British Pharmacopœia Commission; Dr. D. E. Hansen, of Christchurch, New Zealand, for outstanding services in the field of technical education. *C.B.E.:* G. H. Cunningham, for services in plant research and plant diseases; Dr. A. H. Davis, senior superintendent, Armament Research Department, Ministry of Supply; Mr. G. H. A. Field, director of research, Aluminium Laboratories, Ltd.; Mr. C. F. Franklin, consultant, Marconi's Wireless Telegraph Co., Ltd.; Prof. John Garstang, chairman of the British Institute of Archæology in Ankara; Dr. F. H. K. Green, assistant secretary, Medical Research Council; Dr. N. A. Mackintosh, director of research, Discovery Committee; Dr. J. R. Nicholls, deputy Government Chemist; Prof. R. M. F. Picken, provost and Mansel Talbot professor of preventive medicine, Welsh National School of Medicine; Prof. A. D. Ross, professor of mathematics and physics, University of Western Australia; Mr. F. G. Simpson, archæologist; Dr. G. F. Herbert Smith, for services to the preservation of the flora and fauna of the British Isles; Dr. T. E. A. Stowell, for services to industrial medicine; Mr. R. G. White, director of the Animal Breeding and Genetics Research Organisation, Agricultural Research Council.

The following have been elected foreign members of the Royal Society: Prof. P. W. Bridgman, research professor of physics at Harvard University; Prof. N. L. Bowen, petrologist at the Geophysical Laboratory, Carnegie Institution, Washington; Prof. Max von Laue, of the Max Planck Institute, Göttingen; Prof. Erwin Schrödinger, senior professor and director of the Department of Theoretical Physics, Dublin Institute of Advanced Studies, Dublin.

Prof. E. C. Bullard, F.R.S., professor of physics in the University of Toronto, is to succeed Sir Charles Darwin, K.B.E., F.R.S., as director of the National Physical Laboratory; it is expected that Prof. Bullard will take up the appointment in January 1950.

Mr. W. K. Wallace, chief civil engineer to British Railways (London Midland Region), has been appointed chairman of the Building Research Board in succession to Sir George Burt.

Mr. W. C. Moore has been appointed director of the Plant Pathology Laboratory of the Ministry of Agriculture and Fisheries at Harpenden on the retirement of Mr. C. T. Gimingham.

Dr. F. Gross, lecturer in experimental zoology in the University of Edinburgh, has been appointed director of the new Marine Biological Station to be erected on the Menai Strait at Bangor.

Dr. William Davies, director of the Grassland Improvement Station of the Ministry of Agriculture and Fisheries at Drayton, Stratford-on-Avon, has been appointed director of the new Grassland Research Station for Great Britain at Hurley in Berkshire, which is being amalgamated with the Drayton Station.

We have noted with great regret the announcements of the death of the following scientific workers during the quarter: Dr. N. R. Campbell, formerly of the Research Laboratories of the General Electric Co., Ltd., Wembley; Dr. P. H. Cowell, F.R.S., formerly superintendent of the Nautical Almanac Office; Sir Wyndham Dunstan, K.C.M.G., F.R.S., formerly director of the Imperial Institute; Prof. E. J. Garwood, F.R.S., emeritus professor of geology and mineralogy in the University of London; Prof. W. W. Hansen, professor of physics at Leland Stanford University, California; Prof. F. S. Kipping, emeritus professor of chemistry in University College, Nottingham; Mr. Philip Lake, formerly reader in geography in the University of Cambridge; Dr. F. S. Locke, formerly reader in physiology at King's College, University of London; Count Maurice Maeterlinck, philosopher and naturalist; Dr. W. J. Perry, lately reader in anthropology in the University of London; Sir Robert Robertson, K.B.E., F.R.S., formerly Government Chemist.

We have received from the Bee Research Association a brochure setting out the organisation, objects and terms of membership of the Association. Anyone who is interested can obtain a copy of this brochure from the Director, Dr. Eva Crane, 55 Newland Park, Hull.

The *Australian Journal of Scientific Research* for December 1948 contains a paper by V. D. Hopper on an investigation he has recently made into the oil drop method of determining the electronic charge e. Whilst the discrepancy between the value obtained originally by Millikan, and that given by the X-ray method, can readily be explained by the uncertainty in the value assumed for the viscosity, η, of the air, this is not the whole story. Even when any uncertainty in η is eliminated, the results obtained by different workers using the oil drop method still show large variations despite the self-consistency of each set of results. This fact prompted

Hopper to make a further study of the method in an attempt to find the reason for these differences.

As Hopper proceeded, the factors which he found to be significant were the purity of the air, small disturbances to the electric field due to the small hole in the top plate and the layer of oil which collects on the bottom plate, correction to Stokes' equation to allow for the influence of the walls of the apparatus, and oxidation of and impurities in the oil.

The apparatus Hopper used was essentially the same as the one he and Grant had used previously in their study of the effect of walls on a falling drop (*Aust. J. Sci. Res.*, *A*, **1**, 28, 1948). The motion of the drop was recorded photographically, the drop being illuminated every ½ of a second by a spark, the triggering voltage for which was supplied by a multivibrator unit controlled by a quartz crystal oscillator. The motion of drops of apiezon oil B and castor oil was found to vary with time, probably due to oxidation and absorption, and instead butyl sebacate was used, the drops being formed by moving a rod against the wire bristles of a brush on which some of the oil had been placed. The temperature of the room in which the experiment was performed was automatically controlled so that it drifted by less than 0·01° C. per hour, the temperature of the apparatus itself being measured with a fine wire thermocouple. As the viscosity of the air in the apparatus appeared to change slowly with time (0·3 per cent. after a few weeks), it was essential to evacuate the apparatus and fill it with dry air free from carbon dioxide just before taking each set of measurements. The effect of the hole in the top plate was found to be negligible provided it was made extremely small and measurements were not made on drops falling immediately below it.

The paper includes the data obtained from measurements on 47 drops of butyl sebacate. To avoid introducing the absolute value of the viscosity of air, the results have been used to find the ratio $e^{2/3}/\eta$. The value obtained for this ratio is higher than Millikan's corresponding result but lower than that of other workers. When a more accurate value of η is available the value of e obtained by this investigation can be given.

R. E. J.

Although in the past the London Underground Railways have used D.C. motors for their escalators, on a recent installation it was decided to relieve the D.C. power supply by using an A.C. drive instead. The general requirements that an escalator motor has to fulfil and the way in which they were met in this particular

instance are discussed in an article in *B.T.H. Activities* for May–June 1949.

As the wear on the moving parts of an escalator increases as the third power of the speed, it is essential that facilities for idling during slack periods should be incorporated. Thus an empty upgoing escalator is usually run at half its normal speed. As a passenger steps on he interrupts a beam of light which is directed on to a photo-electric relay, and the escalator is accelerated until its speed has reached the normal running speed of 120 ft. per minute, which it does after only two seconds. The Schrage type of A.C. motor which was installed is accelerated by changing the relative position of two sets of brushes bearing on the commutator. As the brushes move, they pass through an infinite number of speed gradations, the motor speed lagging slightly behind. The resulting acceleration is slow to begin with, rises to a maximum and then decreases to zero. This type of acceleration is the most comfortable from the passengers' point of view, whose speed is doubled almost imperceptibly. The escalator continues to run at normal speed for two minutes, when it automatically slows to half-speed unless the beam of light has again been broken.

It is also important that the motor should be able to maintain its speed as the load increases so that the escalator's carrying capacity does not fall at the time when it is most needed. This applies to upgoing escalators. Loaded downgoing escalators, on the other hand, can be used to generate power, provided the motor can be used as a generator and that the speed increase required for regeneration is small.

The article contains many other details, including a description of the control circuits, the facilities for reversing and emergency stopping, as well as a general account of how an escalator works.

R. E. J.

A press release from the Port of London Authority states that, at their request, the Water Pollution Research Laboratory, D.S.I.R., has recently begun a detailed survey of the Thames from Teddington to the sea. The object of the survey is to study the causes and to investigate the sources of silt in the river, as well as to observe the velocities, direction and duration of the tidal currents on the ebb and flood at varying depths during the neap and spring tides. Information is also required regarding pollution of the river by sewage and trade waste, a matter of some concern for many years.

It is expected that the present survey will take at least two to three years to complete. The staff, which includes chemists and

a hydrographer, will be based at Tilbury, where they have been provided with a laboratory and a 75-foot motor launch by the Port of London Authority.

At the same time the Hydraulics Research Organization, D.S.I.R., are constructing a scale model of the river to determine the measures needed to prevent the excessive siltation which at present occurs in certain parts of the river. If this project is successful, it will be possible to reduce considerably the heavy dredging programme which is at present necessary to maintain the requisite depth of the navigational channels.

R. E. J.

The Sonoco Products Company, Hartsville, South Carolina, have recently announced the successful development of a new range of oil-base paints containing D.D.T. which they are now putting on the market. The paint is lethal to flies, mosquitoes, roaches, etc., and should be of great use in hospitals, restaurants, etc., as well as in the home.

The incorporation of D.D.T. into paints, a procedure considered impracticable by some workers, was based upon the behaviour of sulphur in rubber. In this latter case, sulphur present in excess of the amount soluble in the rubber at a given temperature migrates to the surface, where it forms a crystalline layer. The D.D.T. in the oil paint was found to behave in a similar way, forming a layer of insecticide on the surface of the paint, this process being known as "frosting." Although the D.D.T., being part of the film, is anchored to the surface, if at any time it should be removed the crystallisation reappears.

Tests made showed that the number of flies killed in a cage, one side of which was painted with the D.D.T. paint, was usually 100 per cent. after a 24-hour exposure. Tests were also made in homes, where the paints were found to be equally effective. In all cases the paint was found to be still highly toxic after two or three years despite exposure in some cases to dirt and dust.

This method of application of D.D.T. has obvious advantages over the use of sprays and dusts. The quality of the paint compares favourably with other standard paints on the market except in one respect. The "frosting," while difficult to observe with the white flat paint, does show up to a certain extent with the white gloss paint. However, in industry and for use in buildings where sanitary conditions are of prime importance, this disadvantage is far outweighed by the beneficial properties of the paints.

R. E. J.

The Story of the Typewriter, a fascinating little book by the late Lieutenant-Commander R. T. Gould, R.N., has recently been published by Crumwell Services Ltd., 64–65, Chandos House, Palmer Street, S.W.1 (price 2*s*. 6*d*.). Commander Gould, who was well known on the wireless both as " Stargazer " and as a member of the Brains Trust, had for many years been piecing together the history of the typewriter and had collected together many of the early machines at his home in Surrey. He addressed the Royal Society of Arts on the subject in 1928, and broadcast on the same subject just before the war. Many of his early typewriters have been bought by the South Kensington Museum to add to the National Collection.

The first typewriter, of which there is any record at all, was invented by the Englishman Henry Mill, who took out a patent for it in 1714. Whether it was actually constructed is not known, but it is safe to say that, with the tools then available, a machine built to his design could not have held its own against ordinary handwriting. The three points in favour of the modern typewriter are its legibility, its ability to make numerous copies and its speed. This last point is of prime importance, and it was a common defect of many of the early machines that their speed of writing was extremely slow.

The design of typewriters can generally be divided under two headings, the " Block " and the " Bar " forms of machine. In the " Block " class, the types are all mounted together as a single block, and the whole block has to be moved to bring any letter to the printing point. In the " Bar " class the types are mounted separately on moveable bars so that any one can be used without disturbing the remainder. Whilst typewriters belonging to the former class are usually cheaper to manufacture, those of the " Bar " class, having less inertia, are considerably faster in operation, and all modern typewriters belong to this class.

It is only possible to mention a few of the many typewriters which, now but milestones in the general development, are included in this book. One of the earlier machines, Thurber's "Chirographer," was unique in that, instead of printing, it actually wrote the words with a pencil on a vertical sheet of paper. On pressing a key, two cams came into operation, one to move the pencil vertically and the other horizontally, after which the paper was automatically moved to the left a distance corresponding to the width of the character which had just been written. This latter feature of the machine, known as differential spacing, is of particular interest as it has been incorporated in only a very few machines, none of

which has survived. In the modern typewriter the escapement allows a standard tenth of an inch irrespective of whether a full stop or a capital W is being typed. This is the chief reason for the difference in appearance of work turned out by a typewriter and ordinary print.

The first typewriter to have any commercial success at all was designed principally by C. L. Sholes in the United States. Sholes, justly acclaimed the "Father of the Typewriter," persevered for many years before perfecting the machine which, mass produced by Remingtons in 1873, was the first to be offered for sale to the public in any number.

The originality of Sholes' work is debatable, as practically every feature of his machine had been incorporated in one or other of the earlier machines. But, whether he knew of them or not, he had the ability to incorporate their better features into his own design. The keyboard as used today was designed by Sholes, the arrangement being chosen merely to separate the type bars, which are often used consecutively, so that the risk of their jamming was reduced. Although this keyboard has many defects, it was one of the first in the field, and as such has resisted all attempts to change it.

Nowadays the design of the typewriter has been almost standardised, no fundamental improvements appear likely, the machines are fast, robust and easy to operate, but this is not the only result of those seeds which were sown so long ago by the early pioneers. The typewriter has brought about an enormous social change—"the emancipation of millions of women from household drudgery to the position of wage earners."

R. E. J.

ESSAY REVIEWS

MODERN ARACHNOLOGY. By THEODORE H. SAVORY, M.A., F.Z.S. Being a Review of (1) **The Spider Book,** by J. H. COMSTOCK. Second edition revised by W. J. GERTSCH, Ph.D. [Pp. xii + 729, with frontispiece and 771 figures, including 1 coloured plate.] (Ithaca, N.Y.: Comstock Publishing Co., Inc.; London: Constable & Co., Ltd., 1948. 35*s*. net.) (2) **Bibliographia Araneorum,** by P. BONNET. [Pp. xvii + 832, with 28 plates.] (Toulouse: Frères Douladoure, 1945. Frs. 3800.) (3) **A Study of Palæozoic Arachnida,** by A. PETRUNKEVITCH. (*Trans. Conn. Acad. Arts Sci.*, XXXVII, 1949, pp. 69–315.) (4) **Traité de zoologie,** publié sous la direction de Prof. P. P. Grassi. Tome VI: Arthropodes, Chelicorates, etc. [Pp. 1–960.] (Paris: Masson et Cie, 1949. Frs. 5000.)

ARACHNOLOGY is the study of spiders, scorpions, mites and their allies, a class of eight-legged invertebrates often looked upon with less enthusiasm and more distaste than they deserve.

In the preface to a book written some fifteen years ago, I expressed a desire to encourage in Arachnology the acquisition " of the unity and status of an individual science that is possessed by Entomology." This modest ambition has been noted and approved by other zoologists; and it has been obvious to anyone who has been looking for it that of recent years a widening interest and steady consolidation of ideas have, in fact, occurred, and are leading to just that co-ordination of knowledge which will make Arachnology take shape as a matured and autonomous science. The four new publications which have evoked these sentiments justify also the optimism of this opinion.

(1) The well-known *Spider Book* by J. H. Comstock first appeared in 1913. Despite the fact that it was concerned chiefly with North American spiders, and to a smaller extent with Harvestmen and False-scorpions, it received a warm welcome in this country where, at that time, trustworthy information about spiders was not easily obtainable. Comstock, who was Professor of Entomology at Cornell, died in 1931, and his book had been out of print for some years when, just before America entered the war, a second edition was prepared by Dr. W. J. Gertsch, of the American Museum of Natural History in New York. This edition was re-issued last year.

One cannot but feel some disappointment at Dr. Gertsch's statement that "the keynote of the revision has been conservatism." Deservedly admired as the *Spider Book* might have been, it could not have expected immunity from the effects of time, and a very thorough overhauling would surely have been justified. What Dr. Gertsch has, in fact, done has been little more than an incorporation of the work of Prof. C. F. Roewer of Bremen and Dr. J. C. Chamberlin of Stanford University in the sections dealing with Harvestmen and False-scorpions, while nearly all that part of the book which describes spiders preserves the classification, the beliefs and the state of our knowledge existing thirty-six years ago.

This does not mean that the *Spider Book* is unwelcome. It remains a valuable introduction to the study of Arachnida; it tells us a great deal about the structure of their bodies and about the differences on which their classification is founded, and it still possesses, of course, the wealth of illustration, by photograph and drawing, which was accorded so enthusiastic a reception in 1913. At the same time, Dr. Gertsch's forthcoming book, *American Spiders*, which will expound more exactly the Arachnology of 1950, is eagerly awaited. The opportunities before such a book are considerable.

Above all, there is a chance to give the orders other than spiders a larger share of attention. Arachnology has always been, still is, and perhaps always will be dominated by the study of the spider. This is probably inevitable, because there are many more spiders than all other Arachnida together; they are more widely distributed; and they are much more conspicuous. But in a balanced survey of Arachnology, as distinct from a popular one, it ought not to be forgotten that the class contains twelve living and four extinct orders, and that each of these can make its own contribution to Zoology. Moreover, it is among the lesser orders that there are to be expected the major advances of which the science of Arachnology stands so greatly in need.

(2) The supremacy of spiders is emphasised by the second of the works considered here. This is the *Bibliographia Araneorum*, in which Prof. P. Bonnet of Toulouse has given us a complete bibliography of spiders from the time of Aristotle until 1939.

Twenty-five years ago, Dr. Bonnet set himself the task of examining, analysing and indexing every publication, of any age and in every language, which dealt with spiders; and in 1946 his Tome I appeared, justifying this incredible enterprise. It is a substantial volume, and the greater part of it consists of nearly 9000 references, which might seem, at first sight, to be unlikely to produce a readable book. But Prof. Bonnet has given us more than this, for he tells

us of the origin, growth and execution of his plan, of his difficulties with unfamiliar languages, unresponsive curators and unorganised libraries, and of the dangers which his manuscript survived when Toulouse was occupied by Germans and bombed by the R.A.F.

He has given us, too, a *Histoire d'Aranéologie*, from Ovid's story of the fate of Arachne to present times, followed by short biographies of 124 araneologists, with portraits of most of them. This *livre d'or de l'aranéologie* is both fascinating and valuable. Not only does it supply a human interest, it also helps to create a personal touch in a science mainly carried on by scattered workers, who can meet one another but seldom and are usually limited to such contacts as letters can provide.

The book, clearly, is a triumph of perseverance, strengthened by a faith in the value of the task which raises an unusual enthusiasm to an almost fantastic devotion. How does the achievement compare with the conception? The answer is this: should we ask any question about spiders, structural, physiological, evolutionary, experimental or fabulous, Prof. Bonnet's indexes will guide us to everything ever written on the subject: and if, alternatively, we take a country, a county or an island we are similarly guided to every list of its spider-fauna.

In achieving completeness, or something as near to it as makes no matter, Prof. Bonnet does not attempt to distinguish the authoritative from the trivial, the worthwhile from the worthless; but his amazing book—and it deserves the adjective—puts all araneologists deeply in his debt. Never again can a research worker plead ignorance of the conclusions of his predecessors, nor will he find that his investigations were but a repetition of the results of others. There cannot be many biologists who can say, with regard to their special study, that they are in so enviable a position: Prof. Bonnet's service is hard to overestimate.

When one pauses to consider the quantity of knowledge which must be contained in over 8000 printed works, one is able to understand that the study of spiders has reached a satisfactory state of maturity. No doubt one may still speak with justification of "the petty done, the undone vast," but at least the foundations of araneology are firmly laid and the scaffolding above will help the constructions of the future. Therefore it is natural that some zoologists have lately turned to examination of the other orders, where the opportunities of the pioneer are greatest. Some of the results of this spreading interest are to be read in two works which well deserve commendation.

(3) Eugene Simon, who for many a year had reigned in Paris

as the unquestioned omniscient to whom all arachnologists turned for help and advice, died in 1924. In the previous year there appeared in the *Annals of the New York Academy of Sciences* a paper of some 35 pages with the modest title "On Families of Spiders." Its author was Alexander Petrunkevitch, who in 1913 had produced a welcome monograph on the palæozoic Arachnida of North America, and who had then been for six years Professor of Zoology at Yale University.

This paper was recognised by all as an outstanding contribution to a very difficult taxonomic problem, but it was only the first of a series of attempts which Prof. Petrunkevitch has since made to bring light and order into the chaos of spider-classification. His *Systema Aranearum*, which listed all known genera with their type-species, appeared in 1928, to be followed in 1933 by *An Inquiry into the Natural Classification of Spiders*. The significant word in this title was "natural," for the system proposed was based on a study of internal structure, more completely and extensively compared than has ever before, perhaps, been true of a classification of any comparable group of Arthropoda. Some supplementary data were added in 1939 in his *Introduction to a Catalogue of American Spiders*, and these works paved the way to a most impressive monograph in 1942 on spiders preserved in Baltic amber. He expounded the clues which led towards an understanding of the evolutionary history of spiders, and by relating their taxonomy and phylogeny made an advance towards an ideal system of classification.

When Petrunkevitch retired from his Chair in 1944, his reputation was deservedly secure as the most eminent arachnologist in the world; but retirement seemed only to open a door to further opportunities for research, and this year he produced *A Study of Palæozoic Arachnida*, a work which in many ways overshadows even its great predecessors.

One of the chief puzzles of Arachnology has for long been that of the phylogenetic relations between the different orders, which cannot be arranged in a simple evolutionary sequence from the most primitive to the most specialised. All the orders were in existence in the early Palæozoic, and their separation from primitive arachnoid stock must have occurred in pre-Cambrian times, of which we have very little knowledge.

A feature of the paper, outstanding among much else, is the close description of bodily segmentation in each order, so that the evolutionary trends in prosoma and abdomen become apparent. These tendencies to somatic change must have begun among the ancestral Arachnida, and the result of having them now made plain is that

the classification of the group is facilitated and, above all, its relation to the Eurypterida, Xiphosura and Pycnogonida can be understood.

Many of us who were denied communication with French zoologists between 1940 and 1945 must remember the pleasure with which, soon after normal correspondence was restored, we began to receive again the reprints, or *tirés-à-part*, from our colleagues across the Channel. One learnt with delight that Arachnology had thrived during the years of enemy occupation ; nor is it in any way a reflection on the work of others to record the satisfaction with which one found in the publications of Prof. Jacques Millot a growing concern with the orders Pedipalpi, Ricinulei and Palpigradi.

Prof. Millot, who has spent nearly the whole of his working life in Paris, was appointed Professor of Zoology at the Sorbonne in 1934. In 1926 he had published an account of the cytology and micro-physiology of spiders, in which the most welcome feature was that he had overcome the difficulties due to the hardness of the chitinous exoskeleton and was able to obtain satisfactory sections of spiders of all kinds. Much of his work during the next twenty years was based on this successful technique, and he has given us in two decades reliable comparative descriptions of the digestive, vascular, venom- and silk-producing organs. In the papers to which we have referred above he has extended these methods to other orders.

(4) The work of Millot and his colleagues in France must undoubtedly be counted among the most important contributions to general Arachnology during the present century, and thus it is fitting that the fourth book under review comes from France and is largely indebted to their co-operation. Early this year there appeared from the house of Masson et Cie of Paris the announcement of a new and ambitious venture, a *Traité de Zoologie* to be completed in seventeen volumes, each of over 1000 pages. Tome VI, which has just been published, includes the Arachnida and their nearest relatives.

Until now the only comparable work of reference has been the third volume of Kukenthal and Krumbach's *Handbuch der Zoologie*, now approaching the twentieth year of its age, so that this much newer compilation is particularly acceptable. As a textbook is has, in fact, no equal ; it is authoritative, it is copiously and beautifully illustrated, and it is most fascinating to read. The section on Arachnida reflects the greatest credit on all its learned contributors.

It will not have escaped notice that the advances in Arachnology which have inspired this essay have come from America and France.

Recent history has not encouraged the pursuit of pure Zoology in Germany, where ten years ago it was in a state of vigorous growth, but why, may one ask, have there been no comparable publications in Britain ?

The answer is, as it has always been, that our professional zoologists are not interested in the Arachnida ! There has never been, in any British university, a holder of a Chair, or even a reader or a lecturer, who paid the smallest attention to spiders or scorpions. Mites have not been quite so completely neglected : Mr. Cecil Warburton, the author many years ago of the arachnid chapters in the *Cambridge Natural History*, devoted most of his time to these little pests.

Outside the universities our chief biological institution is the British Museum (Natural History), but in South Kensington the word Arachnida is interpreted to mean mites. For the past forty years the officials in charge of the Arachnida have been mite-specialists, who have scarcely concerned themselves with the other orders.

Arachnology in Britain is kept alive solely by the enthusiasm of " amateurs "—clergymen, doctors, chemists and schoolmasters—and of these there have never been enough. When, on 11 June, 1944, four of them met in Harrow, the group was thought worthy of a photograph, because it was believed that never before had so many British arachnologists been together in one room ! It requires no great judgment to perceive that as long as our science is in the hands of those who are obliged to earn their living in other ways, so long will the chief advances continue to be made in other lands.

GENEALOGICAL GUESSES. By S. Zuckerman, C.B., M.A., M.D., D.Sc., F.R.S., Professor of Anatomy in the University of Birmingham. Being a Review of **Human Ancestry from a Genetical Point of View**, by R. Ruggles Gates. [Pp. xvi + 422, with 27 plates and 9 figures.] (Cambridge, Mass. : Harvard University Press ; London : Oxford University Press, 1948. 42*s*. net.)

For no fault of its own, physical anthropology, the science of human taxonomy and phylogeny, seems to attract far more attention of a kind which can only be described as non-professional than probably any other branch of science. This would not matter if it were otherwise a thriving subject—but only this kind of attention seems to make it known at all. The notice which it receives to-day derives mainly from the statements of a school of anatomists who do not seem to realise that the newer methods of quantitative biology are

essential if taxonomic problems are to be treated effectively. Now we have an ambitious attempt by a geneticist to apply the conclusions of his own subject to the problem of human origins. Let it be said at the start—it is an unfortunate attempt. The difficulty here is that Dr. Gates has not been cautious enough in judging the adequacy of the anatomical descriptions which he has used in his study. This, coupled with a disregard for biometric and statistical discipline, is the main shortcoming of his *Human Ancestry*.

Dr. Gates is an Emeritus Professor of Botany—he retired from the University of London in 1942. In 1946 he published a two-volume work on human genetics, in which he attempted to review the vast literature on the subject. During the past few years he has also paid increasing attention to the writings of physical anthropologists, engaging in lively controversies with the late Dr. Franz Weidenreich, a distinguished anatomist and during his lifetime our main authority on the human fossil remains of China and Java. Dr. Gates' own contributions to physical anthropology are some papers on blood groups.

In his new book, he tries to pull together our knowledge of the origin, evolution and present-day taxonomic status of the races of man. The volume opens with a chapter on the principle of parallel evolution. Many illustrative examples of the usual morphological kind are given ; these, however, do not constitute definitive evidence for the "principle," and represent little more than a series of interesting speculations from palæontological studies. Brief references are made to parallel mutations, but none to those modern studies in genetics, biometrics and selection, which show how similar phenotypic forms may be attained in different phylogenetic lines. This shortcoming affects the whole book, because the assumption of parallel evolution, in the sense the concept is used by Dr. Gates, underlies his final conclusion that the modern races of man, generally classified as *Homo sapiens*, actually represent five distinct and independent species.

In his third chapter, Dr. Gates discusses the evolution of mammals, including the Primates, and lends support to the view that they are polyphyletic in origin. Considerable attention is paid to the South African fossil apes, and the usual descriptions of these forms are uncritically accepted in their entirety. The book unfortunately went to press before the appearance of more critical studies, which suggest that the accounts of the South African fossils which are now current may need to be considerably modified. The fourth chapter deals with the evolution of Man, and Dr. Gates then goes on to consider head shapes and their inheritance in an extra-

ordinarily speculative chapter. He tries to distinguish between modern races on the basis of head form, without apparently realising the considerable dangers involved in endowing such isolated characters with a particular taxonomic significance—especially when little is known of their genetics and when, as in isolated fossil forms, their variance cannot be determined.

These chapters make up the first half of the book. The second is devoted to a discussion of the presumed parallel, but independent, evolutionary lines leading in the Far East from *Pithecanthropus*, through *Homo Soloensis* and Wadjok Man to the Australian aborigine; in Africa from *Africanthropus* through the Florisbad and Boskop types to the modern Bushman, Hottentot and African Negro; in Europe from *Eoanthropus* to the modern "Caucasians"; and in China and America from *Sinanthropus* to the modern Indians. The discussion demonstrates a surprising disregard for biometric and genetic facts. For example, the African, American and Australian lineages are derived from yet another uncritical acceptance of the results of non-quantitative investigations of various fossil forms. The history of physical anthropology shows only too often how erroneous such conclusions may be. What is so extraordinary is that Dr. Gates acknowledges R. A. Fisher's work in elucidating the inter-relationship of modern biometrics and genetics. But the fact that he allows the results of the non-quantitative studies of the Chancelade skull by Keith and Vallois to outweigh those of Morant's biometric analysis converts his acknowledgment into mere lip-service.

Having demonstrated to his own satisfaction that human races have evolved independently and in parallel, Dr. Gates concludes his book with a discussion of the principles of biological speciation. Throughout this final section, he emphasises that intersterility between groups does not correspond to any particular degree of morphological divergence, and is therefore worthless in deciding whether or not the groups represent distinct species. Little attention is given to the modern concept of species as inbreeding social and biological communities, and—except that one feels it cannot be true—Dr. Gates writes as though he does not realise that, whereas in palæontology taxonomic groups are artificial, and their definition therefore arbitrary, this is not so for extant groups. One of the clear implications of his description of five species of man is that they have different derivations from different strains of ape. This thesis, it may be noted, was extensively argued, a little more explicitly, by the late Drs. Klaatsch and Crookshank, but has never received any credence in responsible scientific circles. In reply to

an American reviewer who picked up this point, Dr. Gates now denies that the proposition—to which he refers as " a source of hilarity to students of the subject "—is ever made in his book. The fact that what is there, or seems to be there, in black and white is now denied leaves the present reviewer a little bewildered.

Human Ancestry from a Genetical Point of View first appeared in America, where its welcome from professional geneticists and professional anthropologists was hardly warm. It is difficult to see how it could have been otherwise. There was always reason for the student of anthropology to regret that prominent anatomical exponents of the subject neither seemed to know the lessons of genetics nor to be versed in the methods of biometric analysis. It looks as though the attentions of a geneticist who appears to know little human and primate anatomy, and who pays scant respect to the statistical methods which are so essential in genetical as well as morphological study, are also not going to do much good to a subject which suffers considerably from a lack of scientific discipline.

REVIEWS

MATHEMATICS

Elementary Calculus and Allied Geometry. By J. HARVEY, A.R.C.Sc., B.Sc. [Pp. viii + 493, with 231 figures.] (London: Hutchinson's Scientific and Technical Publications, 1949. 25s. net.)

MR. HARVEY has written a book for students of the physical sciences. The ground covered includes the calculus and co-ordinate geometry of the Pure Mathematics course for the B.Sc. General Degree of London University. There is a short discussion of complex numbers and power series of a complex variable. Amongst others there are chapters on Double and Triple Integrals, on Fourier Series and on Roulettes. Besides the relevant theorems, each chapter contains a number of worked examples and at the end a set of examples to be worked.

The book begins with the definition of such terms as "function," "continuous" and "convergent." The differential calculus is developed and the treatment presupposes no previous knowledge of the subject. The author also gives a brief account of the elements of co-ordinate geometry in two dimensions.

The principles which guide the order of the later chapters are not clear. Both physicists and engineers need differential equations early in their work, but the excellent chapter on this subject is the last in the book. Partial differentiation and the associated properties of surfaces are dealt with before integration is mentioned.

Assuming that some students will use this book by themselves, some of the detail is unfortunate. Mr. Harvey quotes Goursat and de la Vallée Poussin, but his own writing lacks the clarity and precision of these authors. In particular one would have liked him to make up his mind about the meaning of x^m when x is real and positive and m is a rational number.

The diagrams are well chosen and printed, although in a few cases the author has attempted to include too much at one time. The type is readable, but there are a regrettable number of misprints.

C. M. RIGBY.

ASTROPHYSICS AND METEOROLOGY

The Pulsation Theory of Variable Stars. By SVEIN ROSSELAND. International Series of Monographs on Physics. [Pp. viii + 152, with 25 figures.] (Oxford: at the Clarendon Press, 1949. 18s. net.)

THE problems of the variable stars have always occupied a major part of the astrophysicist's attention, for the understanding of the variable phenomenon would appear to be a valuable prerequisite to the study of stellar evolutionary processes. Early in the development of our ideas on

the internal constitution of the stars came the realisation of the interdependence of luminosity, mass and radius for stellar equilibrium conditions, and the most promising line of attack on the variability problem seemed to be the analysis of oscillations in the star's radius. The pulsation theory of variable stars has in fact proved most prolific of literature and most productive of result. Papers on the pulsation theory are widely scattered throughout the astrophysical journals of the world, and form a rather heterogeneous mixture, different authors starting from very different assumptions, and using different data. Prof. Rosseland has produced order out of chaos by synthesising into a coherent whole these diverse analyses. Inasmuch as the pulsation theory is still in many respects in its infancy (little attention has been devoted to other than purely radial oscillations, for example), Rosseland's work is not so much an account of a theory as an interim report on progress to date—and a very valuable report it is.

An introductory chapter on the origin of the pulsation theory, from the observational as well as theoretical standpoint, is followed by a general analysis of wave-motion to establish results to be applied later to specific problems. Treatment of radial pulsations of homogeneous stellar models in the adiabatic approximation leads to the derivation of relationships between period and mean density. The theoretical period-density relation, being insensitive to changes in the surface amplitude of pulsation, promises the most rigorous observational tests of the theory; such tests are examined. Integration theory for the treatment of harmonic oscillations enables departure from adiabatic conditions to be considered. Results are discussed in relation to the thermonuclear generation of energy, and the importance of the lifetime of an active nucleus on secular stability is emphasised. The mode of heat transfer inside the star is discussed in the light of the frequently observed phase lag between compression and luminosity. The theory of anharmonic pulsations, although not hitherto completely generalised, is shown to provide a qualitative explanation of the finite amplitude of Cepheid oscillations, departures from sinusoidal light-curves and long-period variations. Novæ are examined from the standpoint of relaxation oscillations, and shock-waves in particular are considered. A final chapter discusses various semi-empirical observational tests of the pulsation hypothesis.

The true worth of a scientific text lies in its influence on future developments in its sphere of interest. One cannot doubt that in the case of *The Pulsation Theory of Variable Stars* this influence will be immense, the more so as the author has taken pains to emphasise at all stages of the work not only the achievements but also the shortcomings of the theory.

The appearance of a new International Monograph is something of an event in the world of physics. Rosseland's second contribution to the series amply fulfils the promise of his first (*Theoretical Astrophysics*, 1936) and is worthy indeed to rank among its valuable companions. It will become a classic.

M. W. O.

—— **Electricity.** By J. ALAN CHALMERS, M.A., Ph.D. [Pp. iii + 175, with 36 figures.] (Oxford: at the Clarendon Press, 1949. 15s. net.)

THIS book provides for the first time in English a comprehensive textbook on all aspects of atmospheric electricity. Up till now most students have

had to be content with Schonland's excellent little monograph in the Methuen series published in 1932. Dr. Chalmers has now brought our knowledge up to date and provided nearly two hundred and fifty references. He has not attempted to include an account of cosmic rays, as Schonland did, and this fact, combined with the larger size of his book, enables him to treat the various branches of the subject in more detail and to commence with a short historical introduction.

This is a clearly written, well illustrated, and well-produced book, and all students of this branch of meteorology will welcome it.

G. B. B.

PHYSICS

Modern Introductory Physics. By IRA M. FREEMAN. [Pp. x + 491, with 271 figures.] (New York, Toronto, London: McGraw-Hill Book Co., Ltd., 1949. 27*s.* net.)

IN this volume the author sets out to present an introductory course in physics for liberal arts and science students in American colleges. This is provided on a selective rather than an encyclopædic basis, and the text is written in a clear, direct style which is more discursive than technical. It would appear that the main aim has been to present a coherent review of modern physics up to and including the recent developments in nuclear transformation, and this work occupies about one-third of the whole volume. It is by far the most valuable feature of the book and gives in well-ordered sequence a descriptive account of a very large number of topics in the field—in fact, almost all the important work and theories concerning the nature and structure of matter developed in the last half-century receive some attention and elucidation.

In order to make possible this generous allotment of space to the more recent developments in physics, the author has discussed in the remainder of the book "only those portions of classical mechanics, electro-magnetism, and optics which he feels are of more fundamental importance and which are needed for an adequate understanding of the newer advances." Here he is on less certain ground and not all his readers will agree that the choice of subject matter, or its presentation, provide the best basis for a comprehension of the more modern work. Thus of the remaining pages, 127 are devoted to a consideration of the simple principles of mechanics, and 97 to optics. There is a short section on waves and sound phenomena, but little or no space devoted to heat. The important section on electricity (and magnetism) is quickly dispatched in 67 pages, and throughout formal mathematical presentation is almost entirely avoided, many formulæ being quoted without proof or derivation. Experimental work too is inadequate.

The purposes and aims of Science and a discussion of its methods are most competently presented in a brief introductory chapter, but the claim that special attention has been given to the historical aspect of the subject can scarcely be substantiated from a series of scattered brief footnotes on scientific worthies. Each chapter is provided with a number of numerical questions, and a most useful reference list to general and more specialised reading. A short appendix is added giving a summary of the electrical units used in the book, and an outline of some (very) simple mathematical operations.

The book is produced in the handsome, almost opulent, style we have

come to expect of American publications, and, although unlikely to be used as a formal textbook in British schools and colleges, a copy on the shelves of the Science library would make a most welcome addition to works of a general nature on modern physics.

F. TYLER.

Principles of Mathematical Physics. By WILLIAM V. HOUSTON. Second edition. [Pp. xii + 363, with 41 figures.] (New York, Toronto, London: McGraw-Hill Book Co., Inc., 1948. 30*s*. net.)

THIS book was written, the author tells us, "to give students some competence in the technique of mathematical physics" and the present (second) edition "has been amplified to better serve this end."

The book will be useful to students working under a competent teacher; but its contents are compressed into too small a space to be useful to any student who has not access to someone able to help him over the more difficult parts. There is of course another class of reader who will doubtless study the book with interest and profit, namely that to which the present reviewer belongs—those readers who think they know already and understand all that is in it.

Apart from its severely condensed character, the book has many excellencies. It is expressly confined to classical methods: there is no quantum mechanics in it. This is a good thing; there are too many young students who are familiar with h and $h\nu$ and have no notion of Hamilton's principle. Among other subjects contained in the book, there are several mathematical chapters; chapters on mechanics, including Hamiltonian mechanics; vector analysis; thermodynamics and statistical mechanics and electromagnetism. Light, as such, is not included.

WM. WILSON.

Tables of Physical and Chemical Constants and some Mathematical Functions. By G. W. C. KAYE, O.B.E., M.A., D.Sc., F.R.S., and T. H. LABY, M.A., Sc.D., F.R.S. Tenth edition. [Pp. viii + 194.] (London, New York, Toronto: Longmans, Green & Co., 1948. 21*s*. net.)

WAR, and the death of the surviving original author, have seriously impeded the production of a new edition of this standard work. Prof. Laby died while printing was in progress, but fortunately he had lived to complete the revision of the ninth (1941) edition, aided by a team of colleagues in Melbourne, including Miss Jean Laby; so this new edition is his memorial, and a worthy one.

The main changes are concerned with revisions of the general constants of physics and of astronomy, which now are much more consistent. Certain sections, such as that on optical glass, have been rewritten and expanded. The general plan and conception of the work have not, however, been appreciably changed since the previous edition, nor in the circumstances is this to be expected; but it is in this connection that the reviewer wishes to make some suggestions for future consideration.

The compilation of such a set of tables as this, which cannot be exhaustive, but must combine maximum usefulness with minimum price, sets extremely difficult problems of choice. It is interesting to compare the solutions adopted

in this book with those in an American publication of comparable price. The range of material covered would appear to be much the same, though the reviewer makes no claim to have read either work from cover to cover, but the attack is very different. The present book aims not only at defining quantities and tabulating values, but at giving a brief reasoned account of how these are obtained and at providing full documentation. It is almost a text-book on principles of physical measurement, and as such is very valuable. On the other hand, the number of substances for which any one numerical datum is given is generally much smaller, in one instance by a factor of thirty-four, than in the American work.

This lack is likely to be felt especially keenly by chemists, who will further deplore the absence of such useful chemical information as electrolytic dissociation constants of acids and bases, standard electrode potentials, and activity coefficients of ionic solutions. One hopes that during the preparation of the next edition, in what we hope will be more settled conditions, this major defect will be remedied. The usefulness of the book would be increased thereby out of all proportion to the necessary increase of size.

The binding, paper, and printing are all of a very high standard.

L. E. S.

Intermediate Heat. By F. TYLER, B.Sc., A.Inst.P., Ph.D. [Pp. xii + 429, with 191 figures.] (London: Edward Arnold & Co., 1949. 12*s*. 6*d*. net.)

DR. TYLER has striven (i) to provide a complete course for H.S.C., Scholarship or Intermediate students and (ii) to include sufficient extra material to meet the additional needs of pass-degree students. Whether it is desirable to attempt to meet both requirements in one book is a matter of opinion. The reviewer feels that the general-degree student suffers unduly from the lack of a really good textbook on heat, written exclusively for him. Accordingly, he considers Dr. Tyler's book should be used in the schools rather than the universities.

The book contains on the whole an adequate presentation of H.S.C. heat, not forgetting the now customary chapter on certain aspects of meteorological physics. Thermodynamics is developed (in one chapter of 45 pages) to a stage sufficiently advanced for the general-degree reader. Some other requirements of this reader are met by insertions and additions to other chapters. There is an abundance of questions, arranged at the ends of chapters, which should prove invaluable both to students and teachers.

A number of slips have escaped detection during proof-reading. These will, no doubt, now be obvious. All points calling for comment cannot be mentioned here. A selection is: the method of correcting for heat exchanges between a calorimeter and its surroundings is certainly not adequate for both classes of reader. In Fig. 21 the ordinate should refer to the total heat of unit mass of the charge. On p. 44 molecular heat of vaporization is expressed in calories, whereas on p. 49 atomic heat is in calories per gm. atom per ° C. Fig. 61—Callendar and Barnes' continuous flow calorimeter—contains a common error. The sentence, "Calculation shows that 'the minimum rotational energy' is very much larger than the mean energy of translation of the molecules . . ." should obviously specify the temperature range under consideration. The definition of thermal conductivity as the quantity of heat per second flowing normally across unit area of a s

with unit temperature gradient under steady state conditions" is inexcusable, especially in view of the good work that immediately precedes it. E_λ as defined on p. 317, is not the same E_λ as appears in the formulæ on p. 322. "The emission spectrum 'of sodium' consists only of two lines very close together in the yellow region" is incorrect.

The second edition must not contain such points of criticism. It must have certain additions, one of the most outstanding being a chapter on convection. It is high time this important mode of heat transfer received proper treatment in an English book of this kind.

J. W. F.

X-ray Optics. The Diffraction of X-rays by Finite and Imperfect Crystals. By A. J. C. WILSON. [Pp. viii + 127, with 33 figures.] (London: Methuen & Co., Ltd., 1949. 6s. net.)

THIS book is a concise account of the theory of the diffraction of X-rays by crystals which, in one way or another, lack the perfect, infinitely recurring periodicity of the "ideal" crystal. It starts, at a point where most textbooks on X-ray diffraction leave off, with an account of the reciprocal lattice construction for representing the ways in which a crystal may diffract X-rays. This very useful concept is explained in simple geometrical terms, and forms the basis for all subsequent calculations.

The forms of imperfection considered are: finite crystal size, "mistakes," distortion, and thermal motion of the atoms. Mathematical expressions are obtained in each case for the broadening to be expected in the lines of an X-ray powder photograph, and in addition the results are explained in more intuitive terms. The mathematics is kept as simple as possible, and complex quantities and Fourier transforms are not introduced until near the end, where their meanings are adequately explained.

This is hardly a book for a non-specialist to read without having first gained some knowledge of X-ray diffraction. The title "X-ray Optics" on the outside cover is misleading, as it suggests a treatment of such topics as refraction, total reflection, and diffraction by slits. This is unfortunate, as the book appears in a Monograph series in which one expects general and fairly elementary introductions to broad subjects. The sub-title would make a much better title.

There are a few points at which confusion may arise; on p. 34 it is not made clear why one can add a number of intensities to obtain a resultant intensity, without taking any phase-relations into account, and in Chap. VI the letters A, B, and C each appear with two different meanings. But such faults are very few and far between, and Dr. Wilson is to be congratulated on being the first to have produced in a small volume such a full and clear account of a difficult subject.

A. R. STOKES.

Elementary Nuclear Theory. By H. A. BETHE. [Pp. viii + 147, with 17 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 15s. net.)

THIS outstanding book has resolved many of the problems of those engaged in teaching nuclear physics as a specialised subject to advanced undergraduates, and can be recommended without reservation for all students whose strength does not clearly lie in the direction of mathematical physics.

Organised teaching of nuclear physics is made difficult by the limited time which the student can properly devote to a single branch of physics, however vital, by the enormous volume of significant experimental work on which our knowledge of the nucleus has been built and by the advanced nature of the rigorous mathematical treatments of its problems. Dr. Bethe recognises that it is for experimental techniques and applications that the undergraduate may most usefully be referred to original material, and his references are uniformly well chosen and are moderate in number. His real achievement, however, lies in the way in which attention is maintained on the primary problems of the subject, and in the clarity with which the essential mathematical ideas are brought out.

The main text covers about 120 small pages, rather more than half of which are given to a discussion of the force between nucleons. The basic features of nuclear stability, the problem of beta-disintegration, and some features of the compound nucleus model with particular reference to interaction with neutrons and resonance phenomena, are also treated.

The average undergraduate will find many of the sections difficult, but he is the more likely to master them because of the author's scrupulous exclusion of all irrelevancies. While primarily a book for the young student, there are probably few experimental workers in the field of nuclear physics and its allied subjects who will not derive profit from Dr. Bethe's skill in exposition, and it is to be hoped that *Elementary Nuclear Theory* will have the widest possible circulation.

J. G. W.

Atomic Energy. By KARL K. DARROW, Ph.D. [Pp. x + 80, with 13 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 12*s*. net.)

THIS book is based closely on four lectures given in 1947 at North-Western University to a lay audience (one presumably expected to be deficient in any sort of acquaintance with the terms and problems of physics), and consists of a discussion of nuclear structure designed throughout to lead to the topic of energy release at fission; neither the technical challenge to be met in achieving the release of nuclear energy in practice, nor the wider problems opened up thereby, are raised at all.

It is difficult to see to whom this book will appeal: it does not cover in general terms the broad results and problems of nuclear physics, an approach which might interest and benefit the "ordinary man," nor does it contain any material to attract the specialist at whatever level; in fact, the detailed and laboured argument (leavened, perhaps, for the live audience by a rather painstaking facetiousness) makes a dullish book. Those familiar with Dr. Darrow's excellent series of reviews in the technical publications of the Bell Telephone Co. will hope that his next published work will be produced for the benefit of a more scientific public.

J. G. W.

Atoms in Action. By GEORGE RUSSELL HARRISON. Third edition. [Pp. x + 410, with 16 plates.] (London: George Allen & Unwin, Ltd., 1948. 12*s*. 6*d*. net.)

IT is readily seen why a third edition of this book has appeared. The author treats in a very lucid manner the physics of everyday problems ranging from

farming to medicine, as well as the developments that have derived from physics such as television and atomic energy. The book is plentifully illustrated with well-chosen photographs, although these do not obviate the need for diagrams, which are almost entirely lacking. The fact that this omission is not more noticeable is an indication of the clear and easy style of the writer. There is little further that need be said of this third edition, apart from recording that it has been brought up to date in many places, but one cannot refrain from quoting the naïve statement on p. 245: "In Europe, particularly in England, television has been more widely used than in America, a fact which appears to indicate that European television workers have outstripped their American colleagues. Such is far from the case: American laboratories have furnished more than their share of television developments, but government subsidies have enabled European manufacturers to put the new toy into the hands of the public earlier, and the European public is more easily impressed with imperfect pictures."

W. E. D.

Two Lectures: 1. The Present Situation in the Theory of Elementary Particles; 2. Electron Theory of Superconductivity. By W. HEISENBERG. [Pp. 52, with 3 figures.] (Cambridge: at the University Press, 1949. 3*s*. 6*d*. net.)

THE two parts of the book are entirely unrelated, but in each case the subject is approached in a more or less formal manner.

In the first lecture the difficulties of the modern theories and the attempts to surmount them are briefly reviewed. It is suggested that the infinities confronted in these theories are not so much difficulties as fundamentals from which a new theory should begin.

In the second lecture the "condensation theory" is introduced, which gives a formal picture of a metal in which there can be an electric current with no resistance on transference of heat.

W. E. D.

Electromagnetic Waves and Light. First Part. By CHARLES F. MEYER. [Pp. xii + 83, with 29 figures.] (Distributed by Ulrich's Book Store, 549 East University Avenue, Ann Arbor, Michigan, 1948. $1.35.)

IT is doubtful whether this first part should have been published separately when the reader is referred, at times, to later chapters and to an appendix which are to appear in Part 2, especially when the author declares that its publication "lies in the uncertain future." This is given as the justification for the separate publication of Part 1.

The book approaches the subject from a descriptive, non-mathematical viewpoint and is, on the whole, successful in giving a clear account of the subject; in fact, some of the later chapters are very effective. It is doubtful whether the first chapter on the Hertzian oscillator, with its detailed history of the lines of electric force, is the most suitable method of beginning such a book, and it might have been much more satisfactory to have departed from the strictly logical order and to have begun with the last two chapters of Part 1 on the Electromagnetic Spectrum and Spectral Data. These two

chapters are illustrated with a wealth of numerical data and would have led up more gently to the more difficult chapters on polarisation and homogeneity. Not that this part is not well done—in fact, the treatment is exceedingly good—but the concepts dealt with are difficult and might have been introduced by the more digestible chapters at the end.

W. E. D.

Fundamentals of Electric Waves. By H. H. SKILLING. Second edition. [Pp. vii + 245, with 86 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 24*s*. net.)

THIS is a revised and expanded edition of a book which was first published some seven years ago.

In choosing as its title *Fundamentals of Electric Waves* the author aspires to embrace a very wide field of study. It represents the kind of treatment that those of us who, from time to time, have endeavoured to correlate wave theory in its countless applications, cannot but regard as an ideal approach. Unfortunately the author has still not entirely achieved his objective, although the new edition represents a substantial improvement on the original. He adopts the engineer's train of thought throughout the volume, translating and interpreting ideas where possible in terms of familiar mechanical processes, which although not always providing a complete analogy are nevertheless distinctly helpful. Where he tends to fail is in linking up his arguments with those already well established in the field of optics and, as opportunity offers, drawing a parallel with other forms of wave motion.

The author first takes the reader through an introduction to the method of Vector Analysis and demonstrates how elegantly applicable this is to wave problems. He proceeds to establish the fundamental mathematical relationships of the electromagnetic field and introduces the concepts enunciated by Maxwell. This takes us half-way through the book, and the remaining chapters are devoted to a discussion of particular cases in the application of the more general theory. Thus Chapters 9 and 10 deal with plane wave propagation, power flow, skin effect and boundary conditions appropriate to reflection at different kinds of surface for both normal and oblique incidence. In Chapters 11 and 12 the radiation of electric waves is discussed, while consideration is given to the basic forms of aerial with their characteristic behaviour. Chapter 13 summarises the problem of guided waves in the microwave band, and the final Chapter 14 gives a brief but useful survey of the behaviour of waves in ionised gases with particular reference to the ionosphere.

At the end of each chapter there is a series of questions on the basis of which the reader can satisfy himself on the extent of his understanding of the preceding arguments. The book undoubtedly represents a very considerable achievement and maintains to the full the publishers' high standard of production. It goes a long way in its very worthy function of co-ordination and correlation in a subject which is becoming more important every day to the electrical engineer. Students will find in its pages a realistic approach to their problems, with a mathematical background which is at once precise and adaptable in practical application.

H. M. BARLOW.

Mercury Arcs. By F. J. Teago, D.Sc., M.I.E.E., and J. F. Gill, M.Sc., A.M.I.Mech.E., A.M.I.E.E. Second edition. Methuen's Monographs on Physical Subjects. [Pp. viii + 107, with 50 figures.] (London: Methuen & Co., Ltd., 1949. 6*s*. 6*d*. net.)

The tone of this book is set by the first page which contains a plain misstatement—that the mercury rectifier "depends on the unilateral conductivity of mercury vapour." Mercury vapour, of course, is a two-way conductor, as millions of mercury-vapour lamps nightly bear witness. The remainder of the book is stiff with misstatements of the same grave order.

One could criticise the book endlessly: there is so much wrong with it. To take a few points at random, the treatment of arc overlap on pages 47–9 is wholly inadequate; voltage regulation is discussed on pages 44–6 in terms of a star/star rectifier which no one would use; Chap. III on grid control discusses first the detailed technics of one of many types of circuit used for grid impulsing, then touches lightly on the theory of the subject, and omits its applications, for example, in cycloconversion; Chap. IV is simply an elementary consideration of the transient and steady currents in an A.C. circuit, and has no essential connection with rectifiers.

The presentation of the book is poor. Many of the figures are badly out of proportion. The 3-phase vectors in Figs. 21, 22, 23, 24, 30, 31, 37, 38, 40, 42, 44, 45 and 47 are not nearly equidistant. The paragraphing is bad. For example, the only treatment of the commonest of all rectifier systems (double 3-phase star) is inserted casually (p. 81) towards the end of a chapter on transformer ratings. To the present reviewer, Chaps. VI and XI are almost unintelligible. The mathematics is elementary, but the problems being attacked are scarcely stated.

The crowning insufficiency is in the treatment of A.C. current waveforms. Chap. VIII deals only with D.C. output waveforms, and ignores the A.C. input waveforms. The authors treat the A.C. waveforms as "hypothetical" (preface): say that certain waveforms "might well represent these phase currents" (p. 88), that "it is impossible to state the exact shape of the current waves" (p. 87), and that "the wave shapes cannot be expressed in simple mathematical form" (p. 71). These A.C. waveforms are, of course, as amenable to simple analysis as is the D.C. output, but this book reads as though these waveforms were indeterminate.

Many interesting modern developments, such as phase-doubling, are ignored; and the bibliography is pitifully inadequate. As a monograph for specialists the book is quite unhelpful: as a textbook for undergraduates it assumes too much and deals insufficiently with basic theory.

It is painful to have to review a book in such terms, but it would be sad if any Electrical Engineers specialising in mercury arc rectifiers thought that this book really represented the present state of knowledge, about the rectifier and its auxiliaries, in the English Universities.

G. H. Rawcliffe.

Pulses and Transients in Communication Circuits. By Colin Cherry, M.Sc.(Eng.), A.M.I.E.E. [Pp. xvi + 317, with 129 figures.] (London: Chapman & Hall, Ltd., 1949. 32*s*. net.)

For the electrical communications engineer, accustomed to steady-state alternating current theory, this book provides an excellent introduction to the analysis of transient effects in communication, television and radar

networks. It does not claim to describe all the methods of transient analysis, but adheres to one method throughout—the Fourier analysis development of transient response from steady-state response—and within this limitation is an excellent treatment.

More than one-third of the book is devoted to the fundamental properties of linear networks, the notations and methods of steady-state analysis, and the frequency spectra of modulated waves. Then follow chapters on the transient response of networks and on the simplifications available when idealised network characteristics are assumed. Typical of the author's treatment is his careful discussion of the validity of these assumptions and of their use and abuse.

Practical cases of multi-stage amplifiers and of their transient responses are then considered, with stress on the accuracy of signal reproduction such as is needed in television amplifiers. A particularly valuable chapter on Asymmetric Sideband Channels gathers in systematic form much information about single-sideband working and mistuned amplifiers, with transient and modulated signals. The final chapter analyses the response of transmission lines and lumped networks to very short pulses, explaining signal distortion in terms of echoes and reflections, and incidentally introduces the idea of the inverse nature of frequency and time variables. A generous bibliography for further reading, with many references in the text, is given with each chapter.

Mathematical treatment is often used in the book, but always as a means and never as an end in itself. The author has added greatly to the value of his work by explaining in physical terms what the mathematical process is about and what its results mean. Discussion of electrical waveforms rather than analytical functions is one of the ways in which the engineering character of the book is achieved.

Logical treatment, a clear style, careful explanations throughout, and neat format and diagrams, are all commendable features of the volume.

H. M.

Advances in Electronics. Vol. I. Edited by L. MARTON. [Pp. xii + 475, with 168 figures.] (New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1948. 49*s*. 6*d*. net.)

THIS is the first of a yearly series of books intended to present critical reviews of selected topics in physical and engineering electronics. It is intended for research workers, who are inevitably restricted to a limited part of this subject, so that they may have an appreciation of advances made in other fields.

In this volume there are competent and useful reviews of various physical electronics subjects. Secondary Emission, Television Tubes and the Problem of Vision, and Particle Accelerators are particularly well covered, with Mass Spectroscopy and Oxide-coated Cathodes closely following. The chapter on the problem of vision by A. Rose of R.C.A. contains some most interesting fundamental work, where the ultimate limitations of vision are discussed in terms of the efficiency of quantum appreciation. Two chapters on the deflection of charged particles are much too mathematical for the general intention of this book, and in fact are suitable only for a specialist in this field.

The engineering side is represented by reviews of Ionospheric Research, Cosmic Radio Noise, Propagation in the 90 to 100 Mc./s. Region (here called the F.M. band), and Radio Aids to Navigation. The chief merit of these,

as is often the case with short reviews, lies in the fairly complete list of references to all except the last chapter.

The text of the book is clear and it is well illustrated. There is an increasing need for general surveys of this kind, and this series should be of great use.

F. G. S.

Centrifugal and Other Rotodynamic Pumps. By HERBERT ADDISON, O.B.E., M.Sc., M.Inst.C.E., M.I.Mech.E. [Pp. x + 492, with 230 figures and 2 folding plates.] (London: Chapman & Hall, Ltd., 1948. 36s. net.)

FOLLOWING the style of presentation which Prof. Addison adopted in his earliest text, his treatise on Applied Hydraulics, the latest of his works shows the same lucidity of writing. The book, which deals with most forms of pumping machinery, is divided into four portions.

The first of these discusses the principles of ideal pumps. Vector figures and flow patterns are described chiefly for impellers having a large number of blades. Accordingly, the various theories of the working of the axial flow pump, namely the momentum, blade strip and vortex theories, are not mentioned. Possibly these need a more detailed knowledge of mathematics than the author thought it desirable to assume in a text which appears to be written chiefly for the benefit of pump users rather than designers.

For the prospective buyer who wants to know what types of pump are available, what may be expected of them and suitable layouts of pipeline, the text is ideal. He will also find sufficient theory to give him a clear grasp of the principles, enough at least to allow him to compare the characteristic curves of pumps offered by different makers.

The author's simple explanations of the effect of counter-rotation, the influence of the number of blades and the acting of the casing of the centrifugal type in converting kinetic to pressure head are excellent.

It has long been known that the specific speed of a water turbine, and hence of a pump, depends numerically on the system of units employed, whether British or Metric. The text shows a method of overcoming this difficulty, by the simple modification of multiplying the head by g, the value of the gravitational acceleration, expressed in the units in which Q and H are measured. This new pump characteristic has some obvious advantages, and the author has aptly named it "the shape number."

Part B is concerned with design and construction, covering, in the case of centrifugal pumps, effects of axial thrust, types of sealing rings, casings and general principles of impeller design. This is followed by an elementary discussion of the axial-flow machine, and of pumps with multiple rotors.

The third section deals with the problem of performance, and should be of the greatest value to the user or designer of the complete pumping scheme. It covers a detailed analysis of the head on a pump, testing performance under design conditions, as well as under reduced and increased flow. The effects of the pipe circuit, suction lift and water hammer are also discussed.

The final portion handles the questions of allied machinery, the complete pumping-plant, including various methods of grouping.

A well-chosen set of 48 numerical examples, each with a full solution, followed by a very complete bibliography, ends a volume which must be regarded as an excellent addition to the literature on pumps.

B. J. LLOYD-EVANS.

Fundamentals of Vibration Study. By R. G. MANLEY, M.Sc. Second edition. [Pp. xvi + 156, with 53 figures.] (London: Chapman & Hall, Ltd., 1948. 15*s*. net.)

UP to the time of the first world war the theory of vibrations was usually regarded as too difficult a subject for the engineer. It was, in fact, looked upon as suitable only for a firm's mathematician. In more recent times every college course has included the elements of the subject.

Owing to the steady rise of rotational speed of machinery over the last thirty years, the subject has become of more and more importance to the practising engineer. A need therefore arose for texts giving the fundamentals of the theory useful alike to students and engineers.

As an introduction to the subject it would be difficult to discover a more suitable work than Mr. Manley's *Fundamentals of Vibration Study*, now in its second edition. It is indeed more than an introduction, for each step in the mathematics is developed clearly and concisely from first principles.

The subject is treated under the headings of systems having one degree of freedom for both linear and torsional vibrations, undamped, damped and forced; undamped motion with two degrees of freedom; systems with many degrees; heavy shafts and beams; with a final chapter on Fourier analysis.

Naturally there is a limit to what can be discussed in a given size of book, but it is a little surprising that the whirling of a shaft is not mentioned. Possibly this could be added in another edition to an otherwise comprehensive introduction to the subject.

Appendices are given on mathematics, pure and applied, and on the practical treatment of vibration. Throughout the book there are numerous exercises and worked examples.

Readers should have little difficulty in applying the principles, here so well set out, to their particular problems.

B. J. LLOYD-EVANS.

Elasticity and Anelasticity of Metals. By CLARENCE ZENER. [Pp. x + 170, with frontispiece and 56 figures.] (U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1949. 22*s*. 6*d*. net.)

THE appearance of this, the first of a series of monographs on the science of metals sponsored by the Institute for the Study of Metals of the University of Chicago, is an event of major importance in the field of theoretical metallurgy. A more auspicious choice than Clarence Zener on Anelasticity could scarcely have been made, since the subject is one of fundamental importance in theoretical metallurgy and the author has unique qualifications to write on it by virtue of his own outstanding contributions in its development.

Some of the matter has not previously been published and the present monograph is the first connected and thorough treatment of the subject. Details of experimental techniques are not included, the author's concern being to develop the theoretical interpretation of the various types of anelasticity and to review the new knowledge which has been obtained through such studies.

The monograph presents a critical discussion of the physical origin of anelasticity, which the author defines as: the property of a solid in virtue of which stress and strain are not uniquely related in the pre-plastic range. After a concise section on elasticity of metals, expounding the formal relations

between stress and strain in isotropic and crystalline media, the bulk of the monograph is devoted to anelasticity: formal theory, principles of measurement and physical interpretation. The latter is treated under two heads, homogeneous and inhomogeneous relaxation respectively. The section on homogeneous relaxation covers: general theory of relaxation by diffusion; relaxation by thermal, atomic and magnetic diffusion; relaxation of ordered distributions and relaxation of preferential distributions. In the section on inhomogeneous relaxation three types of stress relaxation are considered, *viz.* along previously formed slip bands, across grain boundaries and across twin interfaces.

It is clear that this monograph will be essential reading not only for those engineers and metallurgists directly interested in anelasticity (*i.e.* internal friction, damping), but for all serious students of metal physics and theoretical metallurgy.

A. B. WINTERBOTTOM.

Underwater Explosions. By R. H. COLE. [Pp. x + 437, with 7 plates and 121 figures.] (Princeton, N.J.: Princeton University Press; London: Oxford University Press, 1948. 42*s.* net.)

VERY great progress was made during the recent war in the study of phenomena associated with explosions. The stern character of the war at sea necessarily directed attention particularly to underwater explosions. At quite an early stage the Admiralty acted on advice given them by many scientists, both from within Government service and from outside, in establishing UNDEX (Underwater Experimental Establishment) at Rosyth. This step was taken although it was realised that short-term results were not likely to accrue, but it indicates the high level of appreciation of the value of intensive scientific research on the variety of problems concerned with underwater explosions. Much work of a brilliant technical character was carried on in the U.S.A. Dr. Cole's book is devoted to a description of present knowledge of the subject as a result of work carried out in this country and in the U.S.A.

In many respects the subject is of especial interest in itself as providing an outstanding example of phenomena which cannot be described by linear methods—the wave disturbances concerned are definitely not of small amplitude. The mathematical development runs parallel to the other non-linear subject which has swollen greatly in importance in recent years for largely military reasons—gas dynamics.

Our greatly increased understanding of underwater, and indeed of all, explosion phenomena has come about, not only by theoretical advances, but also by the introduction of remarkable new techniques of measurement, which are described very clearly in Chapters 5 and 6 of Dr. Cole's book. These include in particular the methods of measurement of explosion pressures and of photographing the development and subsequent form of the gas bubble produced when the explosion occurs underwater.

One of the most interesting results of the photographic study of gas bubbles was the confirmation of much earlier theories of Butterworth and G. I. Taylor that pulsation of the bubble will occur during its passage to the surface. If, as a result of these pulsations, the bubble contracts to a minimum value, a further shock will result when the minimum is approached. As in practice with, say, an underwater mine explosion, this second shock will occur much

closer to a ship than the initial one, due to the rise of the bubble, it may lead to more damage than the first. This means that the law relating amount of damage with distance from the charge must be a very complicated one, depending on whether or not a second shock will occur before the bubble breaks surface.

Dr. Cole gives a thorough, readable and clear account of these and many other phenomena, discussing the theoretical and experimental technique and their application to this difficult but interesting subject. It is thoroughly up to date and should prove valuable, not only for its own sake, but to students of related subjects which are essentially non-linear in character.

H. S. W. M.

Rockets and Space Travel. By W. Ley. [Pp. viii + 374, with 8 plates and 49 figures.] (London: Chapman & Hall, Ltd., 1948. 18*s*. net.)

Between the wars, great interest was manifested in rocket flight and many experiments were carried out, especially by German and Austrian scientists. The decade preceding the second world war saw the boosting of this form of propulsion by the millionaire Opel with his racing car and the producer Fritz Lang with his film *Frau im Mond*. Curiously enough, the Nazis made little use of these men and their work, the V2 being the invention of a Count von Braun who secured the ear of Hitler because of his typical Nordic features! Meanwhile, the British had applied the rocket weapon in all three fighting services.

Although the present author gives us a history of these developments, he —like most of those who have written books on the rocket—is only interested in the possibility of a *Weltschiff*. Experiments on our earth are a waste of time unless directed towards the object of getting off it on an interplanetary excursion, or at least a trip to the moon. In spite of this King Charles' head of the rocket-planners, the theme of this book is presented scientifically and the history of the subject is given in an intriguing way, though with some unfair emphasis on German priorities. (For instance, it is stated that the Franco-British system of echo-sounding with ultrasonic waves developed from the *Echolot* of Behm, whereas the former was in use in 1918, while Behm's invention was published in 1923 and soon abandoned.)

The author does not pretend that a human being could survive a direct shot to the moon on the Jules Verne method, but discusses the possibility of a gradually increasing acceleration by a train of rockets. The prodigious power and masses involved are frankly set out, but one misses any adequate discussion of the maintenance of the stability of the flight. This seems to be a common hiatus in books on *Raumschifffahrt*, yet anyone who has had to deal with bombs and torpedoes, rocket-propelled or otherwise, will know that this is the most important aspect of their ballistics.

E. G. R.

CHEMISTRY

Smith's General Chemistry for Colleges. By William F. Ehret. Sixth Edition. [Pp. xiv + 677, with 264 figures.] (London: G. Bell & Sons, Ltd., 1946. 20*s*. net.)

It is a long time since the "Alexander Smith" series of textbooks made its appearance on the stage of chemical education; yet so firmly is that author

established as a student's mentor that the issue of a new edition is still an occasion of some moment. Based on a draft first prepared in 1900, his *Introduction to Inorganic Chemistry* grew out of a course of lectures which the author had already been giving for ten years; its appeal was frankly to the experience of the individual by stating generalisations so far as was possible in terms of observed facts, and no pains were spared to make the explanations clear and logical. The first edition was published early in 1906; subsequent rapid advances in chemical theory were reflected in revised editions, of which three were presented, without loss of individual character, by Prof. J. Kendall. The sixth edition, entitled *Smith's General Chemistry for Colleges*, is the work of Prof. W. F. Ehret of New York University, and unreserved tribute can be paid to the way in which he has carried out his task. It is claimed that every effort has been made to write into the text that which is now accepted by reliable sources as the most accurate in fact and the most plausible in theory; the effort has indeed been worth while, and the result is a text that is a pleasure and a profit to read. Changes in the content of some chapters, though seldom in their titles, have been made in the interest of logical exposition. The author deliberately attempts to make the student "structure-minded" so as to clarify the relation between cause and effect and to excite reason rather than memory. An important feature of the book is the inclusion, at the end of each chapter, of a wide selection of references to original articles—principally in the *Journal of Chemical Education* and *Industrial and Engineering Chemistry*—designed to lead the interested student towards wider aspects or finer detail than is appropriate in any general text book. While few would dissent from the view that proficiency in chemistry need not necessarily be dependent on the inclusion of the subject in the school curriculum, the author's statement that generally there is little real advantage to be gained by that early training is surely open to challenge. However, whether he possesses or lacks that elementary instruction, the interested student of chemistry will find in this book a patient and well-informed guide.

A. A. E.

Outlines of Physical Chemistry. By Farrington Daniels. [Pp. viii + 713, with 164 figures.] (London: Chapman & Hall, Ltd., 1948. 30*s*. net.)

Getman and Daniels has long provided a good introductory textbook of physical chemistry, and in this volume, which appears as the first edition of a new book under the authorship of Prof. Daniels alone, further extensions and rearrangements have been made, which now render it an excellent treatment of the subject.

No previous knowledge of physical chemistry is assumed, and the fundamental principles of the subject are approached in a thorough, systematic, and up-to-date manner. Its scope is rather wider than is usually regarded as necessary for a general degree in this country. Each topic is treated in a realistic way, and is illustrated, where possible, with diagrams relating to actual systems and an excellent series of worked numerical examples. In addition, a good selection of problems is supplied for solution by the student.

A novel feature is the inclusion, as a short appendix, of the derivations of a number of equations and other mathematical aspects of the subject. This has the advantage of rendering the main text less terrifying on the

first approach, whilst the material remains available for study when the interest has become aroused.

To preserve the balance of the book a rather more extended treatment of the modern theory of electrolytes and of the applications of conductivity measurements might have been desirable, but when such a wide subject is covered in a book of moderate size it is inevitable that some branches should be dealt with in a rather brief manner. The fact that in some instances experimental apparatus and methods receive little more than a mere mention should prove of no disadvantage provided that the book is read concurrently with a good course in practical work.

The whole book is written in a most attractive style, and it can be thoroughly recommended.

J. W. S.

Advances in Catalysis and Related Subjects. Vol. 1. Edited by W. G. Frankenburg, V. I. Komarewsky and E. K. Rideal. [Pp. xiv + 321, with 82 figures.] (New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1948. 43*s*. net.)

The importance of catalysis in industrial chemistry has focused attention most sharply on the efficiency and conditions of operation of a few well-known processes; this may be one of the reasons why many aspects of the subject still retain the attributes of an art rather than of a science. Certainly most books have tended to emphasise the method of experiment rather than the interpretation of results and the presentation of theoretical concepts. The present volume, the first of an annual sequence, is, therefore, an important one, since it attempts an appraisal of the progress made towards an understanding of the fundamentals involved in catalytic processes. The three editors have been assisted by P. H. Emmett and H. S. Taylor, and there is an imposing list of American and British contributors. It is perhaps intentional that there is no order in the sequence of the separate articles and that one may find the different and often opposed views of the various research schools in the different sections. Nevertheless, the standard throughout is uniformly high, and this book will certainly be widely read and carefully studied.

The first article by H. S. Taylor is concerned with the heterogeneity of catalytic surfaces for chemisorption and, in the main, is a review of the work carried out in the Princeton Laboratories. The treatment is general and the viewpoint that of an experimentalist. The author still inclines to the view that the surface curvature of the catalyst is a primary factor in heterogeneity as applied to chemisorption, and D. D. Eley in his chapter on the catalytic activation of hydrogen on metals has criticisms of this outlook. Thus the latter rightly urges that the notion of "active spots" must remain a relatively sterile hypothesis until more is known about the characteristics of uniform surfaces. However, he later suggests that the surface of a completely aged tungsten wire will approximate to this ideal state. It is probably more true to say that surface lattice defects (still present on such a wire) will be found responsible for most of the characteristics now associated with heterogeneity. This chapter, though duplicating much of the material in Roberts' *Some Problems in Adsorption*, is a clear and comprehensive account of the work of the Cambridge school under E. K. Rideal.

The chapter by M. H. Jellinek and I. Fankuchen on the application of

X-ray diffraction is a little disappointing; too much space is devoted to description of well-known methods and too little to examples of applications already described in the literature and to the possible wide uses of this technique. Related in subject matter is an excellent contribution by R. H. Griffith on the geometrical factor in catalysis. Wisely limiting the discussion to a few well-defined systems, he has dealt clearly and critically with the geometry of a system and its relation to the nature of both the adsorbent-adsorbate bonding and surface heterogeneity. On a more general topic, the measurement of surface areas by the B.E.T. method is a contribution from P. H. Emmett. It comprises a comprehensive but highly condensed account of the value and importance of the method. Many would like to share the confidence of the author in his statement that such measurements "now rest on a very solid foundation" and that the method "can be used with confidence."

The remaining three articles, "Alkylation of Isoparaffins" by V. N. Ipatieff and L. Schmerling, "The Synthesis of Hydrocarbons by Hydrogenation of CO" by H. H. Storch, and "The Isomerization of Alkanes" by H. Pines, will be enthusiastically welcomed because of the great economic and industrial importance of these three rapidly developing fields. In the first the authors have limited the treatment to a critical discussion of alkylation and of the reaction mechanisms. This contribution supplements and brings up to date the comprehensive summary of Egloff and Hulla (*Chem. Rev.*, 1945, **37**, 323). The second is a thorough and well-balanced survey of hydrocarbon syntheses which will be greatly appreciated by chemists, if only because so much of the literature on this subject is not easily available. The third article on isomerisation of alkanes is concerned mainly with the butanes, pentanes and hexanes. Details of various reaction mechanisms and of commercial methods are included in this well-written and authoritative contribution.

F. C. T.

Colloid Science. Vols. I and II. By A. E. ALEXANDER and P. JOHNSON. [Vol. I: pp. xxii + 1–554, with 208 figures, including 6 plates; Vol. II: pp. viii + 555–837, with 100 figures, including 1 plate.] (Oxford: at the Clarendon Press, 1949. 60*s*. net the set.)

IN these two volumes, in which printing, illustration, and materials maintain the high standard of the Clarendon Press, the authors present "a modern treatment of colloidal systems intermediate in level between the elementary, purely descriptive text-books and the specialised accounts of certain aspects available in monographs and the 'recent advances' type of article." The book is in three main parts, in the first of which (127 pp.) an historical survey is followed by a discussion of some general principles, a chapter on thermodynamics and its application to colloidal systems, and another on the application of the Debye-Hückel theory to colloidal particles and the stability of sols. The second and largest part (426 pp.) is devoted to methods of investigation, including the measurement of osmotic pressure, membrane potential, sedimentation equilibrium, sedimentation velocity, translational and rotational diffusion, electrokinetic effects, viscosity, plasticity, elasticity, optical properties, and X-ray and electron diffraction. A relatively short section (66 pp.) deals with interfaces, monolayers, adsorption, and kindred

matters. Part III (278 pp.) contains an account of some important colloidal systems, *viz.* sols, gels and pastes, foams, emulsions, colloidal electrolytes, clays and zeolites, proteins, natural and synthetic polymers, and membranes.

So far as the matter under discussion allows, the treatment throughout is fundamental and quantitative. No excursions are made into the field of industrial or other applications. Whilst the range of subjects dealt with amply justifies the title *Colloid Science*, the relative space allotted to different sections does not always correspond with what many readers may consider to be their relative importance. This unevenness is to a great extent intentional, since it has enabled the authors, by a relatively brief treatment of topics (*e.g.* inorganic sols, emulsions, properties of surfaces) that have been adequately dealt with elsewhere, to do fuller justice to recent developments of which descriptions are not readily available. The sections on osmotic pressure, sedimentation, diffusion, electrophoresis, rheology, and optical properties deserve special mention for the admirably clear account they give of modern techniques, several of which make use of the versatile "Schlieren" method of locating concentration gradients. The separate treatment of foams, pastes, zeolites, and membranes is a welcome recognition of their present-day importance. There will doubtless be many who will regret the omission or cursory treatment of certain matters of special interest to them. In a general treatise of limited size this is probably unavoidable. In the reviewer's opinion the space at the authors' disposal has on the whole been used wisely, having regard to the accessibility of other sources of information on the topics to which relatively little space is given. Very few misprints have been noticed: Berkley for Berkeley on pp. 75 and 99; w for ω in the formulæ on p. 83; n_1 for n_t in the second line above the formula on p. 787; Newman for Neumann on p. 551. On a point of detail, it should be noted that the conclusion (pp. 38, 39) that an electric charge of constant surface density increases the solubility of very small particles is true only when the charge is considered in isolation from the ionic adsorption that, in aqueous systems, normally gives rise to it. The process as a whole must result in a net decrease of free energy, and a solubility lower than that of the same particles with an uncontaminated and therefore uncharged surface.

The book is written in a simple and direct style which combines a judicious economy of words with clarity in description and exposition; it is packed with interest and is never dull or stodgy. Much of the subject matter is the product of the writers' first-hand experience of the problems discussed. Their opinions may not everywhere coincide with the reader's own, but that is a healthy sign that the subject is still in a state of active development. Both the theoretical and experimental aspects of the subject receive their due share of attention. Work published as recently as 1948 is reviewed in the text. A uniform system of symbols is employed and adequate cross-references are provided. There is a subject index only at the end of the second volume.

The authors have done an important service in selecting and assembling a great deal of material which should be especially valuable to teachers, academic and industrial research workers, biophysicists, and physically inclined biochemists. To these and others interested in the modern development of colloid science this book is warmly commended.

F. L. USHER.

Technique of Organic Chemistry. Vol. II: Catalytic, Photochemical and Electrolytic Reactions. Editor A. WEISSBERGER. [Pp. x + 219, with 66 figures.] (New York and London: Interscience Publishers, Inc., 1948. 30*s*. net.)

THIS volume is the second of a series of six designed to cover the techniques which are used in organic chemistry. The first volume, under the title *Physical Methods of Organic Chemistry*, was issued in two parts and extended to 1367 pages. By comparison this second volume is a slender compilation of some 219 pages, which are devoted to catalytic, photochemical, and electrolytic reactions. The section of Catalytic Reactions, by V. I. Komarewsky and C. H. Riesz, is in no sense a comprehensive treatment of the subject. A few pages are devoted to the preparation of solid catalysts in various forms, by such processes as ignition, impregnation, precipitation, or dispersion, and there follows a more detailed discussion of laboratory equipment and methods for carrying out reactions at different pressures. This section gives a very detailed account of the construction of autoclaves, valves, safety devices, forms of agitation and ancillary equipment. The authors admit that much of the equipment described is not necessarily peculiar to catalytic reactions, and may be equally useful for non-catalytic processes. Although these pages are full of useful information, they refer primarily to equipment available in America. One is forced to admit that the title of this section is rather grandiose and certainly misleading.

The second section on Photochemical Reactions, by W. A. Noyes, Jr., and V. Boekelheide, reviews the usefulness and limitations of photochemical reactions as preparative methods in organic chemistry. Attention is devoted to light-sources, quantum yields, and the design of photochemical apparatus, and the behaviour of various organic compounds on irradiation and chain reactions initiated by light are discussed. The third section on Electrolytic Reactions, by Sherlock Swann, Jr., is devoted to the mechanism of electrolysis, the design of equipment, and to a general consideration of reactions both at the cathode and anode. Much useful information has been accumulated in this section, but it is unfortunate that in many instances original references are not given, but instead the reader is referred to tables contained in earlier papers by the author in the *Transactions of the Electrochemical Society*. The reader is left with the impression that the application of both photochemical and electrochemical reactions as aids to synthetic organic chemistry deserves much further study, which would doubtless lead to the successful development of many useful preparative methods. The real value of this part of the work is therefore to be found as much in the revelation of future promise as in the presentation of past achievement.

D. H. H.

Vacuum Manipulation of Volatile Compounds. By R. T. SANDERSON. [Pp. viii + 162, with 40 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 18*s*. net.)

THE manipulation of volatile substances *in vacuo* constitutes a technique which most research chemists are forced to employ at some time during the course of their work. If they have not had the advantage of learning from others already skilled in the art, then reference to a good textbook on the subject may save many hours of misplaced endeavour.

Unfortunately such books are rare. The manual by Dr. Sanderson will

go some way towards supplying this need, but it cannot be described as a "good" book. Its principal fault is one of omission. Thus there is no reference to working in quartz or of the attachment of apparatus made in this material to ordinary vacuum lines. The chapter on the measurement of high vacua, in its context surely one of the most fundamental of topics, contains no mention of rotary McLeod gauges. Again there is complete silence on the subject of glass, spoon or diaphragm gauges for the measurement of pressure, and on the subject of quartz spiral manometers. This list could be multiplied and, indeed, such omissions detract seriously from the merits of the book.

The conclusion to be drawn is that the manual deals adequately with a limited number of topics from the point of view of the beginner. It can, however, hardly be taken as a serious treatise on vacuum technique.

There is an appendix, which should prove useful, for it gives the vapour pressures of many organic compounds, and other relevant information.

D. H. R. BARTON.

Recent Advances in Organic Chemistry. Vol. III. By ALFRED W. STEWART, D.Sc., and HUGH GRAHAM, D.Sc. Seventh edition. [Pp. xii + 387, with 2 plates and 3 figures.] (London, New York, Toronto: Longmans, Green & Co., Ltd., 1949. 38s. net.)

VOL. II of earlier editions of Stewart's *Recent Advances* has been expanded to form Vols. II and III of the new seventh edition. Of these, Vol. II has already been reviewed in SCIENCE PROGRESS (1949, **37**, 367). The present Vol. III includes chapters on bile acids and sterols, vitamins, hormones, cardiac aglycones, some natural porphyrins and related compounds, aza porphyrins, synthetic high polymers and condensates, rubber, some deutero-organic compounds, some cases of isomerism in cyclic compounds, the diphenyl problem, some aspects of stereochemistry, new organo-alkali compounds, other cases of abnormal valency, structural formulæ and their failings, some applications of electronics to organic chemistry, some unsolved problems and appendices on folic acid and the synthesis of vitamin A.

As with Vol. II, there is little in this volume to justify the title *Recent Advances*, for there are only about a dozen references in the whole book to original work reported since 1940, and in certain of the chapters on natural products, notably those on bile acids, sterols and sex hormones, there is no work reported less than fifteen years old. The implications of this are of course obvious; the treatment of these subjects is inadequate, *e.g.* there is no mention of testosterone in the male sex hormones, or of the syntheses of equilenin and œstrone, and the account of thyroxine dates back to 1928. In many places the text is misleading, *e.g.* the account of the work on α-biotin (p. 61) is based on the incorrect formula, vitamin A is stated not to have been isolated in a crystalline condition (p. 35), and incorrect formulæ are given for muscle adenylic acid (p. 52), bilirubin (p. 134) and certain sterols (p. 107). Recent syntheses of muscle adenylic acid and bilirubin are omitted.

The chapters on natural products are, however, the most up-to-date in the book, and the chapters on theoretical organic chemistry (pp. 312–60) might well have been in a book on historical chemistry, for there is no reference to original work published since 1925 and it is quite obvious that the authors have made little effort to revise this portion of the book. Detailed

criticism would be superfluous. The chapter on organo-alkali compounds likewise dates back to 1925 and the important contributions of Ziegler and Morton and many others are entirely overlooked. Emphasis throughout the book tends to be laid on historical aspects rather than recent advances; thus more than half the chapter on rubber is devoted to the polemics of Harries on priority and structure, but no account is given of the modern ideas on the subject. These examples are by no means exhaustive and have been chosen quite at random.

The price is high for a book of its size, and, although the print and formulæ are good throughout, there are very few chapters in the book which can claim to review the recent literature of organic chemistry.

A. W. JOHNSON.

Organic Reactions. Vol. IV. Editor-in-Chief, ROGER ADAMS. [Pp. viii + 428.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 36*s*. net.)

THE series of *Organic Reactions* has proved to be a most popular and successful contribution to the literature of organic chemistry and the present volume fully maintains the standard of its predecessors. Eight reactions are covered: the Diels-Alder reaction with maleic anhydride; the Diels-Alder reaction, ethylenic and acetylenic dienophiles; the preparation of amines by reductive alkylation; the acyloins; the synthesis of benzoins; synthesis of benzoquinones by oxidation; the Rosenmund reduction of acid chlorides to aldehydes; and the Wolff-Kischner reduction. Following previous practice, the subjects are presented from the preparative viewpoint and particular attention is given to limitations, interfering influences, effects of structure and the selection of experimental techniques. Each chapter contains several detailed procedures illustrating the significant modifications of the method, and in addition tables are given to list, as far as possible, compounds which have been prepared by or subjected to the reaction. Gaps in these tables are inevitable, but no outstanding omissions were noted. The binding and general layout of the book are good, although it is to be hoped that the growing practice of steadily increasing the price of this and similar series with the appearance of successive volumes will soon be reversed.

A. W. JOHNSON.

Organic Syntheses. Vol. 28. Editor-in-Chief, H. R. SNYDER. [Pp. vi + 121.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 15*s*. net.)

THE latest volume of *Organic Syntheses* contains detailed preparations of 37 organic compounds and generally follows the pattern and format of previous volumes. The compounds described are 2-acetothienone, 2-acetylfluorene, 9-acetylphenanthrene, 2-allyl*cyclo*hexanone, *o*-aminobenzaldehyde, *p*-aminophenyl disulphide, benzoyl disulphide, 9-bromophenanthrene, 4-bromo-*o*-xylene, 3-carbethoxycoumarin, *p*-chloroacetylacetanilide, *m*-chlorophenylmethylcarbinol, *m*-chlorostyrene, 9-cyanophenanthrene, *trans*-1:2-cyclohexanediol, 4:7-dichloroquinoline, 2:5-dihydroxyacetophenone, di*iso*valerylmethane, 3:4-dimethylaniline, 2:4-dimethylquinoline, 1:4-dinitronaphthalene, diphenylacetonitrile, ethyl azodicarboxylate, ethyl ethoxymethylenemalonate, fluorenone-2-carboxylic acid, hexamethylene chlorohydrin, hydroquinone diacetate, 2-hydroxycinchoninic acid, *dl*-*iso*propylideneglycerol, methyl

4-keto-7-methyloctanoate, 4-nitro-1-naphthylamine, *p*-nitrophenyl sulphide, phenanthrene-9-aldehyde, 1-phenyl-3-amino-5-pyrazolone, α-phenylthiourea, 2:4:7-trinitrofluorenone and vinyl chloroacetate. In addition the volume contains a collected subject index for Vols. 20–28. The high standard of this series is fully upheld.

A. W. JOHNSON.

The Preparation, Properties, Chemical Behaviour and Identification of Organic Chlorine Compounds. By E. H. HUNTRESS. [Pp. xxvi + 1443.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. £8 5*s*. net.)

THIS is one of those massive works which seem to be going to characterise the present age in chemistry. To a man engaged in research, either academic or technical, this volume will be of great value, even if he is not specialising in organic compounds containing chlorine. For a very large number of substances the whole literature is set out very clearly and, from the point of view of simple physical constants (m.p. and b.p.), very revealingly.

The order in which the 1320 compounds mentioned appear in the text is at first sight peculiar. About 500 pages are devoted to solids, then nearly 400 pages to liquids having boiling-points recorded at the ordinary pressure and with density greater than 1·15, and about 350 pages to liquids of density less than 1·15, and so on. It is not clear to the reviewer why an order based solely on empirical formulæ would not have been as satisfactory. As it is, the six dichlorophenols, for example, are not found anywhere near each other. However, as the author says, there are five different types of index and it is undoubtedly very easy to use the book quickly and effectively. In the empirical formula index one also finds the molecular weight and the chlorine content of every compound. The 22,000 references, including those to "Beilstein," are clear and appear to be exhaustive up to 1945: there are extra references to cover the period 1946–47. The book is admirably printed and bound and Prof. Huntress is to be congratulated for having produced it.

E. E. TURNER.

Analytical Chemistry and Chemical Analysis, 1948. Proceedings of the International Congress on Analytical Chemistry, Utrecht, 1948. [Reprint of *Analytica Chimica Acta*, Vol. 2, pp. 417–854, with numerous figures.] Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. 25*s*. net.)

THE International Congress on Analytical Chemistry held at Utrecht in June 1948 was attended by about 300 chemists and physicists from a number of countries. The proceedings lasted four days, thirteen formal lectures being delivered and forty-one original papers presented. The present volume is a report of the Congress and is a reprint of an issue of *Analytica Chimica Acta*, containing all the papers except those on emission spectrography, which will be published in *Spectrochimica Acta*.

The Congress reflects modern tendencies in analytical chemistry rather than its general condition. This is only to be expected in view of the advances and developments which have occurred during recent years. The backbone of analysis is still the classical gravimetric and volumetric determination, but extended information can be obtained and gaps filled by the employment of the newer techniques. Micro-methods have been developed

to permit well-known determinations to be scaled down a hundredfold, with economy in time and material. Organic reagents for inorganic analysis have extended the range of specific precipitations and separations. The electrical characteristics of ions and materials are utilised for their determination. The optical properties of excited elements and of molecules in solution are measurable and are proportional to the amounts present. Adsorption phenomena facilitate separation of mixtures otherwise often inseparable. Microbiological methods determine substances essential to the growth of bacteria by measuring the limited effect of sub-optimal amounts. The electron microscope has extended vision below that possible with the microscope. The mass spectrometer permits quantitative separation and measurement of ionised particles differing but slightly in mass, particularly useful with complex gaseous mixtures and for determining abundance ratios with mixtures of isotopes. Statistical methods are applied to the design of experiments to obtain the maximum information from the minimum necessary data. All these were dealt with at the Congress.

For the proper exploitation of the newer physical methods elaborate equipment is required. The disadvantage of expense is more than counterbalanced by the additional information obtained and by the saving of time where many tests are made. The necessary apparatus can be built into a self-contained instrument which can be operated as a machine—by one who knows how. This has created an increasing demand for the skilled laboratory technician and in large laboratories more team work will result. A possible danger is specialisation in too limited a field ; no scientist should rely solely on one tool.

The Congress shows that analytical chemistry is a healthy and progressive science which is expanding its activities in a variety of directions. The additional knowledge thus gained will contribute materially to the advance of both theoretical and practical chemistry.

J. R. N.

Advances in Food Research. Vol. I. Edited by E. M. MRAK and G. F. STEWART. [Pp. xvi + 459, with 45 figures.] (New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1948. 41*s*. 6*d*. net.)

TEN very different lines of investigation are authoritatively surveyed in this book by one British and nine American authors. To comment adequately on each review in the space available is impossible ; a very brief reference to each must suffice.

Dealing first with food products from the animal kingdom, an admirable paper on the physiology and chemistry of rigor mortis, with special reference to the ageing of beef, is contributed by E. C. Bate-Smith. A great deal of work, widely scattered in the literature, concerning the factors which control the *p*H of meat and the dominant effect of *p*H on the subsequent behaviour of meat as a commercial commodity, is summarised. Change in *p*H has a profound effect on the properties of the meat and on the rate of growth of contaminating bacteria and therefore on such practical problems as storage and curing.

In a survey by Belle Lowe of the factors which affect the palatability of poultry, the importance of the immediate post-mortem treatment is stressed,

and the influence of the treatment of the bird after slaughter (rate and degree of chilling, whether drawn or not, etc.) on the fat and on the flavour, juiciness and tenderness of the flesh is fully discussed. The earlier observations of Pennington on the histological appearance of the flesh have been followed up and the results of studies on ageing are well illustrated.

The directive influence of the war is well illustrated by three reviews of scientific work concerned with the dehydration of food materials. Considerable development took place in the commercial production of dried whole egg powder (so much more economical in shipping space than shell eggs) intended primarily for use as scrambled egg rather than in bakery products, and it is the effect of drying and storage conditions on the palatability (flavour and consistency) of the reconstituted scrambled egg which is discussed by H. D. Lightbody and H. L. Fevold. The bulk and high-water content of vegetables lead inevitably, in the special circumstances, to studies on their dehydration; the knowledge accumulated on the deterioration of processed potatoes, including their discoloration, is summarised by A. F. Ross. The brown discoloration of dried fruits was an aspect of the technology of fruit products almost entirely neglected until similar discolorations were noted in dehydrated vegetables. E. R. Stadtman outlines the three most generally accepted theories put forward to explain this phenomenon (the Maillard reaction, oxidation of ascorbic acid, and the "active aldehyde" theory) and includes a section which stresses the need for further work and suggests lines of future investigations.

The contribution by J. L. Baker on high polymer pectins and their de-esterification is mainly a summary of his own investigations with co-workers; it is to be regretted that the recent papers by Hirst *et al.* and by H. Deuel were not also reviewed. While de-esterified pectin is undoubtedly of importance in food technology, it is an open question whether or not it warrants so much space to the exclusion of other recent advances in this field.

The influence of climatic and fertiliser practices on the vitamin and mineral contents of vegetables is considered by G. F. Somers and K. C. Beeson, who stress the rather haphazard nature and faulty design of many of the experiments reported in the literature. In general, however, the conclusions of earlier reviews are confirmed, namely that light is one of the most important factors influencing the vitamin C content of plant tissues and that the small differences which result from fertiliser treatment are likely to be obscured by differences due to variation in climate and variety. In 1942 the National Canners Association and Can Manufacturers Institute initiated a large-scale study of the vitamin content of canned goods and the effect of the different canning operations on the vitamins present in the raw material. The data obtained from the examination of 1309 samples of 43 major commercial lines of canned fruit, vegetables and fish products are skilfully presented by L. E. Clifcorn, and include a section on the effect of storage for periods up to twelve months.

Orville Wyss reviews the ways in which inhibiting substances can affect the growth or the metabolic processes of micro-organisms—such as interference with the genetic mechanism, the cell membrane or the activity of enzymes. It is stressed that the use of preservatives in food should be sanctioned only after extensive pharmacological tests, and the forecast is made that current research on antibiotics and competitive inhibitors may result in the sanctioning of new permissible preservatives.

The physiological basis of voluntary food intake is discussed by S. Lep-

kovsky, who assembles considerable evidence that dietary habit is a response to physiological need.

The book is a worth-while publication presenting clearly present knowledge of the subjects reviewed therein. It is well set up and pleasingly free from misprints.

From the staff of the Laboratories of J. LYONS & CO., LTD.

Trace Elements in Food. By G. W. MONIER-WILLIAMS, O.B.E., M.A., Ph.D., F.R.I.C. [Pp. viii + 511.] (London: Chapman & Hall, Ltd., 1949. 30*s*. net.)

THIS is a novel and most interesting book. What are trace elements? Dr. Monier-Williams defines them as those which may be present in food in amounts up to 0·005 per cent. This definition embraces quite a surprising number, about 35 in all, and includes iron. Of these, five or six out of the twenty which are common in foods are essential to nutrition and a few are highly toxic. Probably no one in this country has made quite so thorough a study of the subject as the author and none could write on it with more authority. We are accustomed to expect an accurate and balanced report from Dr. Monier-Williams, and he certainly lives up to his reputation here. The book is a mine of information on the subject; it is full of interest from cover to cover and it may be quite surprising to those who have not considered the subject to find how much is known about the properties or effects of almost every element. The physiological effects, too, of various substances are quite remarkable; we are familiar enough with lead or arsenic, but now we have to consider thirty other elements; some are beneficial, even essential, others cause a variety of dietetic ills to man or beast. Yet others cause defects in food such as haze in wine, discoloration in fruit juices, the blackening of shell fish, or promote the oxidation of vitamins. All these matters and many more are carefully considered. Each element in its turn is discussed; all that is known is succinctly surveyed with full references to original sources, effects physiological and industrial are noted, the quantities liable to occur are shown and details are given of methods for their detection and estimation in food or in the body. This latter is often a different and more difficult problem than the mere detection of an element amongst others.

All who have any interest in the subject, and that must be all concerned with food processing or with dietetics, will wish to read and ponder this book; it is excellent, and we are very much indebted to Dr. Monier-Williams for the great amount of time and study he has put into it and for giving us the benefit of his unique experience in this field. It may be added that the production is good, printing clear, index good, and the arrangement of the references is such as not to interrupt or mar smooth reading.

H. E. COX.

X

Methods of Quantitative Micro-Analysis. Collected and edited by R. F. MILTON, B.Sc., F.R.I.C., and W. A. WATERS, M.A., Sc.D., Ph.D., F.R.I.C. [Pp. viii + 599, with 169 figures.] (London: Edward Arnold & Co., 1949. 60*s*. net.)

THIS is a particularly useful and authoritative textbook because it provides a comprehensive survey of modern micro-technique contributed by practising analysts who are specially well versed in the peculiar difficulties of the

subject and in the laboratory arts. To give some idea of the scope it is convenient to outline the six parts: Part I, contributed by the Editors, is on general micro-technique and gravimetric analysis; Part II, on the analysis of organic compounds, is mainly by G. Ingram and W. A. Waters; Part III is on volumetric methods by the Editors with K. M. Wilson; Part IV is on colorimetry by R. F. Milton; Part V, by J. T. Stock, is on electrochemical processes and includes polarography as well as electrometric deposition and titrations; Part VI, on gasometric methods by K. M. Wilson, includes the manometric method of Van Slyke, the Warburg technique and Barcroft differential method. It will be appreciated from this brief sketch of the contents how wide is the range of subjects covered. Besides pure chemical estimations, quite a surprising range of technical determinations are described with meticulous care, *e.g.* blood sugar, the analysis of steel, water analysis, D.D.T., cholesterol in blood, benzene vapour in air, olefines, organo-metallic compounds, sulphonamides, vitamins, urine, active hydrogen, *p*H values, and moisture by the Karl Fischer reagent. All these and much more are set out very clearly with all necessary detail and illustration.

And lest it be thought that the book and its methods are only of value to those who possess a pucka micro-balance, let it be pointed out that very many methods are applicable to macro quantities and others to quantities such as may be dealt with on any good analytical balance. There are also processes, *e.g.* for the determination of trace elements in food, which are based on larger quantities of material.

The production of the volume is excellent, though we think the much-divided index is inconvenient; all necessary data such as reagents, tables, factors and so on are included and set out with care and lucidity. Altogether it is a valuable, comprehensive work, well worth while to all analysts and to all those who have from time to time to do analyses. The latter class will find it particularly useful, as they are by definition less experienced or well equipped. The price seems rather high, but it is for a very substantial volume with many tables, diagrams and figures.

H. E. Cox.

Techniques of Histo- and Cyto-Chemistry. By David Glick, Ph.D. [Pp. xxiv + 531, with 159 figures and 10 tables.] (New York and London: Interscience Publishers, Inc., 1949. 48*s*. net.)

In the study of the complex reactions which occur in the living cell, a very specialised analytical technique has been elaborated. This technique is drawn from all aspects and experience of work in other fields of analytical investigation, but it is essentially carried out on the micro scale. The modern biologist is concerned with the elucidation of the elusive and mysterious chemical pattern which is the basis of life, and any methods which assist in this search are, therefore, his special field. Previously, micro-chemistry has meant to the biologist application of appropriate reagents to cells and tissues to assist in microscopic identification. The new micro-chemistry of the cytologist involves isolation and purification of compounds, on a sub-microscopic and ultra-microscopic scale, by the application of all known physical and chemical methods. The authors of this book have made an extremely searching investigation into the possibilities of the application of such methods to their study of the fundamental chemical pattern of protoplasm. They have not, however, forgotten that in the final analysis

the most sensitive instrument for indication of cellular changes is the living organism itself, and they point out that the methods of immunology allow of distinction between compounds so closely related that the chemist is unable to distinguish between them. In the historical development of these technical methods, bio-assay has played an important part, and such technique is a fitting example of the utilisation of the exquisite sensitivity of the living organism to minute changes in its chemical environment. This volume, however, is to a very large extent concerned with more direct methods of analysis, although there is a small chapter at the end which deals with Vitamin Assays by Biological Methods.

The first part of the book is concerned with microscopic techniques. Firstly, the freeze-drying process for the preparation of tissues is described, with illustrations of the type of apparatus required. There follows an extremely detailed section on chemical methods applied for identification of cellular constituents in conjunction with the use of the microscope. This section is sub-divided into (*a*) inorganic elements and radicals, (*b*) organic substances and groups, and (*c*) enzymes. Under these three groups practically every known substance which plays a part in cellular reaction and vital processes is included. The individual methods are given extremely fully, with explanatory notes on the reactions involved and the specific nature of the reaction.

The second part of the book concerns physical methods. Under this heading are described fluorescence microscopy, emission histo-spectroscopy, and visible and ultra-violet absorption spectroscopy, applied to the cellular examination. Apparatus and methods are described in detail, with notes on the usefulness of characterisation of substances by direct fluorescence observation. Thus Vitamin B Riboflavin, etc., in the cell may be quantitatively studied by the fluorescence technique, and estimation of phosphorus in quantities as low as 0·5 milli-micrograms may be deduced by the Norburg absorption technique. Roentgen absorption spectroscopy is described, including the full apparatus required for application to histo-chemical analysis. This section continues with a short account of analytical electron microscopy and ends with a brief account of radio-autography, which should be of value to readers requiring a superficial understanding of the subject.

The bulk of the book is concerned with micro-chemical analysis on material after separation from the cell. Work of this kind requires specialised types of apparatus for reaction vessels, centrifuge vessels, Kjeldahl tubes, etc., and these, as well as pipettes and burettes to deal with micro-litre quantities, are beautifully illustrated. Stirring devices, ultra filters, heating devices, electrodes, etc., find their place, and the specialised type of balances, including the Quartz torsion balance, are brought to the reader's notice. Colorimetric technique on a capillary tube scale is developed for a large number of biological substances, and this beautifully illustrates the specialised application of micro-chemistry to cytology, previously mentioned. Cuvette colorimetry forms a well-written section, wherein methods for most of the common biological substances are described, using the Beckmann spectro-photometer for colour measurement.

Micro-titrimetric techniques follow. Again, micro-litre burettes and pipettes and the apparatus for this work are dealt with, and a comprehensive review is given of the literature relating to developments in this field. The micrometer screw burette is a great favourite, and elegant apparatus is suggested to avoid CO_2 interference during micro-titration. Methods are

given for sodium, potassium, calcium, iron, phosphorus, chlorine, nitrogen, ammonium, sugar, fats, ascorbic acid, enzymes, etc., mostly developed by Lindeström-Lang and his colleagues. These methods are precise, yet very detailed and appealing. There is a very full section on gasometric methods. This includes capillary respirometry, membrane interferometer volumetry, micro-litre/micro-diver technique, optical diver technique, polarographic methods using micro-electrodes to estimate local oxygen tension in tissues, and then dilatometric technique. This section is very clearly illustrated, full details being given of the apparatus involved.

There is a further section on the determination of the amount of a biological sample, and this includes cell-counting technique and volume measurement in a capillary tube with microscope assistance.

The book is extremely well written, and closes with a very comprehensive bibliography. It is almost encyclopædic in its scope, and, although doubtless invaluable to bio-chemists and histologists in the cytological field, has nevertheless much to offer to general workers in micro-analytical methods, as a reference book for apparatus and for techniques.

R. F. M.

Treatise on Powder Metallurgy. Vol. I. By CLAUS G. GOETZEL, Ph.D. [Pp. xxviii + 778, with 300 figures.] (New York and London: Interscience Publishers, Inc., 1949. 90*s*. net.)

THE present volume is the first of a series of three which are designed to cover the entire existing literature on powder metallurgy. Two short introductory chapters deal with the general principles of powder metallurgy and its historical development. The following chapter on methods of production of metal powders represents an adequate survey of the literature, but is not sufficiently informative on detail for really practical purposes. This section would undoubtedly have benefited from an inclusion of data contained in various B.I.O.S. Reports. However, the author then deals fully with the physical characteristics of metal powders, methods of testing, and the general commercial details and prices of currently available metal powders. The various operations in the preparation of powders for the market are considered rather briefly, but everyone who has to do with metal powders should be interested in knowing even a little of what is involved in washing and drying, sieving and blending, packing, shipment and storage, mixing with non-metallic substances, etc.

The next six chapters are devoted to a discussion of pressing and hot-pressing techniques. First, the theoretical aspects of interparticle bonding are considered, particular attention being paid to the effects of impurities, the work of Balshin, and the more recent work of Unckel and of Wulff and his colleagues on the distribution of pressure in a die. The author attempts to prove experimentally that melting does not occur at points of contact between particles, but it would have been interesting if he had taken into account also Bowden's work at Cambridge on intermetallic friction. The discussion of die design and die materials is eminently well written and should be of considerable assistance to potential manufacturers of powder metallurgy parts. The same may be said of the two chapters on hot-pressing to which the author himself has made notable contributions.

The remainder of the book deals with the theory of sintering, sintering methods, a description of the different types of sintering furnace, sintering

atmospheres, coining, heat treatments, and finishing operations, such as coating, impregnating and joining.

The book is excellently produced, with a large number of photographs and diagrams, and may be recommended as a standard work of reference for both the beginner and the expert in powder metallurgy.

W. D. J.

A Dictionary of Metallography. By R. T. Rolfe, F.R.I.C., F.I.M. Second edition. [Pp. xii + 287.] (London: Chapman & Hall, Ltd., 1949. 18*s.* net.)

The compilation of a good technical dictionary is always a formidable task; when, as in the present case, the subject matter ranges from industrial practice to pure science and some of the terms are used differently by different people, the difficulties become excessive. One way of overcoming them is by organising a panel of specialists, but this is an expensive procedure with special difficulties of its own. Mr. Rolfe has preferred to take on the difficulties of compilation by himself and, within the limitations of a one-man work, has succeeded remarkably well. In this second edition of his book, 152 new terms have been added, making a total of 1350, and the definitions of some 250 others have been revised. The introduction of long sections on nuclear physics and atomic energy, while these are useful in themselves, does not help to remove the criticism levelled at the earlier edition of this book (Science Progress, **34**, 827, 1946) of want of balance, since, for example, constitution is still dealt with in seven words. However, the book is very useful and fully deserves to be given a reception as cordial as that of the first edition. It fulfils a definite need in the literature of metallography and is likely to continue to do so until Mr. Rolfe can be persuaded to assemble a panel of experts to construct an exhaustive work of reference.

A. H. Cottrell.

GEOLOGY

Structural History of the East Indies. By J. H. F. Umbgrove, D.Sc. [Pp. xii + 63, with 10 plates and 68 figures.] (Cambridge: at the University Press, 1949. 30*s.* net.)

This book contains the text of a series of lectures delivered at Cambridge in 1946 by the Professor of Geology at Delft; dealing with the evolution of the festoon of islands strung round the equator from Sumatra to New Guinea, it forms a short conspectus of the researches by Dutch geologists and geophysicists into the structural geology of the East Indies. It is not a mere catalogue of their more important results, but a connected narrative into which a profusion of facts is skilfully compressed and impressively presented.

The first two chapters are devoted to the shallow and deep seas, respectively. The various shelves with their drowned rivers and barrier reefs, the coral cays and the influence of eustatic movements are concisely treated. Brief descriptions follow of the deep basins and troughs, their abyssal waters, bottom deposits, shapes, distribution and time of origin.

The third chapter deals with the volcanoes which crowd many of the islands and contains synoptical accounts of their different types, of volcano-tectonic structures, of the relation between volcanism, plutonism and tectonics and of the epochs of increased activity.

The treatment of the structural zones in the fourth chapter is restricted, though adequate enough for the author's purpose. The palæogeographical maps help the reader to disentangle their complexities, particularly as regards the Miocene and Pliocene movements and their varied intensity from one group of islands to another.

Geophysical results occupy the fifth chapter. The association of some normal tectonic earthquakes with well-established fault zones, and of the deep-seated foci with deep-reaching shear zones, is described. The relations between a remarkable belt of negative anomalies of isostasy on the one hand, and of the outcrop of the seismic plane, the topography and the zone of intensive Miocene folding on the other, are clearly demonstrated.

A summary of Dr. Umbgrove's views on the origin of the double island arc and its associated phenomena, together with a statement of problems yet unsolved, completes the sixth and final chapter. Admirably illustrated with maps, diagrams and plates, this work is an explicit and authoritative introduction to the intricate geodynamical problems of Indonesia.

J. Coggin Brown.

Minerals and Mineral Deposits. By W. R. Jones, D.Sc., D.I.C., Pres. I.M.M., F.G.S. and David Williams, Ph.D., M.Sc., B.Eng., M.I.M.M., Sec.G.S. The Home University Library of Modern Knowledge, No. 202. [Pp. viii + 248, with 56 figures.] (London: Oxford University Press, 1948. *5s.* net.)

With the coming of the atomic bomb and the consequent search for ores of uranium, the subject of minerals has achieved the importance it deserves in the public mind. This little book is calculated to give the intelligent citizen the fundamentals of the subject, as well as to interest him in its social and political aspects. Three introductory chapters deal with generalities, minerals in world history before the industrial age, and the composition of the earth's crust. Three chapters are devoted to mineralogical science, treating respectively of the shapes, internal structures, and physical characters of minerals. Amongst these, Chap. V, on internal structures based on X-ray analysis, is a highly successful attempt to present this not-too-easy subject in a popular way.

The following chapters are also scientific in their implications: Chaps. VII, Minerals and Rocks; VIII, Economic Mineral Deposits, a good succinct account of the geology and origins of valuable minerals; IX, Distribution and Production of some of the chief Metalliferous Mineral Deposits; X, The Search for Mineral Deposits, a most interesting and readable chapter; XI, The Extraction of Minerals and Metals, dealing with mining, milling, smelting, etc.

The two final chapters treat of minerals in their industrial, social, and political aspects: Chap. XII, Minerals in the Industrial Age; Chap. XIII, Some International Aspects of Mineral Resources. Minerals are exhaustible assets, subject to depletion at increasing rates as the demands of national units increase. The table on p. 212 of this book, showing the degree of national self-sufficiency in the key minerals of the principal political units (in 1938), is highly interesting in this respect. By rating degrees of national self-sufficiency as 3 (surplus supply), 2 (supply adequate), 1 (supply inadequate), 0 (supply negligible), the reviewer finds that if 100 represents complete self-sufficiency in minerals, the British Commonwealth and Empire is 68 per cent. self-sufficient, the Soviet Union 59 per cent., and the United States 47 per cent., which is reassuring if the method is regarded as valid.

This little work is extremely well done, and, while it is aimed at the citizen and general reader, it is also of value to the student of the subject. It is written in a readable style, and is illustrated as well as the series in which it appears permits. It has an adequate index, and a glossary of the minerals named in the text is provided.

G. W. T.

BOTANY AND AGRICULTURE

Lectures on the Inorganic Nutrition of Plants. By D. R. HOAGLAND. Second edition. [Pp. xii + 226, with frontispiece, 28 plates and 44 figures.] (Waltham, Mass.: The Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1948. 27*s*. net.)

THIS book, first published in 1944 and now reprinted with only very slight revision, is based on the Prather lectures given by Prof. Hoagland at Harvard University. It is not a monograph, but rather it presents Prof. Hoagland's views on several aspects of plant nutrition with which he has been associated for many years.

The seven lectures deal with rather diversified aspects of the subject, but the author shows clearly how, in his school, several diverging lines of investigation into plant nutrition arose initially from researches into the interrelation of the soil, plant and atmosphere. One of the ultimate aims of these researches is, in his own words: " What we should most like to learn about the inorganic nutrients is how, directly or indirectly, they enter into the synthesis and utilisation of organic compounds." So far the plant physiologist has made very scanty progress towards this goal.

In the first lecture it is shown how studies of the seasonal and secular trend of changes in composition of the soil solution led to researches on soil colloids, soil acidity and so on to study of plants in artificial cultures, including trace element studies. These are considered in more detail in the second lecture. The broad problems are stated and particular attention is given to zinc as an essential element. The third lecture deals with absorption and accumulation of salts by plant cells—a subject in which Prof. Hoagland has a special interest. In the next lecture the upward movement and distribution of inorganic solutes in the plant is considered. A good deal of attention is given to the relation of water absorption to salt absorption and it is disappointing that the more general aspects of solute movement are ignored.

The fifth lecture deals with the growth of plants in artificial media in relation to the study of plant nutrition and includes a brief account of the contact theory of Jenny and Overstreet.

In the next chapter the biochemical problems associated with salt absorption are discussed. Here the phenomenon of salt respiration is mentioned, but not critically discussed in the light of Lundegårdh's hypothesis. The concluding chapter deals in a general way with potassium nutrition and serves to emphasise that our knowledge of the functions of this element in the plant is still largely obscure.

These lectures present a summary of the work on plant nutrition by the Californian school but can by no means claim to be a critical résumé of the subject of plant nutrition. It is a pity that some of the text figures are so reduced as to be almost illegible, a fault which might have been corrected in a reprinted edition.

E. C. HUMPHRIES.

Factors in Botanical Publication and Other Essays. By NEIL E. STEVENS, Ph.D. *Chronica Botanica*, Vol. II, No. 3. [Pp. 119–206, with frontispiece.] (Waltham, Mass.: The Chronica Botanica Co.; London: Wm. Dawson & Sons, Ltd., 1947. 12*s*. net.)

THIS number of *Chronica Botanica* exemplifies a recent change in the journal by which it contains, in the words of the publisher, "more matter than previously dealing with the basic humaniora of the plant sciences: history, methodology and philosophy". The number consists of 17 articles by the well-known plant pathologist, Prof. Neil E. Stevens, now of the University of Illinois. All are very brief (2–7 pages) and all but one are reprints of writings of the author between 1920 and 1946. Among the less-technical articles one finds: "Radicalism and Research in America," "The Botany of the New England Poets," "Botanical Figures in Biblical Prophecy," "The Excessive Meekness of American Botanists," "Brevity at Botanical Banquets," "Applied Botany as Fun," "The Anecdote as an Antidote to Statistical Analyses" (unpublished) and "The Fad as a Factor in Botanical Publication." The last title, in truncated form, has been used—though not very happily—as a general title for the collection of essays. One of the more technical articles, "How Plant Breeding Programs complicate Plant Disease Problems," indicates the rapid extension, even before the war, of the acreage under hybrid maize in U.S.A. For the area under this crop rose from half a million acres in 1935 to five million acres in 1939. The article also emphasises the increased risk of disease which is entailed by the planting of such a crop as this with its lessened capacity for variation. These "Stevensiana," as a sub-title terms them, show the author as a man of wide interests, with an easy literary style and an independence of mind which refuses to be swayed by scientific fashion. Many of the articles are of more than ephemeral interest, but it seems doubtful if, taken as a whole, they can be considered as of sufficient weight to justify republication in this form.

V. H. BLACKMAN.

Science in Agriculture. A Discussion of Scientific Principles in Relation to Farm Practice. By JOHN W. PATERSON, B.Sc., Ph.D. Fourth edition. [Pp. viii + 303, with 181 figures.] (London: Longmans, Green & Co., 1949. 8*s*. 6*d*. net.)

THIS very successful presentation of so much of the basic science of agriculture within so short a space is, in itself, a notable and laudable achievement. The book, however, conveys very much more than this. Not only are the many and varied phenomena and operations of agriculture explained against a scientific background, but there emanates from the text much of the very atmosphere of the farm which is often difficult to capture from cold print. The large number of illustrations doubtless contributes heavily in this respect, and the clear, simply-worded and smooth-running narrative brings no sense of over-compression of the subject matter.

The book covers the formation, chemistry, physics and micro-biology of soils, the structure, physiology and nutrition of plants, cultural operations and farm machinery, practical aspects of cropping and harvesting, with many considerations of farm economy, pests and diseases of common crops, animal feeding stuffs and their utilisation by live-stock, the nature of wool and some vegetable fibres, and the composition and quality of milk and dairy products.

The fourth edition contains an additional chapter on weeds and their control, and to the expositions of fundamental scientific principles, which are freely dispersed throughout the book, is added a further chapter on appropriate aspects of mechanics.

To those concerned with teaching in agricultural schools this little volume offers a well-planned basis of instruction in which scientific explanation and practical methods are happily and effectively blended. Teachers of general science should also find the book extremely valuable as presenting many points of chemistry, physics and biology with an interesting and vital objective.

A. G. Pollard.

Soil Physics. By L. D. Baver. Second edition. [Pp. xiv + 398, with 89 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 28*s*. 6*d*. net.)

This is a revised version (28 pages longer, but less bulky) of a book which first came out in 1940, and rapidly made itself well known among agricultural workers and research students. The topics with which it deals include methods of mechanical analysis; characteristics of soil colloids (with an accurate brief report of the results of X-ray diffraction and electron microscopy); colloid-chemical properties of soils; soil structure (including, it is good to note, a reference, though perhaps too brief, to the role of bacteria and fungi); soil water (a chapter which might perhaps usefully be expanded to include a discussion of the problem of water-flow in land drainage); soil air and temperature; tilth and tillage problems, including ploughing; and runoff and erosion.

The point of view, it will be seen, is definitely agricultural, and "soil mechanics" problems are not directly dealt with. This book must be considered an essential work of reference, if only because it is the only thoroughly up-to-date work of its type. It has, however, sound merits which would recommend it in any event. The literature references are particularly numerous, and of universal value, covering as they do work from British, French, German, Russian and other sources, as well as American. The treatment is, in general, thorough and well balanced: and the author's numerous original contributions to the subject are not given undue prominence. The style of the book is perhaps rather that of a progress report, or monograph, than of a textbook, with many descriptions of published work, and relatively infrequent intrusions of the author's own opinions. Such treatment is perhaps partly imposed by the nature of the subject, where little is of definite and unchallenged validity, but a more thorough digestion and re-forming of the material might in some cases have been an advantage. The author could usefully however have followed one convention of the "progress report" type of publication—that of referring to his own publications by name, instead of using the passive voice without qualification ("It has been found," etc). The effect is confusing at times, and has had the further unfortunate result that most of the descriptions of the author's own work have failed to get into the index under his name.

D. M. C. M.

ZOOLOGY

Textbook of General Zoology. By W. C. CURTIS and M. J. GUTHRIE. Fourth edition. [Pp. xx + 794, with 524 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 27*s*. net.)

THIS is a popular American textbook, now ten years old and in its fourth edition. Space is allocated as follows: vertebrate metabolism and co-ordination, 87 pages; histology, 33 pages; reproduction, development and heredity, 88 pages; survey of the invertebrate phyla, 350 pages; survey of the chordates, 60 pages; evolution, 80 pages.

It is written with complete competence and would be a worth-while addition to any school library. There are plenty of illustrations, line and photographic; many of them are derived from recent research publications or are useful for other reasons; others, such as the photograph of Mr. Mikimoto, the pearl king, or that of timber wolves on the trail, are of less obvious value. The authors have made many changes to bring the new edition up to date. It is a pity that they have not removed such antiquities as "A composite animal cell in the vegetative phase" (Fig. 51) and such unlifelike horrors as "The cœlomic viscera of the frog in ventral view" (Fig. 4).

W. H.

Giant Fishes, Whales and Dolphins. By the late J. R. NORMAN, F.L.S., and F. C. FRASER, D.Sc., F.L.S. Illustrated by W. P. C. TENISON, D.S.O., F.L.S. Second edition. [Pp. xxii + 376, with 8 coloured plates, 97 figs., and numerous other illustrations.] (London: Putnam & Co., Ltd., 1949. 15*s*. net.)

THIS charming book has changed little from its first edition, but it is a delight to see it again in print, and particularly since it has shrunk to a handier size, small enough to go into a reasonable coat-pocket. As well as being scientific and systematic it is full of fishy lore; the authors give the reader the delight of strange anecdotes while protecting their own reputation by the abundant use of quotation marks. Thus they tell us that *Belone belone* has bright green bones, and that it is said that "the Barracuda more often attacks an Englishman than a Frenchman when both are together in the water."

There are keys for the identification of the principal representatives of the groups described, and Lieut.-Col. Tenison's useful and amusing illustrations are both essential and ornamental to the work.

W. H.

Trout Fisheries in New Zealand: Their Development and Management. By DERISLEY F. HOBBS. [Pp. viii + 175, with 31 plates.] (Wellington, N.Z.: New Zealand Marine Department, Fisheries Bulletin No. 9, 1948.)

FROM 1867 onwards many attempts have been made to introduce European and North American fishes into the rivers and lakes of New Zealand. The result is that the brown and rainbow trout fisheries are now world-famous, quinnat salmon (*Oncorhynchus*) are established in a few rivers and Atlantic salmon in one.

In this publication Hobbs subjects the present management of the trout fisheries to a searching examination, and concludes that:

(*a*) Generally speaking the trout fisheries of New Zealand are as productive, in both numbers and size of fish, as they were fifty years ago. The popular view to the contrary arises from the greatly increased number of anglers, so that the fish available are now shared among more people.

(*b*) The practice of catching ripe fish, spawning them artificially, incubating the eggs in hatcheries and liberating the progeny "when they are old enough to look after themselves" produces, except in a few special circumstances, no measurable increase in the stock of trout.

(*c*) In New Zealand waters generally, the supply of natural food is ample for the stocks of trout present.

(*d*) Long-finned eels (*Anguilla dieffenbachii*) exceeding 30 inches in length are predatory on trout. Below that size, they compete for the same food. In the South Island, the black shag (*Phalacrocorax carbo*) is also a dangerous predator.

In the light of these conclusions he recommends a re-orientation of fishery management in New Zealand away from artificial propagation and towards: (1) adequate protection of spawning fish; (2) prevention of loss of fish in drought-stricken areas; (3) so regulating lawful exploitation that sufficient fish survive to make the maximum efficient use of spawning grounds.

Although the main purpose of this book is to supply a biological basis for fishery administration in New Zealand, it so far exceeds its object that it should be read by fishery workers, scientific or administrative, everywhere. It is an admirable example of critical reasoning based on sound biology.

F. T. K. PENTELOW.

The Ocean. By F. D. OMMANNEY, Ph.D., A.R.C.S., F.L.S. The Home University Library of Modern Knowledge, No. 203. (Pp. x + 238, with 12 figures and 4 plates.) (London: Oxford University Press, 1949. 5*s*. net.)

SINCE the pioneering days of the *Challenger* great strides have been made in oceanographical research—advances made possible in large measure by the ever-increasing number and greater efficiency of mechanical appliances brought into use and by improvement in the physical and chemical techniques necessary for the study of seawater as an environment for the plants and animals that live in it.

In *The Ocean* an attempt has been made to compress a comprehensive and up-to-date survey of existing knowledge of marine biology and oceanography into a single convenient and reasonably-priced volume. Few there are who would attempt such a well-nigh impossible task and fewer still could make a first-class job of it. Even Dr. Ommanney—the well-known author of *South Latitude*—has not quite succeeded. The ten chapters of the book are very well written and packed with information skilfully presented. But far too many errors and inaccuracies are scattered throughout its pages. The tube of the lugworm, *Arenicola marina*, is *not* U-shaped; the tentacles of terebellid worms are *not* feathery; the winkle *Litterina neritoides* is *not* viviparous; haddock do *not* normally range as far south as the Bay of Biscay and there give place to the hake (which, incidentally is *not* the largest of the cod family); one does *not* expect to catch whiting in a trawl "anywhere

from the Mediterranean to Bear Island" which is beyond the northern limit of their range—and so on.

If a second edition is called for—as no doubt it will be—a really first-class volume *can* be produced by elimination of the errors and inaccuracies, improved format, better print and perhaps more and better illustrations.

G. A. STEVEN.

Birds in Britain. By FRANCES PITT. [Pp. viii + 576, with 17 coloured plates and 350 illustrations in the text.] (London: Macmillan & Co., Ltd., 1948. 25s. net.)

THE author admits that she wrote this book because she wanted to, and to those who know Miss Pitt's enthusiasm for birds and beasts this is itself a recommendation. Though "books on birds are legion" (*vide* preface), this one contrives to fill a niche, for it contains much information for the novice which otherwise he could cull only by reference to many books. It does not pretend to replace such reference books as the *Handbook of British Birds* or Coward's classic *Birds of the British Isles*; it is intended rather to be used in conjunction with them.

The first part outlines the structure of birds, behaviour and environment; the second part is a detailed study of individual species. The first part may be of value to the beginner (for whom it is presumably written), as it contains much to arouse his interest, but it would have been even more valuable had it contained a bibliography to guide him to further and more intensive reading; for the experienced naturalist it is too superficial. It is, however, more than a reference book, for frequently the author's own experiences peep out, enhancing its value.

In Part II, though the sequence in which species are described follows roughly the accepted order of the *Handbook of British Birds*, the author departs from it in a haphazard way for which no reason is evident. The description of domestic birds and pets is too scanty to have any value; it should have been amplified to form a separate book which would appeal to a different public. Is it consistent to adopt the modern practice of dropping capital letters for the English names while retaining them for the generic names and titles of photographs?

Miss Austen's frontispiece is delightful, and the coloured plates by Roland Green are typical of his crisp work. The numerous photographs are as excellent a collection as I have seen in such a book, depicting some of the finest work of our leading bird photographers. Having seen many of the originals I regret that some are poorly reproduced.

Despite these faults, the book may be recommended to bird lovers who wish to share Miss Pitt's enthusiasm, and to those who cater for the beginner, such as school and public libraries.

J. E. ROBERTS.

A Textbook of Entomology. By HERBERT H. ROSS. [Pp. x + 532, with 434 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. 36s. net.)

THIS is among the best, and is certainly the most lavishly produced (and, unfortunately, the most expensive), of the recent introductory textbooks of entomology. In presenting the essentials of the subject the conventional arrangement has been retained. Thus there are chapters on the Arthropoda,

on internal and external morphology, on the life cycle and on systematics. The latter occupies nearly half the book; the general standard may perhaps be conveyed by the statement that there are fairly elaborate keys for all the principal Orders, the Hemiptera, for example, being keyed for some 38 families.

These horizons have been considerably widened by including additional chapters on such topics as the geological history of insects, ecology, and a lengthy one on insect physiology. These parts are all extremely readable and are well worth inclusion, although some repetition might perhaps have been avoided if the chapters on morphology and physiology had been fused.

The profuse illustrations are a special feature of the book. Dr. Ross, who is Systematic Entomologist to the Illinois State Natural History Survey, has included many of the beautiful insect portraits both from the Survey itself and from the publications of the U.S. Department of Agriculture. In a few instances diagrams might with advantage have been substituted for original published drawings; for example, Figs. 163–5, portraying the embryonic development of a stone fly, are far too complex to be adequately explained in the text available.

In spite of many excellencies, however, it is to be feared that the uses of this book for teaching in this country will be severely curtailed by the very pronounced American orientation of the subject-matter. The opening chapter on the rise of North American entomology may, for instance, prove less stimulating to the beginner on this side of the Atlantic. And the insects chosen for illustrating the principles of classification will be as unfamiliar as the names of many pioneer American entomologists. The same quality of remoteness also affects the interesting concluding chapter on economic entomology, which is concerned only with the control of North American pests.

Finally, a few minor points which have taken the eye. The term "tropism" in the discussion of insect behaviour would have been better avoided. Trophallaxis appears as trophyllaxis. The reviewer has also a small personal prejudice against the customary American use of the word "sheeptick" as a popular name for the sheep ked, *Melophagus*.

A. D. L.

PHYSIOLOGY, BIOCHEMISTRY AND BACTERIOLOGY

The Physiology of the Eye. By Hugh Davson, D.Sc. [Pp. xii + 451, with 25 figures.] (London: J. & A. Churchill, Ltd., 1949. 32*s*. net.)

We all of us have preconceived ideas as to the constitution of an ideal textbook suitable for the use of students. These ideas would probably be found to differ widely but it seems likely that we should all agree on the importance of the following points: clearness and lucidity on the part of the author, and good printing, clear diagrams and strong binding from the publishers. If we applied these principles to the book under review we should find that it complied with our requirements to an outstanding degree. Because it is admirably suited for students' use, not so much for the very advanced student engaged in research who is going to make vision his life study as for the young medical student who is specially interested in ophthalmology. As Dr. Davson writes in his preface, "The research worker and advanced student in the physiology of the eye have been overwhelmed with monographs and textbooks." . . . "The medical student, optician and prospective diplomatist in

ophthalmology have, in comparison, been less well treated, and the present book has been written with these in mind."

The contents of the book may be summarised as follows: Section 1, The aqueous humour and the intra-ocular pressure: the transparent structures: the cornea and the crystalline lens, etc. Section 2, The structure of the retina, photochemical aspects, visual acuity, adaptation, theory of colour vision, etc. Section 3, Extra-ocular muscles and their actions, binocular movements, nervous mechanisms of control, the pupil and accommodation, etc. Section 4, The visual pathway and the projection of the retina, monocular and stereoscopic depth perception, retinal rivalry and the perception of motion, etc. Section 5, The refraction of the eye, the errors of refraction, retinoscopy, and the optical constants of the eye, etc.

This summary will give the reader some idea of the scope of the book. I can strongly recommend it.

H. Hartridge.

Organic Form and Related Biological Problems. By Samuel J. Holmes. [Pp. viii + 169.] (U.S.A.: University of California Press; Great Britain and Ireland: Cambridge University Press, 1948. 27*s*. 6*d*. net.)

This thoughtful small book from the veteran American biologist, Dr. Samuel J. Holmes, can be highly recommended to all who are interested in the principles which underlie the determination and maintenance of form in living things. It is something more than the type of essay with which we are all too familiar, for the author shows no inclination to select any one facet of his immense problem, but courageously stands up to nearly every problem of growth which can be posed. A rather long chapter on the biological problems of cancer, a concise account of the regeneration of blood and its bearing on morphogenic theory, discussions on autocatalysis and the origin of life, genes versus gradients, the competition idea as a factor in integration of the organism, show the scope of Dr. Holmes's programme.

These interesting topics are knit together by a central theme, the importance of continuous integration in the organism. Our bodies are mechanisms so constituted that they respond to stimuli by checking excesses and speeding up deficiencies in the performance of their several functions. A whole series of mechanisms is brought into play, not only for the regulation of many diverse functions, but also for the adjustment of structure, whereby harmony between it and function is preserved. Amongst the interadjustment devices are hormones, vitamins, mechanical stresses and strains, nervous impulses, products of excretion and even simple compounds such as carbon dioxide and the H ion. Some are products of the organism itself, for cells may give out substances which influence the metabolism of contiguous cells, whilst others come from the interplay of environment and organism. Some such theory applies also to development of the organism, and a number of developmental potencies must be assumed in the germ cell, which are elicited under the influence of various stimuli. When released they secure an automatic balancing of morphogenic and physiological activities at the different stages of development because they are mutually dependent. The fact that certain regions of the developing organism seem to be favoured at the expense of others indicates that functional hypertrophy is an all-pervasive process, appearing at all stages of ontogeny from the growth of the unfertilised

ovum to the final integrative effort to sustain life. This results in a structural complexity of the organism, and indeed of the protoplasm, which can be referred, like many problems of cell differentiation, to the kind of genes which are aroused to their specific enzyme activity through the evocatory influence of co-ordinating parts. It is along such a plan that Dr. Holmes builds up a graceful and ingenious edifice which deserves first-hand study.

The book is remarkably free from misprints (Vishnu is wrongly spelt on p. 64), and there is a useful list of references and an index.

One small error on p. 72 needs correction. Cancer resulting from hot coals carried in a receptacle held close to the abdomen ("kangri" cancer) is not found in natives of Ceylon, but is confined to much colder climates such as Kashmir.

G. R. C.

Mathematical Biophysics. By N. RASHEVSKY. Revised edition. [Pp. xxiv + 669, with 193 figures.] (U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1948. 42*s.* net.)

IN 1938 the first edition of this book described the work of Prof. Rashevsky and his collaborators in building up a system of mathematical biology. Since that time a regular school has been formed, with its own journal, and the present volume aims at summarising progress in the field up to date.

Part I, the longest and most detailed section of the book, is concerned with individual living cells. The cell is thought of as a metabolising system contained in a permeable membrane, and acting under the forces set up by the diffusion of the products of metabolism. It is found that the assumed model cell becomes unstable and is liable to split into two when the radius exceeds a critical value which agrees closely with that found experimentally. The growth and deformation of the model also agree with those of actual cells.

Part II outlines a theory of nerve action and serves as a basis for Part III, which deals with the central nervous system. The mathematical models here consist of chains of neurons which activate or inhibit each other, and it is shown that chains can be constructed which will, in some sense, recognise universals, learn from experience, and reason in the various modes of formal logic.

Part IV considers a general approach to biology from the angle of mathematics, and deals very cursorily with the form of animals and plants, and the interaction of competing species.

It is a pity that Prof. Rashevsky's school does not include a number of experimental biologists to supplement the theoretical work here reported. Part I in particular contains enough suggestions to occupy a team of cytologists and biochemists for years, and their findings would provide new points of departure for the theoretical work. As it is, the book has an air of unreality, and it is sometimes hard to decide when Prof. Rashevsky is claiming to describe the actual mechanism of the living organism and when he is merely constructing an ingenious mathematical analogy. Perhaps its chief value is in showing that phenomena which have up to now been considered too complex for any but descriptive treatment will yield to a certain extent to comparatively straightforward mathematical analysis, and in stimulating both experimenters and theoreticians to more detailed study of familiar biological events.

M. J. R. HEALY.

Biological Reactions caused by Electric Currents and by X-rays. By J. TH. VAN DER WERFF, M.D., D.Sc. [Pp. xii + 203, with 38 figures.] (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. 30s. net.)

THE author first draws attention to a parallelism between electrical stimulation of nerve and the irradiation of tissues by X-rays. Both processes result in ionic effects; both are subject to a strength-duration law, which, for short durations and constant effect, is of the form *strength* × *duration* = *constant*; both show summation of subliminal stimuli and similar post stimulation excitability changes. He then postulates a general equation for metabolism, based on the law of conservation of mass in the velocity form, from which the equations of electrical stimulation and X-ray irradiation are derived as special cases. The equation for electrical stimulation explains qualitatively the local excitation phenomena of nerve in terms of synthesis and breakdown-time coefficients. The quantitative agreement is not satisfactory, but is improved by the replacement of the breakdown-time coefficient by its mean value and the assumption of a Poisson function for its distribution.

The theory developed here seems to be equivalent to the classical electrical theories of nerve excitation, *e.g.* Hodgkin and Rushton (1946) *Proc. Roy. Soc. B.*, **133**, 444, to which incidentally the author does not refer. The translation of the metabolic equation into electrical terms theoretically should transform it into the familiar cable equation form. The data to enable this to be done do not exist, however.

The discussion of the effect of X-rays shows that the general equation includes: metabolism and growth rate changes under the influence of X-rays; Schwarzschild's and Bunsen-Roscoe laws; the Hit-theory.

The author is well aware of the fundamental weakness of his thesis, that the time courses of the two phenomena are widely different, the one concerned with fractions of a second and the other with durations from minutes to weeks. He overcomes the difficulty by postulating a belief that the X-ray effects are always secondary reactions and that "the primary effects" on which they depend "are as quick as those with the excitation-phenomena." The price of theoretical uniformity is thus the acceptance of a postulate concerning the immediate reactions of tissues to X-ray exposure. Unfortunately the postulate cannot at present be confirmed by direct observation. This is not necessary, of course, for its acceptance, but is experimentally desirable. It is also implicit that the structure of the secondary reactions is the same as the structure of the primary effects.

The book is well produced but seems unnecessarily expensive. On pp. 37 to 85 the title of Chapter II is incorrectly given: it should be "The case $\tau_1 > \tau_2$."

W. F. FLOYD.

Industrial Hygiene and Toxicology. Vol. I. Edited by FRANK H. PATTY. [Pp. xxviii + 531, with numerous figures.] (New York and London: Interscience Publishers, Inc., 1948. 60s. net.)

THE first volume of this monumental work is devoted chiefly to the more general aspects of industrial hygiene and toxicology, leaving the specific toxic agents, with the exception of radiant energy and dust, to be dealt with in the second volume.

Some idea of the thoroughness and detail with which the various subjects are treated may be gained from the introductory remarks to the chapter, "The Mode and Entry of Toxic Materials," by Dr. Frank H. Patty: "In order to plan the prevention of injury from toxic materials in industry it is essential that we have a clear understanding of how these materials enter the body, how they act therein and how they are eliminated. In order better to understand these processes, we should understand respiration and circulation and their role in absorption and elimination. This in turn necessitates an understanding of the gas laws, with an ability to apply them to the solution of gases in liquids and specifically the body fluids." This high standard is carried out in practically every section of the book, and there can be scarcely any specialist in any branch of industry who will be unable to find not only the general basic information on every aspect of his speciality, but also the highly technical details usually confined to textbooks on physics, chemistry, engineering, physiology, and even industrial psychology.

The industrial hygienist, among whose special tasks is that of carrying out comprehensive surveys of the whole plant, with the co-operation of directors, executives, plant physician, engineers and foremen, has no exact counterpart in this country. It is pointed out that in England attention is directed more to the medical than to the technical or engineering aspects of industrial hygiene, while in America emphasis is laid more strongly on the need for a common understanding between the engineer and the industrial hygienist.

The ventilating engineer will find all the problems of controlling industrial atmospheric pollution, air conditioning, and the relations of these problems to the health, comfort and productivity of the industrial worker discussed exhaustively by Dr. N. Witheridge, while the section on analysis of air contaminants contains highly technical descriptions of modern methods and instruments of analysis, accompanied by encyclopædic tables of permissible air pollutions, maximum ignition and explosive temperatures and the like.

The chapter on Radiant Energy and Radium, by Dr. Leon Curtiss, gives not only the fundamental concepts of injury by radiation, but also detailed information on measurement of exposure and on devices and materials for protection, including a most valuable table of safe distances and lead equivalents.

"Occupational Dermatoses," by Dr. Louis Schwartz, contains an alphabetical classification of skin hazards according to types of workers or occupations, which will form a useful reference for the industrial dermatologist.

The book claims only "to present industrial hygiene and toxicology in simple understandable terms in sufficient detail to be of some use to all persons interested in safeguarding the health and welfare of working people and in improving the working environment." This is a very modest estimate of its actual achievement.

ETHEL BROWNING.

Das biologische Weltbild. Band I: Die Stellung des Lebens in Natur und Wissenschaft. By Dr. LUDWIG VON BERTALANFFY. [Pp. 202.] (Bern: A. Francke AG. Verlag, 1949. Paper, Swiss Frs. 11.–; cloth, Swiss Frs. 14.50.)

DR. BERTALANFFY who, we are glad to see, is now back in Vienna as Professor, is well known as one of the foremost champions of the holistic or organismal

standpoint in biology, which he first expounded with great skill in his *Theoretische Biologie* (Vol. I, 1932). The present volume is of more general scope, though the basic idea of the organism as a dynamic system of activities remains the same. In the organismal view, the living thing is not machine-like, it is not a mere sum of separable parts, but an integrated unity of inter-dependent functional activities, essentially an active, "open" and autonomous system. This conception is greatly superior to the older mechanistic view, with or without the addition of a vitalistic factor, and it has gained many adherents of recent years. Bertalanffy develops this modern view in a very thorough and logical manner, taking full account of the developments in physical concepts which have revolutionised our concept of matter, and demonstrating the applicability of the holistic-dynamical view in all realms from the microphysical to the psychological. The book is not easy reading, but no serious student of theoretical biology, or, for that matter, of the philosophy of science, can afford to neglect it. His treatment of the mechanism-vitalism problem is brilliant, so also is his discussion of the gene theory and its bearing on the theory of evolution. He distinguishes clearly between science and metaphysics; his organismal view, which treats the living thing as essentially a physical system, though with laws of its own, is a scientific view which does not pretend to give an adequate picture of reality, and he is fully aware of this. It seems to us that he has not taken sufficient account of the efficacy of the psychological element in life, so clearly shown in animal behaviour, and we look forward to what he has to say in Vol. II on the relation of the physical to the psychical.

E. S. RUSSELL.

The Stuff We're Made Of. By W. O. KERMACK, M.A., D.Sc., LL.D., F.R.S., and P. EGGLETON, D.Sc., F.R.S.E. Second edition. [Pp. viii + 356, with 8 plates and 75 figures.] (London: Edward Arnold & Co., 1948. 10*s*. 6*d*. net.)

ONE of the most ticklish expository jobs in the world is that of trying to explain scientific matters to a non-scientific audience with maximum lucidity and no loss of accuracy. It is difficult enough to do this in a lecture: it is much more difficult by the written word, for this involves an additional problem of holding and maintaining attention. This is much aided by the possession of a simple, accurate and therefore unambiguous prose style. Let us see how our two authors are equipped for what they have set themselves to do, which is to describe to the non-biochemist the chemical compounds that make up the animal and vegetable body and the kinds of chemical reaction in which these compounds are known to indulge—habitually, that is to say, in health, and accidentally, that is to say, in disease.

It is interesting to note that neither author is, in fact, strictly to be classified as a biochemist, though both are well known for their extensive experience of biochemical problems. Anyhow, the holder of a chair in biological chemistry and a lecturer in biochemistry attached to a department of physiology, both very much concerned with the teaching of medical students, surely ought to be a strong combination. Moreover their teaching experience is a real asset, if only for what it must have shown them about the heights and depths of the human intellect.

It may therefore be said at once that Prof. Kermack and Dr. Eggleton

have the facts at their finger-tips. They are sticklers for correct statements of those facts, though they are clearly aware—sometimes almost uncomfortably aware, it seems to me—that the essential unpalatability of the precisely circumscribed statement needs often to be flavoured with the saucy metaphor or simile. Some of theirs are indeed distinctly piquant. Their chapter headings give an indication of this, for we read of "Atomic Bricks and Mortar" (IV), "Not by Bread Alone" (XI) and "The Busy B's" (XIII). The subject-matter of those chapters is perhaps easy to guess, as is that of "The Vitamin Racket" (XII)—a little unfair, perhaps, for proteins and hormones have their stuntsmen as well—and of the more staidly named "On Growing Up" (XV), "Sex and Reproduction" (XIV) and "Giant Molecules" (VI). Less obvious, by far, will it be to the page-riffler what exactly the authors are after when they are writing about "Stepping-Stones" (VII)—an ingenious expansion of a somewhat novel idea to represent catalysis—or "An Unsolved Problem" (X), which is concerned with the basic energetics of muscular action, or "No-Man's Land" (XVIII), though the biochemist will probably spot this as concerned with viruses and chromosomes. But the page-riffler will have to read to find out.

For its subject matter and method of exposition, this book can comfortably be read by chemists of almost every known sub-species, but one wonders how even the most intelligent of intelligent laymen will cope with some of the less straightforward parts. In his attempts to do so he will, moreover, be but little assisted by the authors' mode of writing. This contains some very bad examples of mishandled words, phrases and sentences and forms an unfortunate and depressing contrast with the high aims and many virtues of the book.

Words like "definite," "actually" and "quite" could as a rule have been omitted altogether: phrases like "in the case of," "with regard to," "as far as . . . is concerned" generally need replacing by a simple preposition. And why "commence" what can be simply begun? If this book is to become as attractive and useful as its subject-matter and its authors' justifiable reputation warrant our hoping, its writing must receive successive treatments with a toothcomb and a garden-roller.

At the same time it will be necessary to remove certain errors of typography ("can" for "cane" on p. 88; riboflavin and riboflavine used alternatively on p. 117; "is it" for "it is" on p. 206; "depends" for "depend" on p. 292, among others) and of statement. Thus the formula for vitamin A (p. 189) is described as that of carotene. The 20 grams of protein lost by the "average-sized adult" daily (p. 185) are assuredly not to make up for "ordinary wear and tear" as usually understood, for they are independent of the amount of muscular work done: this method of expression obscures one of the most fascinating of the unsolved problems in animal metabolism. Nor is the view any longer accepted that iron by mouth can only be utilised if taken in the simple inorganic form (p. 178) or that methylnaphthaquinone given orally requires bile salts to promote its proper absorption (p. 203). It is not ergosterol that turns into vitamin D in the animal body, as is implied at the top of p. 195, and the Ascheim-Zondek test is carried out neither on rabbits nor on guinea-pigs (p. 240).

But enough of comment and criticism. The second edition of this book records enormous scientific advances that have occurred since the first edition appeared in 1938. We may hope that a third edition will be needed and published soon and that in it the authors' powers of pleasing exposition will

have become commensurate with their grasp of the subject, for this is indeed wide and deep to a remarkable extent.

A. L. BACHARACH.

Vitamins and Hormones. Advances in Research and Applications. Edited by ROBERT S. HARRIS and KENNETH V. THIMANN. Vol. V, 1947: [Pp. xvi + 478, with 74 figures.] 41*s*. 6*d*. net. Vol. VI, 1948: [Pp. xii + 435, with Cumulative Subject Index of Vols. I to V.] 43*s*. net. (New York: Academic Press, Inc.)

EACH year these now permanently established annuals leave us more and more in debt to editors and contributors, but with less and less to say by way of acknowledging our indebtedness. This is even greater than it might have been, because the two collective indexes to authors and titles of all published articles that appeared at the end of Vol. V have now been supplemented and extended by the inclusion in Vol. VI of a complete cumulative index to all the contents of the first five volumes. Thus what was already a valuable collection of authoritative monographs has by a single act also become a complete reference book for a quinquennium!

The subjects chosen by the editors and expounded by the authors in Vols. V and VI show the same catholicity in selection of topic and writer that we have come to expect in *Vitamins and Hormones*. The subjects are predominantly biochemical with clinical and physiological aspects making neck-to-neck seconds. The 30 authors are mainly American, but they include 3 from England, one from Switzerland and one from Denmark. Of the 19 separate monographs, 12 come from research or teaching departments in universities or medical schools, 4 from State or other specialist research institutions and 3 from the laboratories of industrial companies or consultants.

Of specially topical interest are the contributions on the pteroylglutamates (Vol. V, 38 pages), by Prof. William Darby, and on their chemical and biological action (Vol. VI, 27 pages) by Drs. B. L. Hutchings and J. H. Mowat. So burning were the questions involved that they have been given detailed examination in two successive volumes, a practice very rarely adopted by the editors. Even so, much of what they have written needs revaluation in light of recent discoveries and the isolation and properties of the "anti-pernicious anæmia factor," or vitamin B_{12}, for this certainly has some fundamental biochemical connection with the physiological part played by the pteroylglutamates.

There is, perhaps, less excuse for the need to revise some other statements; thus it is a little surprising that Drs. Hans Molitor and Gladys Evans, writing on "Vitamins as Pharmacological Agents" (Vol. VI, 33 pages), presumably no earlier than 1947, should omit all references to massive calciferol therapy in the treatment of lupus, to say nothing of its use in parathyroprivea. In all modesty, too, I would urge both on them and on Dr. H. M. Sinclair to revalue the evidence about the effect of tocopherol for the treatment of human habitual abortion, in the light of a statistical analysis made nine years ago and never once challenged since.

Three rather unusual contributions record published and unpublished work on the vitamin requirements of chicks (Vol. V, 12 pages) and mice (Vol. V, 23 pages) and the nutritional requirements of cotton rats and hamsters (Vol. VI, 14 pages), communicated to the symposium on "Vitamin

Requirements of Laboratory Animals" organised by the American Chemical Society at Chicago in September 1946, and reflect the continued interest of experimental nutritionists in devising "synthetic" diets adequate to the needs of their most important test objects.

Among more strictly chemical chapters is that on the synthesis of vitamin A (Vol. V, 39 pages), whereas a marked clinical emphasis is to be found in the contributions on the use of androgens in women (Vol. V, 76 pages) and of testosterone in the male (Vol. V, 81 pages). Botanists are not overlooked, for in Vol. VI there is an article on the distribution of vitamins in micro-organisms (61 pages) and on the B vitamins as plant hormones (53 pages).

The endocrinologist takes the forefront of the stage when Prof. E. C. Kendall (Vol. VI, 51 pages) discusses the relation between adrenal cortical hormones and water metabolism and also in the chapter on the biology of anti-thyroid agents (Vol. V, 44 pages) by Drs. H. A. Charipper and A. S. Gordon. Dr. C. H. Li and Prof. H. M. Evans write with expected authority on the pituitary growth and adrenocorticotrophic hormones (Vol. V, 35 pages) and in the hormone group comes also one of the British contributions, by Drs. C. W. Emmens and A. S. Parkes, on the effect of exogenous œstrogens on the male mammal (Vol. V, 41 pages).

Another British contribution, of special concern to the nutritionist, is Dr. H. M. Sinclair's characteristically erudite, critical and sometimes mordant review of "The Assessment of Human Nutriture." He sticks to his word, and justifies it by logic and, I believe, etymology, but some of us still find it hard to digest.

Finally, this long list, to be complete, must record a valuable publication from Copenhagen by Prof. H. Dam on vitamin K (Vol. VI, 27 pages) of which no one in the world is better qualified to speak, a Swiss contribution by Dr. A. von Muralt, on vitamin B_1 and the physiology of peripheral nerves (Vol. V, 26 pages) and a very general and most informative survey of vitamin availability in man and other animals by Drs. D. Melnick and B. L. Oser (Vol. V, 54 pages).

Indeed, within the wide field chosen, there is something for almost everyone; it is all good and some of it is very good indeed.

A. L. BACHARACH.

Recent Progress in Hormone Research. Vol. III. Edited by GREGORY PINCUS. [Pp. viii + 378, with numerous figures and tables.] (New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1948. 43*s*. net.)

THE volumes recording the proceedings of the Laurentian Hormone Conference, of which this is the third, are now well-established and valuable keys to current endocrine literature in specialised fields.

This volume, like the previous ones, is divided into a number of sections dealing with different aspects of the subject, each main contribution being followed by a summary of the discussion. The Editor, Dr. Gregory Pincus, acknowledges the co-operation of the various speakers at the informal discussion which followed each paper in the editing of the discussions. It can certainly be said that these discussions form an extremely valuable aspect of the published record of the Conference for those who were unable to be present.

The sections and titles of the individual contributions were as follows:

Hormones, by C. H. Li and H. M. Evans; Chapter 15—Hormonal Control of Mammary Growth, by S. J. Folley and F. H. Malpress; Chapter 16—Hormonal Control of Lactation, by S. J. Folley and F. H. Malpress.

Collaborators have been chosen from outside the United States and Canada as well as from within, as shown by the presence of chapters by Drs. S. J. Folley and F. H. Malpress.

In the reviewer's opinion, this work constitutes an eminently suitable book for the advanced student or the research worker who is not specialised in the subjects with which it deals. It is inevitable, of course, that the time taken in publication of such a book renders some part of it out of date, but on the whole few serious omissions or mistakes have been noted in the chapters read by the reviewer. The Editors are to be congratulated on covering a difficult and somewhat diffuse field in an effective and lucid fashion, and this book is recommended to those who wish to gain some general knowledge of the subject of endocrinology.

F. G. Y.

Bacterial Metabolism. By MARJORY STEPHENSON, Sc.D., F.R.S. Third edition. [Pp. xiv + 398, with 1 plate and 81 figures.] (London, New York, Toronto: Longmans, Green & Co., 1949. 30*s*. net.)

IN the preface to this third edition of her book *Bacterial Metabolism*, the late Dr. Marjory Stephenson points out that the subject has now developed to an extent which cannot conveniently be covered by one author. She announces that this is the last edition which she would produce alone. It would appear that, courageously, she looked forward to another edition in collaboration. This was not to be and we must take this as the final product of a remarkable career.

The general lay-out of this third edition is unchanged, but every chapter has been rewritten to a greater or less extent and altered in accordance with the great advances of the last ten years. The first major change is the alteration of Chapter 5 from "Decomposition of Proteins" to "Nitrogen Metabolism," which allows consideration of synthetic processes, decarboxylation, transamination and assimilation of amino acids by gram-positive bacteria. The 17-page table on the alleged actions of various bacteria on amino acids has been dropped.

The chapter on Nucleic Acid has also been extended to include the Feulgen reaction, the chemistry of the ribonucleic acids, the relationship of the desoxy type to genetic structures, the association of these substances with protein synthesis in general and with the gram staining complex. A new plate shows variations in the nucleic acid content of bacteria during the growth cycle.

The chapter on Growth and Nutrition has also been largely rewritten to include the mass of quantitative data on growth phenomena which have accumulated in recent years. The section on nutrition, so trifling ten years ago, has now assumed important proportions, with consideration of all the vitamins and their bearing upon the elucidation of synthetic and other processes.

It may be thought that there is too much suggestion of association of growth factors with particular species. Another long table (8½ pages) showing the ability of various substances to act as sole carbon source has been retained in this edition and might be eliminated without serious loss.

The chapter on Enzyme Variation and Adaptation has been enlarged to

include "spontaneous" changes due to normal genetic accidents and genetic alterations induced by, for instance, X-rays. There is also consideration of the newer work on the temporary adaptations of enzymes which may be induced by changes in the environment.

When a further edition is called for, and if economy is still a potent consideration, a few pages could be saved by omitting the Appendix.

One advantage of the book is its suitability for a wide circle of readers. Although it includes, no doubt shortly, all biochemical matter about bacteria which an expert biochemist could use profitably, it is written in a form which is intelligible to, let us say, those "microbiologists" whose conversion to the chemical outlook was one of Dr. Stephenson's many interests.

PAUL FILDES.

Fundamental Principles of Bacteriology and **Laboratory Manual.** By A. J. SALLE, B.S., M.S., Ph.D. Third edition. [Pp. xii + 730, with 297 figures, including 1 coloured plate; pp. x + 176, with 4 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1948. 36*s.* net and 13*s.* 6*d.* net.)

BACTERIOLOGY as an independent science has only recently begun to flourish in Great Britain. Its development has been largely utilitarian, suited particularly to the needs of medicine, agriculture and various industries. Even when it is included as a subject in the science course, the teaching is often done in the department of medical bacteriology, where a bias towards the application of bacteriology to medicine is almost unavoidable. Now that the bacterial chemist is fast becoming an essential member of any team investigating a bacteriological problem, whether in medicine, agriculture or industry, serious consideration should be given by the science faculties of our universities to the inclusion of bacteriology as a principal subject in the science course.

As an indication of our backwardness, there is no British textbook on bacteriology designed for the science student and he has therefore to turn to America to fulfil this need. This book by Salle, now in its third edition, is a useful primer on the fundamental principles of bacteriology. It covers a wide field, dealing with such subjects as bacterial nutrition, metabolism and dissociation, the action of disinfectants, and enzymic activities on proteins and carbohydrates. In the later chapters on the more applied aspects of bacteriology, recent progress is well reviewed, although the English reader misses reference to the Bourdillon slit sampler in the bacteriological examination of air, to the Ministry of Health's memorandum on the bacteriological examination of water supplies, and to milk-borne Salmonella infection and the occurrence of Salmonellæ in dried egg powder. However, the review of the literature is remarkably catholic for an American book and there is a good bibliography with full titles of the relevant papers at the end of each chapter. The book is produced on good-quality glossy paper and is profusely illustrated with diagrams and photographs which are for the most part clear and helpful. It is accompanied by a practical laboratory manual.

R. C.

HISTORY OF SCIENCE

Scientists and Amateurs, A History of the Royal Society. By DOROTHY STIMSON. [Pp. xvi + 270, with 20 plates.] (London: Sigma Books, Ltd., 1949. 15*s*. net.)

THERE have been several books about the Royal Society written by historians among its officers, with special knowledge of its administration and affairs. Miss Stimson's book is of a different character. It is designed for the general reader, particularly in America, and is the outcome of a study of seventeenth-century science in England; it sets the origin and growth of the society against the intellectual and historic background of the seventeenth and later centuries.

The title is not a very good indication of the contents, and is somewhat misleading. It contrasts those amateurs, the *virtuosi*, who took such a large part in the early activities of the society, with the professional men of science who comprise the bulk of its membership to-day; but there is a sense in which the true man of science is and always will be an amateur, and it is certainly true that the great contributions to knowledge have by no means all been made by those who earn their living in a scientific vocation.

This is, however, a minor criticism. In fact, the book gives an admirably balanced and objective account of the beginnings and progress through the years of this oldest of scientific institutions to its present position of leadership and authority; and the study is all the more valuable because it is clearly the outcome of extensive reading and research.

Miss Stimson traces the movement towards the new learning, based not on written authority but on observation and experiment, which resulted in the foundation of the Royal Society by King Charles II, to its source in the writings of such men as Francis Bacon, of Comenius and Hartlib, of John Evelyn and Robert Boyle. There were the meetings at Gresham College in London and at the lodgings of the hospitable Dr. John Wilkins at Wadham College in Oxford, that "invisible college" to which Charles' foundation gave concrete form. In the early years the work of Christopher Wren, of Isaac Newton and others hardly less distinguished among its officers and fellows gave prestige to the society, despite criticisms of the trivial nature of its activities and of many of the papers it published. Its association with the newly founded Royal Observatory resulted from the King's interest in the progress of astronomy as an aid to navigation—a responsibility similar to that assumed by the society in more recent years, for another great public institution, the National Physical Laboratory.

The beginning of the nineteenth century was an unhappy period when there were gloomy predictions of the decline of science in England and much criticism of the management of the society and of the methods of admission to its fellowship. By the middle of the century, however, reforms had been introduced and it gradually assumed the position of unchallenged authority which it has ever since held. Miss Stimson adds to her account of these developments some interesting particulars of American connections and of prominent Americans among the fellows.

T. M.

CELLANEOUS

Sampling Methods for Censuses and Surveys. By FRANK YATES, Sc.D., F.R.S. [Pp. xiv + 318, with 10 figures.] London: Chas. Griffin & Co., Ltd., 1949. 24*s*. net.)

THE importance of investigation by sample is now widely recognised, owing to its rapidity and economy in comparison with complete surveys. This is mainly due to the development of national planning, to the need for rapid observation of the effects of regulations such as rationing, and to the programme of the United Nations Organisation for studying the resources of statistically undeveloped areas. Dr. Yates' book will be of great use to the numerous officials and investigators who are engaged in planning and carrying out any kind of enquiry by sample. He has had unique experience, as head of the statistical department at Rothamsted, as a member of the United Nations Sub-Commission on Statistical Sampling (at whose request the book was undertaken) and elsewhere.

In simple cases the broad rules that the population must be exactly defined, and that every member of it should have an equal chance of inclusion in the sample, are generally understood; it is rightly emphasised by the author that only when they are obeyed can a complete statement be made of the precision of the results. But especially in agriculture and forestry, to which Dr. Yates naturally devotes a great part of the book, the problem is complicated, and there are many different methods of approach. These are described with numerous worked examples, and their merits compared. The book is not a mathematical treatise. The earlier chapters are descriptive and numerical, using only very simple algebraic formulæ, and can be readily used by non-mathematical statisticians. The later chapters deal with the estimation, on more severely mathematical lines, of sampling errors and precision. "Mathematical proofs have not been included," and consequently there is no discussion of the logical and mathematical background on which the methods are based. Thus it is stated (p. 190) that the normal curve of error is widely prevalent, but the conditions under which this holds are not analysed. The comparison of standard deviations is almost the only method used for determining the significance of differences between samples. It is surprising that Pearson's χ^2 test is only mentioned in footnotes, though it is of great importance in practice. "Fiducial probability" (or belts of confidence) is treated very cursorily (p. 191). Inverse probability is not named, though ultimately it forms the basis of our judgment of the position of the "target" from examination of the shot-marks, as Edgeworth has put it. These exclusions are deliberate, and mathematical statisticians are already familiar with the problems involved. It is not clear, however, why Sheppard's corrections for the standard deviation, when computed from graded observations, is completely ignored.

A. L. BOWLEY.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

From Euclid to Eddington. A Study of Conceptions of the External World. By Sir Edmund Whittaker, F.R.S., Tarner Lecturer, and formerly Fellow, of Trinity College, Cambridge. Cambridge: at the University Press, 1949. (Pp. x + 212.) 15*s*. net.

Cardinal Algebras. By Alfred Tarski. With an Appendix, Cardinal Products of Isomorphism Types, by Bjarni Jónsson and Alfred Tarski. New York and London: Oxford University Press, 1949. (Pp. xii + 326.) 50*s*. net.

Introduction to Statistical Mechanics. By Ronald W. Gurney, Visiting Professor of Physics, Johns Hopkins University. International Series in Pure and Applied Physics. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1949. (Pp. viii + 268, with 59 figures and 17 tables.) 30*s*. net.

Weather Forecasting in Tropical Regions. By A. G. Forsdyke, Ph.D., D.I.C. Meteorological Office, Geophysical Memoirs No. 82. London: H.M. Stationery Office, 1949. (Pp. 47, with 12 figures.) 2*s*. 6*d*. net.

Wind at 100,000 ft. over South-East England. By R. J. Murgatroyd, B.Sc.(Eng.) and C. J. B. Clews, Ph.D. Meteorological Office, Geophysical Memoirs No. 83. London: H.M. Stationery Office, 1949. (Pp. 14, with 5 figures and 3 tables.) 6*d*. net.

Atmospheric Electricity during Disturbed Weather. By Sir George Simpson, K.C.B., F.R.S. Meteorological Office, Geophysical Memoirs No. 84. London: H.M. Stationery Office, 1949. (Pp. 51, with 18 plates, 8 figures and 7 tables.) 3*s*. 6*d*. net.

Foundations of Modern Physics. By Thomas B. Brown, Professor of Physics, The George Washington University. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1949. (Pp. xvi + 391, with frontispiece and 181 figures.) 30*s*. net.

Physics: Principles and Applications. By Henry Margenau, Professor of Natural Philosophy and Physics, William W. Watson, Professor of Physics, and Carol G. Montgomery, Associate Professor of Physics, Yale University. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1949. (Pp. x + 760, with 612 figures and 47 tables.) 30*s*. net.

Physics Tells Why. By Overton Luhr. Illustrated by Ruth C. Schmidt. London: George Allen & Unwin, Ltd., 1949. (Pp. x + 387, with numerous figures.) 16*s*. net.

The Structure of Matter. By Francis Owen Rice, Professor of Chemistry, The Catholic University of America, and Edward Teller, Professor of Physics, The University of Chicago. Structure of Matter Series. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1949. (Pp. xiv + 361, with 70 figures and 14 tables.) 30*s*. net.

Viscometry. By A. C. Merrington, B.Sc., Ph.D., F.Inst.P., Senior Scientific Officer, Ministry of Supply. London: Edward Arnold & Co., 1949. (Pp. viii + 142, with 60 figures.) 16*s*. net.

Electricity and Magnetism. By C. L. Reynolds, M.A., sometime Scholar of Clare College, Cambridge, now Headmaster of Nottingham High School. Second edition. London: G. Bell & Sons, Ltd., 1949. (Pp. x + 366, with 292 figures.) 7*s*. 6*d*.

The Fundamentals of Electro-Magnetism. By E. G. Cullwick, Professor of Electrical Engineering in the University of St. Andrews. Second edition. Cambridge: at the University Press, 1949. (Pp. xxvi + 327, with 139 figures and 3 plates.) 18*s*. net.

Computation Curves for Compressible Fluid Problems. By C. L. Dailey and F. C. Wood, Research Engineers, University of Southern California. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1949. (Pp. x + 33, with 31 figures.) 12*s*. net.

Gas Tables. Thermodynamic Properties of Air, Products of Combustion and Component Gases. Compressible Flow Functions. By Joseph H. Keenan, Professor, and Joseph Kaye, Assistant Professor of Mechanical Engineering, Massachusetts Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. x + 238, with 76 tables and 5 figures.) 30*s*. net.

The Physical Principles of Gas Liquefaction and Low Temperature Rectification. By Mansel Davies, M.Sc., Ph.D. London, New York, Toronto: Longmans, Green & Co., 1949. (Pp. viii + 205, with 88 figures and 4 folding charts.) 25*s*. net.

Scientific Foundations of Vacuum Technique. By Saul Dushman, Ph.D., Assistant Director, Research Laboratory, General Electric Company, Schenectady, N.Y. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1949. (Pp. xii + 882, with 326 figures and 220 tables.) 90*s*. net.

Lighting Technique. By Professor B. F. Fedorov. Translated from the Russian by W. R. Stoker, B.Sc.(Eng.), A.M.I.E.E. With additional sections on Fluorescent Lighting by W. T. O'Dea, B.Sc., A.M.I.E.E. London: Hutchinson's Scientific and Technical Publications, 1949. (Pp. 299, with 128 figures and 54 tables.) 25*s*. net.

Atomic Energy Year Book. Edited by John Tutin, D.Sc. London: Temple Press, Ltd., 1949. (Pp. xii + 237, with frontispiece and 23 figures.) 21*s*. net.

Electronic Time Measurements. Edited by Britton Chance, Assistant Professor of Biophysics, University of Pennsylvania, *et al.* Office of Scientific Research and Development, National Defense Research Committee. Massachusetts Institute of Technology Radiation Laboratory Series. New York, Toronto, London: McGraw-Hill Book Co., Inc., 1949. (Pp. xviii + 538, with 383 figures and 16 tables.) 42*s*. net.

Microwaves and Radar Electronics. By Ernest C. Pollard, Associate Professor of Physics, and Julian M. Sturtevant, Associate Professor of Chemistry, both at Yale University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. viii + 426, with 193 figures and 12 tables.) 30*s*. net.

Radio-Frequency Heating. By L. Hartshorn, D.Sc., A.M.I.E.E., A.R.C.S., D.I.C., of the National Physical Laboratory. London: George Allen & Unwin, Ltd., 1949. (Pp. 237, with 103 figures and 13 tables.) 21*s*. net.

Worked Examples in Electrical Engineering. By W. T. Pratt, B.Sc.(Eng.), A.C.G.I., D.I.C., A.M.I.E.E., Senior Assistant in Electrical Engineering

at Southall Technical College. London: Hutchinson's Scientific and Technical Publications, 1949. (Pp. 263, with 134 figures.) 25*s.* net.

An Introduction to the Gas Turbine. By D. G. Shepherd, B.Sc., A.M.I.Mech.E., Assistant Professor, Sibley School of Mechanical Engineering, Cornell University. London: Constable & Co., Ltd., 1949. (Pp. xii + 387, with 174 figures, including 6 plates.) 24*s.* net.

Basic Naval Architecture. By Kenneth C. Barnaby, O.B.E., B.Sc., A.C.G.I. London: Hutchinson's Scientific and Technical Publications, 1949. (Pp. 340, with 100 figures and 45 tables.) 42*s.* net.

Isotopic Carbon. By Melvin Calvin, Professor of Chemistry, Charles Heidelberger, James C. Reid, Bert M. Tolbert, and Peter F. Yankwich, Instructor in Chemistry, all members of the scientific staff of the Radiation Laboratory, University of California, Berkeley. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1949. (Pp. xiv + 376, with 107 figures and 39 tables.) 33*s.* net.

A Class Book of Physical Chemistry. By T. Martin Lowry, C.B.E., M.A., D.Sc., F.R.S., late Professor of Physical Chemistry in the University of Cambridge, and Samuel Sugden, D.Sc., A.R.C.Sc., F.R.S., Professor of Chemistry in the University of London (University College). Second edition. London: Macmillan & Co., Ltd., 1949. (Pp. x + 454, with 75 figures and 101 tables.) 8*s.* 6*d.* net.

Acids, Bases and Non-Aqueous Systems. By Ludwig F. Audrieth, Professor of Inorganic Chemistry, University of Illinois. Twenty-third Annual Priestley Lectures. Pennsylvania: Phi Lambda Upsilon, Dept. of Chemistry, State College, 1949. (Pp. viii + 66.) $2.00.

Absorption Spectrophotometry. By G. F. Lothian, M.A., F.Inst.P., Physics Lecturer, University College of the South West, Exeter. London: Hilger & Watts, Ltd., Hilger Division, 1949. (Pp. 196, with 71 figures, including 6 plates, and 11 tables.) 26*s.* net.

Vacuum Manipulation of Volatile Compounds. By Robert Thomas Sanderson. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. viii + 162, with 40 figures and 17 tables.) 18*s.* net.

A New Notation and Enumeration System for Organic Compounds. By G. Malcolm Dyson, M.A., D.Sc., Ph.D., F.R.I.C., Technical Director, British Chemicals and Biologicals, Ltd., Loughborough. Second edition. London, New York, Toronto: Longmans, Green & Co., 1949. (Pp. x + 138.) 10*s.* 6*d.* net.

The Terpenes. Vol. II: The Dicyclic Terpenes and their Derivatives. By J. L. Simonsen. Second edition revised by Sir John Simonsen, D.Sc., F.R.I.C., F.R.S., Director of Research, Colonial Products Research Council, and L. N. Owen, B.Sc., Ph.D., F.R.I.C., Lecturer in Organic Chemistry, Imperial College of Science and Technology. Cambridge: at the University Press, 1949. (Pp. xii + 619.) 35*s.* net.

Organic Syntheses, Vol. 28. H. R. Snyder, Editor-in-Chief. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1948. (Pp. vi + 121.) 15*s.* net.

Monomers. A collection of data and procedures on the basic materials for the synthesis of fibers, plastics, and rubbers. Edited by E. R. Blout, Chemical Research Laboratory, Polaroid Corporation, Cambridge, Mass., W. P. Hohenstein and H. Mark, Institute of Polymer Research, Polytechnic Institute of Brooklyn, Brooklyn, N.Y. New York and London: Interscience Publishers, Inc., 1949. (Pp. 374, with numerous figures and tables.) 45*s.* net.

Elastomers and Plastomers: Their Chemistry, Physics and Technology. Vol. II: Manufacture, Properties, and Applications. Edited by R. Houwink, External Lecturer in the Technical University at Delft. Elsevier's Polymer Series. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1949. (Pp. xvi + 515, with 225 figures and 122 tables.) 50*s*. net.

Organic Coatings in Theory and Practice. By A. V. Blom, Ph.D., Consulting Chemist, formerly Head of the Paint and Varnish Research Station of the Swiss Institute for Testing Materials. Elsevier's Polymer Series. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1949. (Pp. xii + 298, with 121 figures and 82 tables.) 32*s*. net.

Synthetic Perfumes. Their Chemistry and Preparation. By T. F. West, D.Sc., Ph.D., A.M.I.Chem.E., F.R.I.C., Director, Developmental Division, Drug Houses of Australia, Ltd., H. J. Strausz, Dr. Phil., M.A., F.R.I.C., Consulting Chemist, and D. H. R. Barton, Ph.D., D.I.C., F.R.I.C., Imperial Chemical Industries Research Fellow, Imperial College of Science and Technology. London: Edward Arnold & Co., 1949. (Pp. viii + 380.) 70*s*. net.

Recent Advances in Analytical Chemistry. Edited by R. E. Burk, Plastics Department, E. I. du Pont de Nemours & Co., Wilmington, and Oliver Grummitt, Morley Chemical Laboratory, Western Reserve University, Cleveland. Frontiers in Chemistry, Vol. VII. New York and London: Interscience Publishers, Inc., 1949. (Pp. xii + 209, with 93 figures and 10 tables.) 27*s*. net.

Physics and Chemistry of Cellulose Fibres, with particular Reference to Rayon. By P. H. Hermans, Director of the Institute for Cellulose Research of the A.K.U. and Affiliated Companies, Utrecht. Elsevier's Polymer Series. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1949. (Pp. xxii + 534, with 225 figures and 58 tables.) 50*s*. net.

Petroleum Production. Vol. V: Oil Production by Gas and Flooding. By Park J. Jones, Consultant, Houston, Texas. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1948. (Pp. viii + 274, with 117 figures and 62 tables.) 36*s*. net.

A Dictionary of Metallography. By R. T. Rolfe, F.R.I.C., F.I.M., Chief Metallurgist, Messrs. W. H. Allen, Sons & Co., Ltd., Bedford. Second edition. London: Chapman & Hall, Ltd., 1949. (Pp. xii + 287.) 18*s*. net.

Treatise on Powder Metallurgy. Vol. I: Technology of Metal Powders and Their Products. By Claus G. Goetzel, Ph.D., Vice-President and Director of Research, Sintercast Corporation of America, New York. New York and London: Interscience Publishers, Inc., 1949. (Pp. xxviii + 778, with 300 figures and 82 tables.) 90*s*. net.

The Practice of Research in the Chemical Industries. By R. H. Griffith, Senior Research Chemist, North Thames Gas Board. London: Oxford University Press, 1949. (Pp. viii + 184.) 12*s*. 6*d*. net.

The Geology of Water Supply. By Sir Cyril S. Fox, D.Sc., M.I.Min.E., F.G.S. London: The Technical Press, Ltd., 1949. (Pp. x + 209, with 15 plates and 49 figures.) 25*s*. net.

The Geology of the British Empire. By F. R. C. Reed, M.A., Sc.D., F.G.S., F.R.G.S. Second edition. London: Edward Arnold & Co., 1949. (Pp. x + 764, with 26 figures, including 15 folding maps.) 70*s*. net.

The History and Social Influence of the Potato. By Redcliffe N. Salaman, M.D., F.R.S., late Director of the Potato Virus Research Station, Cambridge. With a Chapter on Industrial Uses, by W. G. Burton, Low Temperature Research Station, Cambridge. Cambridge: at the University Press, 1949. (Pp. xxiv + 685, with frontispiece and 108 figures, including 34 plates.) 50*s.* net.

The Periodic Partial Failures of American Cottons; Their Causes and Remedies. By R. H. Dastur, Plant Physiologist, Cotton Physiological Research Scheme, Indian Central Cotton Committee. Second edition. Bombay: Indian Central Cotton Committee, 1949. (Pp. xii + 172, with 30 figures and 84 tables.) Rs. 6.–.

Sampling Methods for Censuses and Surveys. By Frank Yates, Sc.D., F.R.S., Head of the Department of Statistics, Rothamsted Experimental Station. London: Charles Griffin & Co., Ltd., 1949. (Pp. xiv + 318, with 10 figures and 68 tables.) 24*s.* net.

Vitamins and Hormones, Vol. VI. Edited by Robert S. Harris, Professor of Biochemistry, Massachusetts Institute of Technology, and Kenneth V. Thimann, Associate Professor of Plant Physiology, Harvard University. New York: Academic Press, Inc.; London: H. K. Lewis & Co., Ltd., 1948. (Pp. xii + 435, with 3 figures and 43 tables.) 43*s.* net.

Biochemical Preparations. Vol. 1. Herbert E. Carter, Editor-in-Chief. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1949. (Pp. xii + 76.) 15*s.* net.

Hæmoglobin. A Symposium based on a Conference held at Cambridge in June 1948 in memory of Sir Joseph Barcroft. Editors, F. J. W. Roughton and J. C. Kendrew. London: Butterworths Scientific Publications, 1949. (Pp. xii + 317, with frontispiece and numerous other illustrations.) 40*s.* net.

Annual Review of Physiology, Vol. XI. Victor E. Hall, Editor, Jefferson M. Crimson and Arthur C. Giese, Associate Editors, all of Stanford University. Stanford, California: Annual Reviews, Inc. and the American Physiological Society, 1949. (Pp. x + 643.) $6.00.

Conditioned Reflexes and Neuron Organization. By Jerzy Konorski, Head of the Department of Neurophysiology in the Nencki Institute of Experimental Biology and Professor in the University of Lodz. Translated by Stephen Garry. Cambridge: at the University Press, 1948. (Pp. xiv + 267, with 18 figures.) 18*s.* net.

The Principles of Scientific Research. By Paul Freedman, B.Sc., M.I.E.E., F.I.E.S., Head of Lamp Research, Messrs. Crompton Parkinson, Ltd. London: Macdonald & Co. (Publishers), Ltd., 1949. (Pp. 222, with 11 figures.) 15*s.* net.

The Freedom of Necessity. By J. D. Bernal, M.A., F.R.S., Birkbeck College, University of London. London: Routledge & Kegan Paul, Ltd., 1949. (Pp. xii + 437.) 18*s.* net.

Science, Servant of Man. By I. Bernard Cohen, Instructor in the History of Science and General Education, Harvard University. London: Sigma Books, Ltd., 1949. (Pp. xvi + 362, with 7 plates.) 15*s.* net.

Russian Science Readings (Chemistry, Physics, Biology). By L. Light, Ph.D., A.R.I.C. London and Philadelphia: Hirschfeld Bros., Ltd., 1949. (Pp. 98.) 7*s.* 6*d.* net.

Shaw on Vivisection. Compiled and edited by G. H. Bowker for the National Anti-Vivisection Society. London: George Allen & Unwin, Ltd., 1949. (Pp. 65, with frontispiece.) 5*s.* net.

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